Response to RC2: 'Comments to "A leading-order viscoelastic model for crevasse propagation and calving in ice shelves" by Zarrinderakht et al.

February 26, 2024

Reviewer Comment: In this paper, the authors coupled a boundary element method with the viscous ice-flow model, in order to combine the cracks propagation process with the viscous ice-flow model. The authors improved previous elastic models by using the real geometry at the time of crack propagation in their calculation. This work is potentially valuable to the cryosphere community, where the fracture and calving models are poorly developed.

However, I find the manuscript is hard to follow. This is partly because it's heavily citing other papers, some hasn't been published (Zarrinderakht et al., submitted), and some are not well known in glaciology. Furthermore, some of the key reference, which is used to describe the numerical solution, is wrongly cited. I hope the authors could improve the writing by being accu- rate, and bearing in mind that fracture mechanics is not widely implemented in ice-flow models, and some concepts are not well known (not as good as Stokes equations, for example). For example, when introducing equations, not only cite the original publication but also put the essential equations in the paper; also describe the physical meaning of the variables and equations in more details. I suggest a major revision to this manuscript. I hope the authors can put some effort in the writting style. There are some specific examples in the following comments.

Response: In our defense, we checked the model definition in sections 2.1–2.4, and could not find any undefined quantities. There is one key spot in which we rely on citation to prior work (Zarrinderakht et al, 2022), namely when we decompose total Cauchy stress into a viscous pre-stress and an elastic additional stress in equation (18). We decided it was not a good use of journal space to rederive that explicitly, since the place to find it is clearly identified (appendix A of Zarrinderakht et al, 2022). We made some changes to section 2.4, for instance to front-load the definition of the static stress intensity factor, which hopefully helps smooth the crack propagation description. We would note, however, that linear elastic fracture mechanics is not that poorly known in glaciology, there being eight papers in the pertinent literature cited in the introduction, and it is likely there are others.

As per the other referee's suggestion, we have also compacted the specification of the lateral stretching rate (previously in section 2.6) with the description of the lateral boundary conditions in section 2.1. That hopefully also makes the text more readable.

Reviewer comment: Abstract: The authors mention they solved the fracture mechanics problem on the actual domain geometry. I think here actual domain geometry here doesn't mean real glacier/ice shelf, but solve the cracks boundary. This is slightly misleading. Nevertheless, can we use observational datasets to validate the model?

Response: The actual domain geometry is the domain geometry predicted by the viscous flow

solver, the point being that other attempts to solve similar coupled viscous-elastic models have typically stopped short of solving the linear elastic fracture mechanics component properly (in the sense that they have typically used a simplified, rectangular proxy domain). This is expanded upon in section 3.5.

Can the theory be tested with actual data? In principle, of yes. At face value, creating the relevant data set looks like a generational commitment: for the cases where calving does involve the interplay between viscous flow and episodic fracture propagation as described in section 3.4, typical times to full fracture penetration from an initially intact ice shelf typically take a few decades, if we assume an initial ice thickness around 500 m and ice shelf flow that is not strongly buttressed. Ideally, you would want to scan the geometry of a particular piece of ice regularly over that length of time as it transits an ice shelf, while also monitoring average strain rates. The technology undoubtedly exists (geometry can presumably be scanned from UAVs under the ice, and by optical satellite for the surface, though seismic measurements are likely necessary to determine the location of crevasse tips). It's less likely that the funding would be available.

How much can be done using existing satellite data (especially given that basal crevasses are a key part of the picture) is less clear — and unfortunately lies outside our area of expertise. We would note however that we have not seen many process-scale remote sensing papers on calving, presumably because much of the relevant crack propagation is hidden from view. (There is a relatively recent piece by Joughin et al on calving at Sermeq Kujalleq that is an outlier in this regard, but the observations there pertain to grounded ice, so the model would need to be rewritten).

Reviewer comment: The key novelty of this work is the implementation of the boundary element method. A general description of boundary element method and why it's a good solution for the crack propagation problem (advantage) should be necessary?

Response: We're not quite sure what "advantage" refers to — the existing glaciological literature contains few if any examples of linear elastic fracture mechanics problems being solved numerically, the dominant method apparently being the use of canned kernel functions from Tada et al's (2000) *The Stress Analysis of Cracks Handbook*, while "advantage" suggests comparison with other numerical techniques. The point is that Tada's handbook doesn't have a recipe for arbitrary geometries.

As for describing the method, we have already done so in Zarrinderakht et al (2020), also published in *The Cryosphere*. It would seem a poor use of journal space to repeat that description. We have updated the text in the third paragraph of section 2.5, to reference that description, and point out the reasons for using a boundary element method (these being the ability to deal with arbitrary domain shapes and a small number of degrees of freedom — the BEM code runs rapidly on a single processor, which cannot be said for the FEM code for the Stokes flow problem)

To do so, we solve the linear elastic fracture dynamics component of the model using the boundary element method described in Zarrinderakht et al (2022, appendix B). This method is well-suited to computing stress intensity factors for cracks in domains of arbitrary geometry while using only a small number of numerical degrees of freedom, and is easily adapted to changing crack geometries (since only the domain boundary needs to be discretized, avoiding the need for remeshing in two dimensions) (see also Crouch and Starfield, 1983).

Reviewer comment: L31: Unit of extensional stress is missing

Response: Indeed. Our apologies. We have added "Pa" to the numerical figure.

Reviewer comment: L110-L114: Give the physical description of equations (5a).

Response: We have expanded the relevant passage to say the following

$$either \quad -[v_i n_i]^+ > 0 \quad and \quad -\sigma_{ij} n_i n_j = p_{\rm f}, \tag{5a}$$

or
$$-[v_i n_i]^+_{-} = 0$$
, $[\sigma_{ij} n_j]^+_{-} = 0$, and $-\sigma_{ij} n_i n_j \ge p_f$, (5b)

where $[f]_{-}^{+} = f^{+} + f^{-}$ is the sum of the limiting values of the bracketed quantity, n_i^{\pm} being the outward-pointing normal to the side labeled '+' or '-'. Lack of a superscript indicates that the equation holds regardless of which side of the contact the limit is taken from. In addition,

$$(\delta_{ij} - n_i n_j) \sigma_{jk} n_k = 0, \tag{5c}$$

where δ_{ij} is the usual Kronecker delta. The conditions (5a) state that normal stress in the contact areas is still given by equation (4) when the surfaces are about to move apart, since the sum of the outward-pointing normal components of velocity $v_i^+n_i^+ + v_i^-n_i^-$ measures how fast the two sides of the contact area move towards each other. By contrast, if the surfaces are not moving apart as in condition (5b), normal stress is continuous across the interface, and compressive normal stress must equal or exceed the fluid pressure. The third condition (5c) imposes vanishing shear stress. as was done previously in Zarrinderakht et al (2022): in other words, the model ignores the possibility of ice-on-ice friction.

Reviewer comment: Equation (7), extra comma

Response There are two separate equalities on the same line, separated by a comma, and a trailing comma to link with the text that follows. That use of punctuation seems correct to us.

Reviewer comment: Citation of Figure 1 is missing. It should be somewhere in section 2.1. Furthermore, the first figure citation in the main text is Figure 5a, which is also unusual.

Response: We have added a reference to figure 1 at the end of the first sentence of section 2.1 ("(see figure 1)"). The reference to figure 5a was a legacy error, resulting from switching the orders of figures 2–4 and 5–7 during the writing process for the original submission, That reference (first para of section 2.4) should now be to figure 2a.

Reviewer comment: Equation (10), consider indicating hw and s in Figure 1 sketch.

Response: The updated figure now includes these.

Reviewer comment: L156: a d \mapsto and

Response: Corrected.

Reviewer comment: L192: where... the sentence is not finished (?)

textbfResponse We have removed "and δ_{ij} ; the Kronecker delta is defined at an earlier point in the paper.

Reviewer comment: L194: $t_i \mapsto t_c$

Response: Corrected.

Reviewer comment: L205: $\partial \Omega_b^-$ should be $\partial \Omega_s^-$?

Response Indeed, this expression is nonsense; it should have said $\partial \Omega_{\rm b}^- \cup \partial \Omega_{\rm s}^-$. Corrected.

Reviewer comment: Equation (21): delete the negative sign before 0.

Response: Corrected.

Reviewer comment: Line 237: Again, try to cite figures in order, e.g. Figure 2a?

Response: Corrected. See response to "Citation of figure 1 is missing" above.

Reviewer comment: L250: The authors are using stress and displacement matching method to estimate the static stress intensity factor. The stress matching method requires high degree of mesh resolution to obtain accurate value. Did the authors implement convergence studies on this problem? What would be the relative efficiency compare to the J integral approach?

Response: We tested extensively for convergence using a variety of different known solutions (including the solutions in Tada's handbook employed by Lai et al (2020), full citation in the manuscript) when first developing the boundary element code for Zarrinderakht et al (2022). The challenge with using a J-integral approach with a boundary element method while modeling multiple cracks in the same domain is that you cannot use just the outer domain boundary (excluding the matching crack faces) to compute the J-integral, as you would with a single crack: the domain boundary encompasses multiple cracks, so you do not get the stress intensity factor for just one of the crack tips from the calculation. You would have to introduce other contours inside the domain, compute displacement gradients, stress and strain energy density on those contours from the boundary element solution, and then compute the J-integral from these. You would also have to make sure that this interior contour does not intersect any other crack, which makes it a bit of an exercise in computational geometry that looks non-trivial to automate. The BEM solver is quite inexpensive to run even with high resolution so that seemed like a much simpler route to take.

Reviewer comment: L261: "We assume that such short cracks are readily available as material flaws in the ice shelf...". Does this sentence indicate cracks can potentially develop everywhere (with tensile effective pre-stress) with the rate defined by equation (24), although only at the predefined locations in this study?

Response: For the present version of the code, such flaws are assumed to occur only at the predefined locations, as the qualifier immediately following the cited passage is intended to indicate: \dots (although we consider them only at the predefined locations $x_{s}(t)$ and $x_{b}(t)$ as discussed above) Our next goal in this line of research is to incorporate arbitrary crack geometries (as well as buoyancy effects). That is eminently possible,¹ but beyond the scope of the present paper, or the PhD thesis it is based on. We reference this in the discussion A closely related issue is our insistence that there can be only two cracks, one on each ice surface. That choice allows a relatively simple model set-up, with cracks in known locations propagating vertically. The plot of effective pre-stress σ_{xx}^{eff} in Figures 2b and 5b, however, suggests that additional seed cracks would grow (and would have grown prior to the domain shape shown having been attained) if inserted in a large range of locations along the basal surface. σ_{xx}^{eff} is the effective pre-stress stress acting on a vertical seed crack, which is the likely favored direction in which new cracks should grow on a horizontal surface. As the two plots show, σ_{xx}^{eff} is tensile along most of the lower boundary, and in particular, where that lower boundary is approximately horizontal, suggesting that seed cracks inserted there should grow. It is plausible that seed cracks at the upper surface would also grow: Figures 2b and 5b show effective stress as defined in terms of the basal water pressure, and are therefore not relevant to the formation of surface cracks.

This suggests a future improvement of the model should incorporate not only buoyancy effects on stresses at the boundary, accounting for the effect of elastic surface displacements on the fluid pressure there, but also the possibility of multiple interacting cracks that can have arbitrary orientation, in the expectation that a preferred crack spacing and orientation will emerge spontaneously, rather than being imposed by the choice of initial domain width, and through the assumption of vertical, laterally offset cracks.

Reviewer comment: L299: "sea spring" scheme is not a well known scheme in glaciology (at lease to me). Furthermore, the citation Durand et al., 2009 does not has section 3.4 and is not about handling the normal stress condition. Therefore, this part and the rest of that paragraph is quite unreadable to me.

Response: Our apologies. The Durand et al reference given in the reference list appears to be the

¹see the Gordeliy et al reference in the updated paper, although the method for determining crack orientation described there needs to be applied to a BEM rather than XFEM discretization

result of a bibtex mix-up, and points to the wrong paper entirely. This has been corrected, and now references the appropriate paper:

Durand, G., Gagliardini, O., de Fleurian, B., Zwinger, T., and LeMeur, E.: Marine ice sheet dynamics: Hysteresis and neutral equilibrium, J. Geophys. Res., 114, doi:10.1029/2008JF001 170, 2009.

This paper does have a section 3.4, which describes the regularization method (although the phrase "sea spring" seems to have emerged later; it was in use by the time the Bassis and Berg paper cited in the manuscript was written)

Reviewer comment: section 2.5: How sensitive is the model to temporal $(\delta t/10)$ and spatial mesh resolution?

Response: There are two pieces here: the $\delta t/10$ part is the time step used to update the viscous pre-stess after a crack propagation epsiode. The results are not sensitive to the scale factor 1/10; this is simply what we used.

The temporal step size δt is determined by a CFL condition as described in section 2.5, and changing the finite element mesh automatically changes the time step size. We tested sensitivity of our results to finite element size (as well as boundary element size as previously reported in section 3.3 and found no noticeable effect of double or halving mesh resolution. We state this in the new final paragraph of section 3.3 in the updated manuscript as

Figure 8 focuses on the effect of boundary element size, because of the coarser resolution used in the boundary element method near the crevasse tips compared with the finite element mesh. We also tested for the effect of finite element mesh resolution, by doubling and halving linear element sizes. Doing so was found to have no noticeable effect on results.

Reviewer comment: L335: Describe the physical meaning of R_{xx} and \tilde{R}_{xx} rather than cite the variable from other references.

Respose: We have moved all of this material forward to section 3.1, and substantially re-written it. The probably most relevant part of this the 14th paragraph If B and n are the usual parameter's in Glen's law (Cuffey and Paterson, 2010), then we can define a proxy \tilde{R}_{xx} for V_X through

$$\tilde{R}_{xx}(t) = 2BV_X(t)^{1/n}.$$
 (12)

The quantity \tilde{R}_{xx} has units of stress and equals the non-cryostatic extensional stress in the ice if the domain remains an unfractured rectangle (in which case $\sigma_{xx} = \rho_i g(\bar{s} - z) + \tilde{R}_{xx}$). In that case, R_{xx} corresponds to the viscous extensional stress parameter R_{xx} in previous models for elastic fracture propagation (e.g., van der Veen 1998a,b; Lai et al, 2020, Zarrinderakht et al, 2022).

That should unambiguously define both \tilde{R}_{xx} ; the reference at the end of the paragraph is simply meant to help the reader understand how our \tilde{R}_{xx} relates to the fairly commonly used R_{xx} in the relevant prior literature.

Reviewer comment: L363-: Again, these variables (same with κ mentioned a few times) are cited from other papers (especially unpublished) without explanation. Very hard (if possible) for the readers.

Response: Unpublished material — that is an unfortunate part of The Cryosphere's submission set-up. You can submit "companion papers" but these may not be automatically linked, and obviously the cross-citations in the uploaded pdfs do not automatically update to the doi assigned to the (publically accessible) preprints. Googling the title of the submitted Zarrinderakht et al paper would probably have fixed the issue; in either case, the citation has been updated to give the doi for the preprint of the companion paper, now listed as "Zarrinderakht et al (2023)".

In terms of presentation here, again, this material has moved forward to section 2.1. The reference to parameters in other papers does *not* to define η^* and tau^* , which have already been defined

fully. Rather, the reference to the notation in other papers is here to help the reader who may also be reading these other papers / manuscripts understand the relationship between parameters used there, and in the present manuscript. That seems highly advisable.

The relevant updated paragraphs in section 2.1 are paragraphs 10 and 15,

... We assume instead that, as the ice stretches and thins, the surface water level h_w remains equal to a constant fraction η^* (that is, constant in time, but otherwise unconstrained as a forcing parameter) of the mean ice thickness $\bar{H}(t)$ over the domain at time t,

$$h_{\rm w}(t) = \eta^* \bar{H}(t). \tag{13}$$

 η^* is then the direct equivalent of the dimensionless surface water level parameter η used in Zarrinderakht et al (2022,2023).

and

For such an unfractured rectangular domain, the standard theory of unbuttressed ice shelves (e.g., MacAyeal and Barcilon, 1988) predicts that $\tilde{R}_{xx}(t) = (1 - \rho_i/\rho_w))\rho_i g\bar{H}(t)/2$. To account somewhat crudely for buttressing effects, we define $V_X(t)$ by putting

$$\tilde{R}_{xx}(t) = \tau^* \rho_{ig} \bar{H}(t), \qquad (14)$$

with τ^* held constant in time at a value that represents the degree of buttressing; τ^* is then the direct equivalent of the dimensionless extensional stress parameter τ used in Zarrinderakht et al (2022,2023).

The emhpasis here is on "equivalent" as opposed to "definition". You do not need to have read either Zarrinderakht et al paper to understand the definition of η^* or τ^* ; the sentences involving "equivalent" could be omitted from the manuscript without impacting the definition of either parameter.

Reviewer comment: L386: correct the unit of temperature

Response It now says -10°C

Reviewer comment: L406: Are d_b^{tot} and d_t^{tot} crack lengths at the bottom and top, correspondingly?

Response: We have moved the detailed definitoin of cumulative crack length to a new appendix D in response to a comment by the other reviewer. To calrify, the notation, the running text in section 3.1, paragraph now says ... we define cumulative basal and surface crack length variables $d_{\rm b}^{\rm tot}$ and $d_{\rm s}^{\rm tot}$ over multiple fracture propagation episodes ...

Reviewer comment: L416: variables are repetitive

Response: We do not see any actual repetition: one set has a superscript ⁻, the other does not. Note that this material has now moved to appendix D.

Reviewer comment: L417: h_b , $ht \mapsto Hb$, Ht?

Response: Yes. Corrected (now in appendix D)

Reviewer comment: Figure 2: no units for t?

Response: t in all figures is dimensionless, see section 2.6 / second sentence of section 3.1

Reviewer comment: L436: 'can begin', delete 'can'

Response: Corrected.

Reviewer comment: L447-L451: Figure 2b1 and Figure 2b2 should be Figure 4b1 and Figure 4b2

Response: Indeed. Thank you for spotting that.

Reviewer comment: L461: involve \mapsto involving

Response The text says

Subsequent episodes involve a single fracture propagation event each...

This seems correct to us.

Reviewer comment: Figure 4: are there some plotting issues such that the axes are smaller than the domain?

Response: There were significant issues with getting a zommed-in plot in paraview and subsequently overlaying axes. This led to the solution you see now.

Reviewer comment: Figure 7: same problem as Figure 4, the axis is offset, and there are two blue lines in the panels.

Response: Same explanation for the axes being where they are. The two blue lines were indeed an issue. We have updated the figure caption to say

... The horizontal light blue line at z = 0.025 indicates the surface water level, the dark blue line at z = 0 is sea level. ...

Reviewer comment: section 3.3: Could you present a figure with the mesh on top, so we can see the finite element mesh in the calculation domain as well as the boundary element?

Response: Yes.

Reviewer comment: Section 3.4: In L346 and L362, and are described as 'a constant', while these are actually the two essential forcing parameters tested in section 3.4. For these important parameters, the physical meaning should be clear, and the chosen of the ranges should be justified. **Reviewer comment:** L478-479: What are the different element sizes tested here? I think a proper mesh convergence study should be conducted and presented.

Response: The element sizes being used are specified in the caption to figure 5. We now point this out explicitly in the revised text

To test for possible resolution-dependent effects that may result, we have recomputed the solution shown in Figure 5 with different boundary element sizes as shown in Figure 8 (see caption for details).

As for a "proper" convergence study, we'd need to know more about exactly what the referee has in mind. From our knowledge of how this works in numerical analysis, you would need a known, exact solution to compute residuals, and typically would refine the mesh over multiple orders of magnitude in element size to see rates of convergence. For the former (an exact solution), that is conspicuously absent for the time-dependent, coupled problem here. For the latter (how much to change resolution by), the time-dependent nature of the problem and the curse of dimensionality rapidly kill our ability to do that with the unfunded computational resources we have available (which is the reason why we tested for robustness to halving or doubling element size instead). We *would* note that the finite element package Elmer has been tested extensively as a widely used open source code. The boundary element code was tested in detail during the preparation of Zarrinderakht et al (2022), where the viscous pre-stress is prescribed analytically. To test the coupling beween Elmer and the boundary element code, we were able to use those analytical pre-stresses, which are exact solutions of the Stokes flow model for a rectangular slab and therefore easily replicated by Elmer; the key here was to test for any indexing issues in coupling the codes.

Response: The updated text in paragraphs 10 and 15 of section 2.1 (where we have moved the relevant description of forcing) now states explicitly that η^* and τ^* are constant *in time* (but can be changed between different runs of the model):

... We assume instead that, as the ice stretches and thins, the surface water level h_w remains equal to a constant fraction η^* (that is, constant in time, but otherwise unconstrained as a forcing parameter) of the mean ice thickness $\bar{H}(t)$ over the domain at time t... and

To account somewhat crudely for buttressing effects, we define $V_X(t)$ by putting

$$\ddot{R}_{xx}(t) = \tau^* \rho_{ig} \bar{H}(t), \tag{14}$$

with τ^* held constant in time at a value that represents the degree of buttressing...