Review of egusphere-2024-665

General comments:

The manuscript "Viscoelastic mechanics of tidally induced lake drainage in the Amery grounding zone" by Zhang et al. explains the physics that drive a series of supraglacial lake drainage events in Antarctica with numerical models. Remote sensing data suggest that the extensional stress regime of the background ice flow is not enough to trigger the lake drainage events. They conduct a series of targeted numerical experiments to show that tidal flexure provides the necessary extensional stresses to drive hydrofracturing, depending on the depth of the supraglacial lake. This essentially confirms the hypothesis in the observational study, Trusel et al. (2022), that detailed these drainage events. While I have some comments primarily related to clarification and discussion, my judgment is that this would be an excellent contribution to TC.

Specific comments:

- In the last line of the abstract and conclusions you mention calving. While I understand that supraglacial lakes might play a role in ice-shelf breakup (Banwell et al., 2019), I was not exactly sure how the results in this study related to the calving front. Clearly there are similar physics because you are modelling fracture and flexure, but additional clarification would be helpful if you want to include this statement.
- Introduction: Maxwell time of "approximately 9 hours in our estimation", this needs some more context for how you calculated this or a reference.
- Section 2.1, last paragraph, "In B" should be "In Appendix B"?
- When you introduce the upper-convected derivative, you should add a reference and probably provide some motivation (i.e. objectivity). The following review is excellent:

  Snoeijer J. H., Pandey A., Herrada M. A. and Eggers J. (2020). The relationship between viscoelasticity and elasticity. Proc. R. Soc. A. 47620200419
- Regularized flow law: It is good that you explicitly discuss the regularization because this is often not the case. I think it might be worth noting, with proper references, that the ice viscosity can vary over several orders of magnitude, and that the upper bound you have set seems to be on the lower end of the spectrum? Also suggest adding a statement here that you later test sensitivity to the Maxwell time.
- Comment somewhere on how the modelled grounding zone widths compare to width estimated from interferometry (Chen et al., 2023)?
- After equation (18), "…variational formulation weakly converges…". To my knowledge, this is less obvious for the UCM model because it cannot be cast directly as a minimization problem like those dealt with in Kikuchi & Oden (1988). Nevertheless, approximating the contact conditions by $\sigma_e = \max\left(0, \sigma_e + \frac{1}{\epsilon}\, u_n\right) \approx \max\left(0, \frac{1}{\epsilon}\, u_n\right)$ for small ϵ (where $\sigma_e = \sigma_n - p_w$) still makes sense for UCM and motivates the use of a penalty term, as long as there aren't singularities in σ_e.
- Last paragraph of section 2.5, "In A" should be "In Appendix A"?
- Section 2.6 seems kind of random at first glance and needs more context… e.g., say what are you going to do later with the lake depths? Also, it might be better to place this after Section 2.1 rather than after the modelling material.

- Table 1: Units on viscosity and friction regularization parameters?
- Section 3.1: Specify that σ– A relationship is for $\sigma_{xx,max}$.
- Figures 5 and 6 needs to label panels (a) and (b)
- Figure 5: clarify why the dashed lines go into the positive region? I thought they were compressive at high tide so not contributing to fracture, and I became *confused*.
- Section 3.2: Not many details are provided about the weight function method. I presume that you are doing something with $\sigma_{xx,max}$ but some more context would be helpful.
- I don't think you say what is the value of fracture toughness K_C ?
- Section 3.3: All of this is in terms of the stress intensity factor, but I was wondering what are the stress thresholds associated with fracture propagation so that you can relate these to the background extensional stress (<40 kPa), which you say is not enough to cause fracture propagation on its own? Something to verify this claim would be good.
- Section 3.3: "Supraglacial lakes would not be able to form under such large tidal stress." Does this limit correspond to the zero water depth in Figure 6 (state if so)? This is also an interesting point that you could revisit in the discussion.
- Section 4: You are using present tense "We use" / "We construct" but you have already done these things at this point in the paper so maybe "We used" or "We have used"?
- Section 4.1: Change "Ice Maxwell time" to "The Maxwell time of ice"?
- Section 4.1: I thought λ was the Maxwell time but here you are using τ, which was already used for deviatoric stress?
- Section 4.1: "may be possible to infer ice mechanical properties from observations on the range of GL migration". This is a really interesting idea that could be discussed a little more, e.g., reference back to grounding zone width ranges from interferometry?
- Section 4.3: "is important to incorporate the subglacial hydrology…". I felt like this should have some references. Also in previous paragraph, should include a ref. for "damage".
- Section 4.3: "the supraglacial lake can induce additional stress in the surrounding ice". In Section 3, you neglect the influence of the lake, but here you say that it may be important. Maybe say in Section 3 that "we later revisit this assumption…" or similar?
- Sensitivity tests: Does it matter that you are varying the elastic modulus to change the Maxwell time instead of the viscosity? In other words, would you get a different result if you instead varied the viscosity at fixed shear modulus?
- Discussion: General comment. I felt like some of the implications of the results could be discussed more, as noted in a couple instances above. As it stands the discussion is sort of technical with the focus on sensitivity tests. The sensitivity tests provide valuable information but I think they could be placed in a broader context – e.g., how do shear modulus, bedslope at the GL, or grounding zone width vary in Antarctica, and what implications does this have in light of the tests? For example, it seems from Figure 8b that lower bedslopes have lower stresses and therefore less likely for lake drainage, but this isn't discussed? Does having a wider grounding zone at lower bedslope have any influence on the lakes? Also, are there any broader implications with respect to rifting/stability/vulnerability of ice shelves, given that you talk about these things in the introduction? These are just examples that could lead to a more stimulating discussion.