Response to RC2: 'Comment on egusphere-2023-1252', Jeremy Bassis, 25 Sep 2023

January 30, 2024

Reviewer Comment: This study is the second (or third) in a trilogy of papers by the same author team that examines propagation of crevasses in freely floating ice shelves using a boundary element model. The contribution of this manuscript is to study the effect of surface water filled and longitudinal extension on the interaction between surface and bottom crevasses.It has long been assumed that surface and basal crevasses intersect to form rifts, but the dynamics of surface and basal crevasse propagation and interaction have rarely been studied. Hence, this is a welcome study into an old, but important problem.
The problem of the interaction between adjacent cracks also has a long history in the fracture mechanics literature. This is a surprisingly challenging problem because, even under pure model I loading, the crack tip stress field from interaction results in mixed-mode loading. As a consequence, experiments and theory indicate that en echelon cracks coalesce in a "kink" rather than in a straight intersection. The analytic, numerical and experimental results that I am familiar with for this problem are typically done under idealized pure mode I propagation so it isn't clear that this necessarily applies to the ice shelf problem. Nonetheless, it would be reassuring if the authors can use their model to reproduce some of the classic results of en en-echelon elastic fracture propagation—or at least touch base—with some of the literature. I would be surprised if the interaction between surface and bottom crevasses resulted in pure mode-I behavior. The fact that this problem has a lot of history, in my opinion, deserves a little bit more attention in the introduction.
Response: Thank you for bringing this up as something to be flagged early in the paper. The method that we are using is capable of determining the direction of crack propagation through a maximum hoop stress or maximum energy release criterion (which we plan to implement in this context in the next iteration of the code). Here we have, in a sense, "cheated" to create or model set-up where crack orientation is prescribed, ot facilitate what we see as the simplest setting for interacting cracks (in which we can really take a dynamical systems approach of representing cracks geometry by crack length alone!). "Cheated" however only in very limited ways: the model we use is perfectly self-consistent in assuming crack orientation is fixed because the stress field is guaranteed to be of mode I type through symmetry. This should be obvious for the aligned cracks in figure 1. For the offset cracks of figure 4, it is important to recall that the domain is periodic, which ensures that the stress field possesses the necessary reflection symmetry about either crack. This differs from en echelon cracks, where such symmetry does not exist regardless of the far field forcing.
The cheat in our manuscript is two-fold in that sense. In reality, cracks can form anywhere and we should be seeding the domain with lots of incipient cracks that can grow and change direction. (As per the above, that is next on the docket for this work.) The second, and perhaps more relevant cheat is that we assume that the cracks do not branch or develop kinks through an instability, where even though the stress field is symmetric about the crack tip, the greatest hoop stress could be off-

axis and attract or repel the cracks from each other. We discuss these issues (without pretending to be providing a comprehensive review):

To keep the scope of our work tractable, we restrict ourselves to understanding simple interactions between basal and surface crevasses. In particular, we seek to identify how the spacing and alignment of crevasses on opposite sides of an ice shelf affect calving. Note that the study of interacting cracks has a long history, often involving complicated geometries in wich the direction of crack propagation must be determined as part of the solution of the linear elastic fracture mechanics problem (e.g. Seagall and Pollard, 1980; Baud and Reuschlé 1997). Here we use the fact that two-dimensional ice shelves flow in pure shear at leading order (Morland, 1987) to restrict ourselves to simple crack geometries in which the stress field remains symmetric about each crack and the assumption of vertical crack propagation remains self-consistent with a maximum hoop stress criterion Zehnder (2010).

In the second paragraph of section 3.3, we have also added

Note that the assumption of vertical crack propagation is then consistent with a maximum hoop stress criterion (Zehnder 2010, setion 4.4.1) but we do not address the question of crack path stability (Cotterell and Rice 1980), namely that a perturbed crack could evolve progressively away from a vertical orientation.

In addition, we reiterate the point in the penultimate paragraph of section 4.2, . . . *In addition, the assumption of purely vertical crack propagation is contingent on the highly specific crack orientations considered here, which ensure that we have purely mode 1 crack propagation. In reality, there are likely to be many interacting and potentially curved cracks, which we will address with a future iteration of the model.*

Reviewer comment: I mentioned this in my previous review of a different manuscript, but I find the non-dimensionalization counter-intuitive and hard to track. The two main dimensionless numbers are τ and η. The parameter τ is a measure of longitudinal extension and eta is a measure of the water pressure filling crevasses. A more natural (to me) definition of tau would define the non-dimensional longitudinal extension stress based on the reduced gravitational acceleration ($g' = (1-r)g$) or, equivalently, based on the resistive stress associated with a freely spreading ice shelf. This would imply that a value near unity corresponds to an ice shelf spreading under its own weight and values larger or smaller would correspond to extensional stresses that are larger or smaller compared to an ice shelf spreading purely under its own weight. I have a hard time visualizing what a τ of 0.02 means physically without resorting to using my calculator to mess with densities. I think the η parameter is even more difficult for me to visualize. The situation most relevant for most ice tongues is the surface-water free case. Previous studies have defined water depth in crevasses as a fraction of the crevasse depth, which is a bit more intuitive to visualize (brim-full vs empty). I would encourage the authors to consider their non-dimensionalization and to connect the values as much to physical situations as possible (i.e., water-free crevasses, extension larger/smaller than the gravitationally induced spreading, etc.) to make it as easy as possible for readers to understand the underlying physical situation the authors envision.

Response: Thank you for pointing this out — having settled on a notation, it is easy to start imagining that that notation is "natural".

The suggestion of scaling using reduced gravity is attractive in principle, certainly for the simple underlying parallel-sided slab geometry in the present paper. Presumably, this would be $\tau = R_{xx}/(\rho_i g' H)$ as the simpler version, or $\tau = 2R_{xx}/(\rho_i g' H)$ if you wanted to have $\tau = 1$ for an unconfined 2-D ice shelf, the factor of 2 being the result of the usual depth-averaging

The reason why we did not use a reduced gravity variable for the problem is the Stokes flow problem for the viscous pre-stress studied in the companion paper referenced in the comment above. If we did use a reduced gravity variable, that naturally would go with a reduced pressure $p' = p + \rho_i g z$. The reduced gravity scaling and reduced pressure variable leads to a simpler (body-force-free) version of the Stokes equations and simplifies the boundary conditions for the part of the lower boundary below the water line. The cost is that the boundary conditions at the upper surface become more complicated (a moot point for most buoyant fluid flow problems, which replace that upper boundary with a rigid one), and the density ratio r actually cannot be scaled out of the problem — making the scaling used here seem like the better choice at the time of writing.

That said, we are still hesitant to change the scaling here, because (at least for τ), it is the same scaling used previously in Lai et al (2020) and Zarrinderakht et al (2021), and it seems unwise to make a reader who is reading these papers in sequence translate from one scaling to another. (Notably the scaling for τ is also used in a number of theory papers on marine ice sheets / tidewater outlets by one of us (CS) as well as others (Sergienko and Haseloff, primarily).

We're hoping that this can be resolved by explaining better how to read the numerical values of τ, which differ from the ones you would get with the reduced gravity scaling above by a factor of $g'/g = (1 - r) \approx 0.11$. We have reworded the ninth paragraph of section 2 (which introduces the non-dimensionalization) as follows, providing typical values of τ and η to guide the reader:

To simplify the set of geometrical and forcing parameters, we non-dimensionalize the model using the same set of scales as in Zarrinderakht et al (2022) and Lai et al (2020). This leaves only the following dimensionless parameters,

$$\tau = \frac{R_{xx}}{\rho_{\mathrm{i}} g H}, \qquad \eta = \frac{h_w}{H}, \qquad \kappa = \frac{K_{Ic}}{\rho_{\mathrm{i}} g H^{3/2}}, \qquad W^* = \frac{W}{H}, \tag{1}$$

in addition to the dimensionless material constants given by Poisson's ration ν, and

$$r = \frac{\rho_{\mathrm{i}}}{\rho_{\mathrm{w}}}. \tag{2}$$

Above, τ is a dimensionless extensional stress, η a dimensionless depth to the surface water table, and κ a dimensionless fracture toughness. We will primarily focus on dimensionless extensional stress τ and water level η as forcing parameters, since κ is likely small: with a dimesional fracture toughness $K_{Ic} = 0,4$ MPa $m^{-1/2}$ (Rist et al, 1996) and an ice thickness of $H = 500$ m, $\kappa \approx 0.004$. To understand better how to map the dimensionless parameters to dimensional ones, recall that the extensional stress in an unconfined, one-dimensional ice shelf is $\rho_{\mathrm{i}}(1-r)gH/2$ (van der Veen, 1983; MacAyeal and Barcilon 1988). With a density ratio of $r = 0.89$, this corresponds to $\tau = 0.055$, which provides a reference value for the dimensionless extensional stress. The water level parameter is somewhat simpler: $\eta = 0$ corresponds to completely full surface cracks with the water level at the upper surface. $\eta = 1$ corresponds to a surface crack that remains dry no matter how far it is incised. A value of $\eta = 1 - r = 0.11$ represents a surface crack for which any portion below sea level is filled with water.

At risk of sounding patronizing, the meaning of η should be easier to deal with once explained, as it ranges from $\eta = 0$ when surface crevasses are "brim-full" to $\eta = 1$ for surface crevasses that remain empty no matter how deep they are, with $\eta = 1 - r$ holding additional significance by representing surface crevasses that start to fill with water when their tip reaches sea level. The proposed change in the text above should cover this along with the interpretation of τ.

The comment includes an additional suggestion / reference to modelling crevasses as being filled to a certain fraction of their length. That would be more than a change in the non-dimensionalization, but represent an entirely different way of forcing surface crevasses. We struggle, however, to envision a surface hydrology that would lead to this outcome — of the water level simply dropping in proportion to the length of the crevasse. Our earlier paper (Zarrinderakht et al 2020) considers to hydrology end-member that we consider plausible, namely (1) a fixed surface water level fed by some form of aquifer that acts as a buffer to water level changes as the crevasse propagates and widens and (2) a fixed volume of water injected into the crevasse, the latter choice turning out to be somewhat problematic for the purposes of modelling calving

Reviewer comment: One of the novelties of this study is the display of basal and surface crevasse depths in a phase plane. I think this is an interesting way of displaying the results with a lot of potential. This method introduces a slightly different perspective than the way we typically think of these problems. The way we normally think of the system is how deep will a crevasse penetrate given a small "starter crack" of some pre-determined size. The phase plane encourages us to think about pre-existing crevasses of a variety of sizes, including those that aren't necessarily "small". The question that this introduces is what processes introduce large-is crevasses that seed the initial conditions? Is the idea that crevasses advect from a region where the stress was larger? I see the authors come back to this on page 15. It might be helpful to foreshadow or mention this earlier. This is especially relevant because what we typically see is that rifts and crevasses initiate along the margins and propagate from the margins into the interior of the ice shelf. This requires a more 3D treatment of fracture, but it seems relevant that the starting depth for basal or surface crevasses here might be related to the horizontal propagation of a crevasse or rift with some stress that includes stress concentrations associated with the horizontal fracture.

Response: We admit to being daunted by the prospect of doing this in three dimensions. The point is however well made: why would you consider any initial conditions other than a small seed crack? We telegraph the later development of this idea at the end of section 2,

The ability to visualize evolution from arbitrary initial conditions using a phase plane also allows us to address how the dynamical system evolves under slow changes in forcing parameters (see also Zarrinderakht et al, 2022, sections 4-4–4.5): if started with a combination of forcing parameters that does not cause calving (generally with τ being too small or η too large), partially incised crevasse will still typically result. A subsequent change in parameters may then lead to full crack penetration starting with initial conditions dictated by the previous formation of a partially incised crack (as opposed to short seed cracks only), subject to the caveat that we do not re-compute the full viscous pre-stress in this paper when doing so (but see also Zarrinderakht et al, submitted).

Reviewer comment: I think this might be addressed in one of the other manuscripts, but when the authors introduce a crevasse into a freely floating ice shelf, the ice shelf has a flexural response that is not incorporated by the "viscous pre-stress". The flexural response tends to reduce the stress concentrated ahead of crevasses. Is this included in the boundary element model? What effect would neglecting it have on model results? What does the flexural stress do to the lateral boundary conditions? I assume this is negligible for domains that are very large compared to the flexural wavelength, but the domain sizes here seem roughly comparable to the flexural wavelength or smaller. This seems especially relevant to the interaction between crevasses. I see this is returned to near lines 385. I think it might be worth introducing this earlier, perhaps in the methods/model section as it seems quite important.

Response: This depends on a bit on what flexure means in context. If we are talking about bending moments induced by transverse (vertical) displacements, then the model does account for those: the model is the full elastostatic version of Navier-Cauchy equations for plane strain, for which the behaviour of an elastic beam is the appropriate long-wavelength (or far field) behaviour. The boundary element aspect is simply the method by which the model is solved in discretized form; the same problem could be solved with for instance an XFEM solver, or an FEM solver and some suitable method of computing a J-integral.

Beam-like behaviour should be evident in e.g. figure 8b, especially if you imagine that extended periodically to the left and right. What the model does *not* include is the buoyant restoring force that results from flexural uplift in the far field. In the near-field (over horizontal distances comparable with ice thickness scale $[H]$), that neglect is appropriate in the small strain limit that underpins the rheology: uplift is so small that hydrostatic changes in fluid pressure at the boundary have to remain small (the error in omitting them being comparable to the strain). That fails in the far field, at horizontal distances comparable to $\{E/(rho_\mathrm{i} g[H])\}^{1/4}[H] = [\varepsilon]^{-1/4}[H]$, $[\varepsilon]$ being the scale for strain, which is presumably the flexural wavelength in question. Here, vertical displacements are large enough that they affect the stress field at leading order through buoyancy effects. This is discussed in Zarrinderakht et al (2022) at the top of page 4495 (note that there is a typo in the definition of the flexural length scale on line 10 of that page (the exponent should be $-1/4$ rather than $1/4$). Implications are discussed in more detail in section 6.3 of Zarrinderakht et al (2022; see especially figure 10 therein), where we point out that a model that neglects the buoyant restoring force most likely underestimates the critical extensional stress at which calving due to basal crevasse propagation occurs (as the original comment indicates, flexure will reduces stresses around the crack).

The same point as in section 6.3 of Zarrinderakht et al (2022) is reiterated in section 3.4 of the present manuscript (around line 385 in the original submission as identified in the reviewer comment) and again in the second paragraph of section 4.2,

We anticipate that incorporating the feedback between displacement and fluid pressure at the boundary will lead to additional torques generated by vertical displacements in the far field, suppressing crack growth for very large crack spacings. We leave a study of this effect to future work. We have added a note to the effect that buoyancy effects are neglected in the model in the updated section 2, appending the following to the second paragraph:

As in Zarrinderakht et al (2022), we ignore the effect of elastic displacements on the fluid pressure at the boundary, thereby omitting buoyancy effects. This is a potentially significant omission that affects large-scale flexure effects as discussed further in section 4.2 below (see also sections 2.1 and 6.3 of Zarrinderakht et al (2022)).

Beyond that, we plan to incorporate buoyancy effects in the next iteration of the model, and would probably prefer to deal with the issue in detail then, rather than speculating further here.

Reviewer comment: Is it true that vertical propagation is the most optimal orientation for crevasse propagation? If the direction of propagation is determined by the direction of maximum principal stress, are crevasses expected to kink or turn based on the direction of maximum principal stress? A relevant physical question is what happens to crevasses that are slightly offset from each other? It would be surprising if crevasses were exactly aligned, but what if they are mis-aligned by a small fraction of the width? Would they never intersect? Is it possible that the phase space is not well resolved if crevasses are allowed to kink or turn?

Response: This is probably covered in the response to the first major comment of the review, see above. The crevasse orientation and alignment / spacing is chosen to make sure that vertical crack propagation in pure mode 1 is self-consistent, but that does not ensure that that orientation is stable to turning / kinking, as the paper now states explicitly.

Reviewer comment: Line 18-35. I think the more relevant comparison is between boundary element models and damage mechanics. Damage mechanics can be used so simulate failure under a wide variety of circumstances. Judicious choice of the damage production function allows damage mechanics to reproduce LEFM results, creep rupture or any heuristic method of simulating failure. One of the reasons that damage mechanics is so popular is that it avoid the need to remesh that is the bane of many LEFM simulations. Damage mechanics has been used to simulate the growth of both isolated surface and basal crevasses and arrays of crevasses. It would be nice to a more detailed comparison between the results considered here and those previous results.

Response: Our original statement here was misleading — it was mostly intended to refer to the observation that, if you "refine" a discrete element network by for instance halving the spacing d between discrete elements, then the breaking strength (that is, the size of hte force required to break a bond) presumably does not simply scale with element spacing: near a crack tip, a continuum model would predict that stress satisfies a one-over-square-root relationship with distance from a crack tip, and therefore bond forces near crack tips should scale as $d^{1/2}$, rather than with d. Bond strength should therefore scale as $d^{1/2}$ (rather than d?), and it was not clear to us that this is what is usually done in discrete element models — LEFM provides a systematic way around this by computing K_I (with corresponding methods that converge under mesh refinement).

Anyhow, we have removed the original discussion of discrete elements in favour of the text below, and have added additional text about phase field methods (which do agree well with LEFM — as they are intended to — albeit at additional computational cost (offset by their flexibility in capturing crack initiation and complex crack geometries). More generic damage mechanics methods do seem harder to reconcile with LEFM as the set-up us fundamentally different, and the damage production parameter (usually $\hat{B}$) is independent of other model parameters but must play a very important role, since it determines whether damage evolves significantly faster than viscoelastic stress relaxation and attendant crack tip blunting etc:

Discrete element models (Bassis, 2011, Åström et al 2013, Crawford et al 2021) are better able to cope with multiple interacting cracks, and with cracks of arbitrary geometry, but they are computationally expensive and therefore difficult to apply when exploring larger regions of parameter space. More recently, phase-field models for fracture mechanics have been applied to crevasse formation (e.g., Clayton et al 2022, Sondershaus et al, 2023), which reproduce the predictions of linear elastic fracture mechanics closely while also being able to handle phenomena such as crack splitting and viscoelastic relaxation of stresses (though, at present, seemingly only for small viscous strains). As with discrete elements, phase field models however are also computationally more expensive than classical linear elastic fracture mechanics approaches., requiring additional degrees of freedom to be solved for. Note that more general damage mechanics models (Duddu et al, 2014, Duddu et al, 2020, Jiménez et al 2017, Keller and Hutter 2014, Mobasher et al 2016) aim in a similar direction, but unlike phase field models are not ostensibly based on the energetics of creating new fracture surfaces, and introduces additional parameters that control not only a critical stress for damage production, but also the rate of damage production, which makes comparison with models based on fracture mechanics more difficult.

Reviewer comment: Line 33. It is true that discrete element models do have a dependency on the packing orientation, but it has been shown that these models to converge to the continuum elastic limit under some circumstances. One of the open questions, however, is how to specify the bond strength. Conventional discrete element models include two fracture parameters and this allows mixed-mode failure. Mixed-mode failure is something that can also be difficult to simulate within a linear elastic fracture mechanics framework because it requires an additional criterion to allow cracks to kink or bend. Typically, one assumes that cracks propagate in the direction of the largest principal stress. It seems like this study, however, assumes single mode loading.

Response: We hope this is already covered by the response to the previous comment and the response to the first comment of the review. The boundary element method used here can be adapted to curving cracks (by something analogous to what is done in

E Gordelyi, S Abbas and A Peirce (2019) Modeling nonplanar hydraulic fracture propagation using the XFEM: An implicit level-set algorithm and fracture tip asymptotics, Int. J. Solids and Structures, 159, 135–155

and that is indeed what we plan to do next. The short answer is however "yes", the current paper imposes boundary conditions (through symmetry in a periodic domain or otherwise) that ensure single mode fracture propagation.

Reviewer comment: Line 135: Vanishing elastic traction implies that elastic strains vanish at the domain boundaries, but least displacements are allowed, right?

Response: Yes. We don't think that you can impose both vanishing strain and displacement. You could have mixed conditions (one component of traction and one component of displacement vanishing) but not both components of traction and displacement vanishing, since that would be an overdetermined elliptic system.

Reviewer comment: Equation (4). What are the units and numerical value of K'(0)? I apologize if I missed this in the manuscript. If K'(0) is dimensionless, it is unclear how the units of the equation work out. If it is dimensional, then we need to know the numerical value.

Response: This wasn't particularly well described in the original submission, with just a blanket reference to Freund's book on dynamic fracture propagation. We have changed the text around equation (4) to say

As in Zarrinderakht et al (2022), we assume that each crack propagates at a rate related to how much the stress intensity factor exceeds fracture toughness K_{Ic} *by*

$$\dot{d}_{\mathrm{b}} = \max\left(-\frac{K_{\mathrm{Ib}} - K_{\mathrm{Ic}}}{K_{\mathrm{Ic}}|K'(0)|}, 0\right), \qquad \dot{d}_{\mathrm{t}} = \max\left(-\frac{K_{\mathrm{It}} - K_{\mathrm{Ic}}}{K_{\mathrm{Ic}}|K'(0)|}, 0\right),$$

where the overdot indicates differentiation with respect to time, and $|K'(0)|$ *is the derivative of Freund's (1990) universal function* K *(given by equation (6.4.26) in Freund's book), evaluated at zero crack propagation velocity. An approximate form of the universal function is* $K(\dot{d}) \approx (1 - \dot{d}/v_{\mathrm{R}})/\sqrt{1 - \dot{d}/v_{\mathrm{p}}}$*, with* v_{R} *and* v_{p} *being Rayleigh and primary wave velocities, so* $-1/K'(0) \approx 2v_{\mathrm{p}}v_{\mathrm{R}}/(2v_{\mathrm{p}} - v_{\mathrm{R}})$*. As discussed in* **?***, there are alternative hydrofracture-based models for crack tip propagation that could replace this description. We pursue the latter here due to the qualitative insights it provides.*

Reviewer comment: Line 245: Placing cracks a distance of W/4 and 3W/4 depends on the width of the domain. What about slightly offset crevasses? There is, in theory, two distances in the problem, right? The distance between the crevasses and the length of the (periodic) domain. What happens if the distance between crevasses remains the same, but the length of the domain increases?

Response: That is the point at which the symmetry conditions that lead to single mode fracture propagation fail. We hope the response to the first comment of the review (and attendant changes to the manuscript) cover this adequately.

Reviewer comment: Line 87: Punctuation? Is the semi colon supposed to be there?

Response: Oops. That should have been a full stop, and the "to" has no place here either. Corrected to say

The symmetry conditions we impose on their locations below makes that choice of orientation self-consistent.

Reviewer comment: I think lines 295 are saying that you need a larger stress to propagate an array of crevasses all the way through compared to isolated crevasses. This is consistent with previous analytic calculations by Weertman and others.

Response: This is true, in a slightly subtle sense: Weertman (1973) and van der Veen (1998a) (the two studies in which we are aware of this result) deal with crevasses in an infinitely deep ice domain when looking at interacting crevasses, and find that the relationship between penetration depth to spacing controls how much K_I is reduced relative to the case of an isolated crevasse subjected to the same spacing. The limitation to an infinite depth corresponds to a crevasse that has penetrated only a small fraction of the full ice thickness, and a lateral crevasse spacing comparable such a small depth would be the limit of a small crevasse spacing in our model (distances being scaled with ice thickness H). We have added to the text here to say

Note that in the limit of a small crevasse spacing (much less than a single ice thickness), the effect of neighbouring crevasses observed here agrees with the previous results of **?** *and* **?***, who found a significant reduction in crack tip stress intensity factor for crevasses that are spaced closer than their depth of penetration, relative to an isolated crevasse.*