Review of Comprehensive Assessment of Stress Calculations for Crevasse Depths and Testing with Crevasse Penetration as Damage

The authors make an important point that those in the field should be more careful about their definition of stress. The variety of stresses used in previous literature can lead to pronounced effects on the predicted crevasse depths, and subsequently damage and viscosity, which could potentially modify predictions of ice sheet flow and sea level rise.

That said, the authors miss relevant points in literature, and do not adequately address uncertainties in their modeling. As such, major revisions for this paper are suggested. As a starting minor point, I will refer to your "Nye's theory" as the "Zero Stress Approximation" throughout this review. First, following Benn et al. 2007's error and subsequently many others, the wrong paper is cited in Line 111 - Nye's back-of-the-envelope fracture depth calculation is equation 2 in his 1955 paper "Comments on Dr. Loewe's Letter and Notes on Crevasses". Second, Nye did not discuss basal crevasses, meltwater, or any other variation that has since been applied to the theory, so the version used in this study is not Nye's conception. Third, the term Zero Stress approximation already exists in the literature - Duddu et al., 2020; Huth et al., 2021; Coffey et al., 2024.

We appreciate the correction regarding Nye's 1955 and 1957 papers and have corrected this in the text. We would change to "Zero Stress Approximation" throughout the manuscript as recommended to be consistent with recent literature. We are encouraged that the reviewer concurs with the importance of showing how stress calculations in recent literature lead to different crevasse depths which impact parametrizations of damage and viscosity used in ice sheet modeling. We answer the concerns about additional relevant literature and modeling uncertainty inline below.

General Comments

1. You should not include discussion with LEFM or papers that apply it unless you will do so properly. In its current form, this preprint does not adequately detail LEFM nor its limitations, summarized by the following two points. Overall, since you do not model LEFM, you should remove comparison of previous papers that use LEFM, as well as your description of the theory near the start of the paper. I recommend you focus on comparisons given what you have modeled, which is the Zero Stress Approximation (you call Nye's theory).
    a. Mode I LEFM, as presented in van der Veen, 1998a,b from Tada's Handbook of Stress Intensity Factors, assumes plane strain ($\epsilon_{yy} = 0$, with y the direction into or out of the page). Naturally, as strain rate is the time derivative of strain, this would make the deviatoric stress $\tau_{yy}=0$. Hence, for Mode I LEFM, you must assume that the flow is 1D, and $R_{xx} = 2 \tau_{xx} + 0$. As such, it's inappropriate to discuss using stress states with 2HD (2 horizontal dimensions) because that goes against the assumptons used to calculate the SIFs (stress intensity factors) in the Tada Handbook.

b. Second, there are other Modes of fracture for LEFM, specifically in-plane and out-of-plane shear, which are referred to as Modes II and III, respectively. You should not discuss Mode I LEFM as the failure mechanism in shear margins, which makes including Mode I LEFM papers confusing in your discussion of over-/under-predicting in shear margins.

   We appreciate the constructive feedback regarding our consideration of LEFM. We believe that that our findings are relevant to the application of LEFM to crevasses based on our responses below to the two individual points. In a revised manuscript, we would provide a more thorough introduction to LEFM including mode I,II,III and mixed mode fracture and studies assessing crevasse orientation.

a. We appreciate the raising of this complexity and agree that it is an important broken assumption when comparing stress intensity factors to fracture toughness from plane strain tests. That said, we question whether, if LEFM is to be applied to all regions across ice shelves as has been done in Lai et al., 2020 and is likely to be done by future studies, applying a crack perpendicular stress that is known to be inaccurate is the best course of action. Put another way, when applying LEFM where tau_yy~=0, neglecting the unknown effect of violating this assumption is unavoidable. Applying a stress perpendicular to the crevasse (s_xx), which is the direction that seems reasonable to assume as most important, that is not the actual stress seems like an additional but more avoidable error. A minor additional point about this: our understanding is that in 2D LEFM derivations with plain strain, it is plain strain in an elastic material such that a stress parallel to the crevasse tip will occur (s_zz in fracture literature, s_yy in crevasse literature) (Anderson, 2005 section 2.10). The point remains that this assumption is violated because any value could occur when applying LEFM across glaciers and ice shelves versus the specific value controlled by Poisson's ratio.
b. Our understanding of Mode I, II, and III crack orientations is that they are based on the loading direction rather than the deformation state. Mode I is defined as "where the principal load is applied normal to the crack plane" (Anderson, 2005 section 2.6). Given the observation of crevasses (admittedly surface crevasses) in shear margins aligning approximately 45 degrees from flow (Colgan et al., 2016; Van Wyk de Vries et al., 2023), which aligns with the maximum principal stress direction, we would consider these to be mode I dominated cracks. Mode III would be a crevasse aligned parallel to flow in a shear margin. There is certainly additional complexity (mixed mode) as crevasses in shear margins reorient with flow, which is less the case for mode I crevasses in the center of flow. But for crevasse initialization in particular, we would argue mode I is an appropriate starting place.

   With these responses, we propose to maintain discussion of LEFM and LEFM-based studies in the manuscript. We would be clear about the violation of the assumption noted (a) and that there is additional complexity in shear margins as discussed in (b) particularly as crevasses evolve with flow. We would also be clear that our recommendation of Calculation F is more caveated in LEFM accordingly. At the same time, the differences in stress calculation feeding into LEFM will still give differing results with the approximate pattern shown with the zero stress approximation and past studies employing LEFM have selected different resistive stress versions, so we think that our results are worth reviewing for future LEFM based studies even if we did not make a recommendation.

2. The Zero Stress Approximation does not uphold horizontal force balance. This has been shown in Buck 2023 and discussed in Coffey et al., 2024. For isothermal ice, incorporating force balance as discussed in those two papers yields deeper crevasses, and reduces the calving stress threshold by a factor of 2. This is a significant omission that would alter the predicted crevasse depths maps and velocity misfits when using damage.

   We appreciate the raising of this important point regarding the zero stress approximation and Buck's horizontal force balance model. Reviewing in particular Figure 6 of Buck (2023), we make the following notes:

   1. For the surface crevasse depth / basal crevasse height predictions, similar to LEFM, this model would increase the difference in basal crevasse that predicted from each stress calculation, because of the non-linear relationship between stress and crevasse size.
   2. For the modeling component of the paper, inclusion of Buck's horizontal force balance model would likely have an impact like going from calc. F to calc. E for the Larsen B remnant. The deviation from the zero stress approximation grows with decreasing buttressing. As buttressing numbers tend to be lowest at the front, this would cause a relative weakening of the ice near the front relative to elsewhere. This is similar to the relative weakening toward the front from calc. E from omitting ice softening from the vertical strain rate.

   With this, we propose to leave the crevasse penetration plots (figures 5 and 9) in terms of the zero stress approximation. This is in part because it is not clear how the force balance model should be applied when longitudinal flow cannot be assumed (shear margins). We would note the qualitative impact on table 3 and figure 4 as we did for LEFM. For at least the Larsen B, we would generate a crevasse penetration map with calc. F with the force balance model applied, and show the difference to calc. F with only the zero stress approximation. We would also test velocity misfit from damage with calc. F and force balance and compare to the original calc. F and calc. E results. All new results discussed above would be included in the supplement and noted in the discussion section of the manuscript.

3. There are substantial omissions in addressing uncertainties in your data-model comparison.
    a. From the modeling side, I have the following questions. They all tie to the point of inverse problems allowing for non-unique matches to data.
        i. What is the uncertainty regarding rheology, such as the flow law exponent?
        ii. Is there uncertainty in the dependence of your effective viscosity on temperature?
        iii. What is your uncertainty in the vertical temperature profile?
        iv. Is using a depth-averaged ice hardness equivalent to depth-varying ice hardness when computing fracture depths, or do these give possibly different results as discussed in Coffey et al., 2024?
        v. Is SSA, a long-wavelength or large-scale continuum approximation of the momentum equation, still a good approximation when you have fractures or rifts?
        vi. Why do you use isotropic damage mechanics when Huth et al., 2021 suggest using anisotropic damage?
        vii. For the inversion, what is your cost function?
        viii. If using damage is worse than inversion (Figure 10a), could this mean that your starting point for rheology with no damage (pink) is unreasonable?

    Each of these uncertainties would be briefly discussed in the methods section consistent with the statements below. The purpose of modeling in this paper was primarily to assess whether calc. E or F yields a relative pattern of damage that is more consistent with observed velocity, as opposed to providing an estimate of crevasse sizes with fully assessed error bounds. We respond to these points individually below noting where we would do additional work:

        i. As reviewed in Cuffey & Patterson (2010), rheology is anisotropic and path dependent as crystallographic preferred orientation and grain size change with deformation. Values of n from 2 to 5 have been found with 3 being used most frequently per their recommendation. Millstein et al. (2022) recently recommended n=4 as a better “average” representation, and we would assess n=4 for the Larsen B remnant (Scar inlet) and PIG with the same modeling workflow as supplementary material.
        ii. This uncertainty follows the above uncertainty, as microscale processes are temperature dependent and the function for flow factor will have error accordingly.
        iii. We briefly discuss in the supplement the dearth of constraining observations for through thickness temperatures in shelves. We would note this in the methods section.

iv. No. We would reference (Coffey et al., 2024) in discussing the difference in predictions caused.
v. No continuum stress balance equations including full stokes will be a good representation of fractures and rifts.
vi. This is a limitation of the ice sheet model selected, ISSM, and discussion of this limitation has been added.
vii. We use the velocity error alone (no regularization) because damage from crevasses may be expected to have sharp gradients. Checking of whether this prevented the algorithm finding the best solution versus using a small regularization coefficient was performed.
viii. We would not expect damage as forward calculated from crevasse penetration to be better than inversion, because inversion minimizes error with no physical constraints and can thus account for local unknown temperature profile, imperfect rheology, and crevasse effects. Our methodology aimed primarily at assessing Calc. E vs Calc. F only addresses temperature in bulk and crevasses (with its own error). That said, we were surprised out how close a first pass at making a damage field with crevasse penetration and bulk temperature tuning got to matching inversions and think that future work including rheology modifications and crevasses could help understand how much of each is being accounted for by inverted damage.

b. From the observations side, I have the following questions.
   i. Isn't the data returned from rifts inappropriate for use in your model because a) mélange may have different material properties, b) you may be computing strain rates from mélange velocity and spreading rather than glacial ice, c) the ice thickness is significantly decreased, greatly decreasing observed thickness H and increasing your crevasse depths e.g. R_{xx} / \rho_i g H to by default predict a full thickness fracture? One attempt to deal with this by Coffey et al., 2024 is masking H with an average of local unbroken ice thickness, but this does not fix the problem with strain rates. For a study explicitly on crevasse depths you should mask out rifts as your prediction of crevasse depths becomes non-causal.
   ii. How do you compute the derivative of the velocity field to create strain rate maps? For example, the strain rate maps in Wearing et al., 2016 (thesis) vs Furst et al., 2016 are quite different. Wearing discusses the influence of various spatial filters - it would be nice to see maps of strain rate that go into your crevasse depth maps, perhaps in an appendix.

Responding to each point:

i. We agree on all subpoints and would mask the rifts from figures 5, 6, and 9 (based on a thickness/strain rate criterion) to avoid readers taking information from these zones where the result is self-fulfilling.
ii. We use second order accurate central differences (single-sided at boundaries) and no filtering. Strain rate maps for the Larsen B and PIG shelves would be added to the appendix as you suggest.

4. Since you discuss cliff failure, your paper is about errors of around a factor of 2 with the Zero Stress approximation, and you are asking authors to be careful about the confusion between resistive stress and deviatoric stress, is there anything more you would like to say about Bassis and Walker, 2011?

   Reviewing Bassis & Walker (2011), S_{xx} is termed horizontal deviatoric stress when it is instead the horizontal resistive stress. Their Equation 2.2 is consistent with the definition of resistive stress and the resulting equation 2.4 appears to match the depth-averaged boundary condition for a marine ice cliff in terms of resistive stress.

5. Please use names for the stress calculations that have physical relevance and meaning. Calculation A-F gives the reader no insight into the differences. Please make this change in the text and especially in the figures.

   We propose the following naming system to add physical relevance while maintaining brevity: E[X]-S[Y]-[Z] where [X] is assigned to 0 (no eff strain rate), P (planar eff strain rate), or F (full effective strain rate); [Y] is assigned to F (flow direction) or M (max prin direction); and [Z] is assigned to 0 (lateral deviatoric stress not used) or 1 (lateral deviatoric stress used).

   This would give the following mapping from the old to the new system:

   Calc. A = E0-SF-0

   Calc. B = E0-SM-0

   Calc. C = EP-SM-0

   Calc. D = EF-SM-0

   Calc. E = EP-SM-1

   Calc. F = EF-SM-1

6. Following Table 2 and Figure 4: Can you compute, for a given ice shelf or idealized rectangular domain ice shelf, maps of the ratio of components of strain rate, e.g. minimum / maximum principal strain rate? This will give your audience a good idea of where these different choices of stress are most at play and would pair very nicely

with your Figure 4 if you put them side by side. I think this would greatly strengthen your study.

We appreciate this suggestion and agree it would go a long way towards connecting figure 4 to the later crevasse penetration and crevasse penetration difference plots. We will make the suggested plots for the Larsen B remnant (or Scar inlet) and PIG shelves and include them with figure 4 or as an immediately subsequent figure. In conjunction with the recommendation from reviewer #2 to make code available for reproducing Figure 4, we will also plan to add code to make these plots to the Jupyter notebook.

<u>Specific Comments and Technical Corrections</u>

1. There is no need to include surface meltwater in your figure 1 diagram. You do not use it, making it confusing for a fast read of your figures.
   We will remove meltwater from both panels of figure 1.
2. Lines 12-14: unnecessary 2 sentences. Either put those in the main text with specific citations or leave them out of the abstract. All you need to say in this upper abstract is that stress calculations vary greatly across studies and make cross-study validation challenging.
   Sentences removed.
3. Introduction paragraph 1 is too large. Be more succinct or make 2 paragraphs.
   a. Line 29: be more specific in tying back these fracture processes to grounding line flux, which is a glacial contribution to the rate of sea level rise.
      Added.
   b. Line 31: define buttressing with citations.
      We have added the following definition with a sentence before this one.
      NEW: Ice shelves restrain upstream ice flow via buttressing, backstress from shear load transmitted to embayment walls or from compressive load caused by pinning points (Fürst et al., 2016; Gudmundsson, 2013; Schoof, 2007).
   c. Line 36: I would not equate calving with shelf collapse. “Both can result in” rather than “the result can be the same”
      Change implemented verbatim.
   d. Line 38: New paragraph at Finally, maybe drop that word choice.
      We would add a split at line 40 between processes that drive uncertainty in sea-level rise projections and how crevasse depths are used in parametrizations of those processes.

e. Lines 38 to the end of the paragraph: reads as summarizing some previous work with no clear story arc, ending in surface energy balance which is never again mentioned in the paper. Decide if there is a message here or move this to when you discuss individual studies.
We would drop energy balance and end with a stronger statement on importance of crevasse depths for these parametrizations of important processes. We would still cite Colgan et al. (2016) elsewhere as it is the most recent crevasse review paper and summarizes some of the above.

4. Line 54: I would add Horizontal Force Balance (see main point 2).
We have modified and added the start of that paragraph
OLD: There are two primary methods for calculating crevasse depths from stress. The Nye crevasse formulation (Nye, 1957) assumes ice has no tensile strength and that the presence of a crevasse does not modify the surrounding stress field. Linear elastic fracture mechanics (LEFM) …
NEW: There are three primary methods for calculating crevasse depths from stress. The Zero Stress Approximation (Nye, 1955) assumes ice has no tensile strength and that the presence of a crevasse does not modify the surrounding stress field. The Horizontal Force Balance method (Buck, 2023) maintains the assumption that ice has no tensile strength but considers the impact of water pressure in basal crevasses on force balance. As basal crevasse height increases with stress according to the Zero Stress Approximation, so too does the force balance impact creating a crevasse size amplifying effect. Linear elastic fracture mechanics (LEFM) …

5. Line 56: LEFM "can recognize" ice strength, but it does not have to as you can choose zero fracture toughness e.g. for Mode I, K_{Ic} = 0.
In an attempt to give a better one sentence opener for LEFM (because another fair point is that a "100KPa stress criterion" would also be recognizing ice strength, we make the following change.
OLD: Linear elastic fracture mechanics (LEFM), which was applied to crevasses by van der Veen (1998a, 1998b), recognizes ice strength and considers the stress-amplifying effect of crevasse geometry.
NEW: Linear elastic fracture mechanics (LEFM), which was applied to crevasses by Rist et al. (1996) and van der Veen (1998a, 1998b), considers the stress-amplifying effect of crevasse geometry and allows laboratory measurements of a material's resistance to fracture to be used for predicting fracture in more complex stress states.

6. Paragraph of Line 75: please end with your main result at the end of the introduction so the audience knows where it is going, not just the broad methodology.

We have added the following final sentence:
NEW: "We find that common assumptions made when calculating resistive stress from strain rates can lead to differing crevasse depths by a factor of two and that the most physically based calculation applied in an ice sheet model as damage best recreates observed velocity."

7. Line 81: ice deformation is set from the full stress tensor regardless of rheology. Also, if you're talking about ice shelves or SSA to start with, please begin with that instead so readers can follow your logic.
We are seeking to provide a distinction between the use of the deviatoric stress and the full or Cauchy stress.
OLD: While the viscous flow of ice is driven by stress differences (deviatoric stresses), brittle failure comes from the full stress.
NEW: While the viscous flow of ice is driven by deviatoric stress, the component of the Cauchy stress that does not cause volume change during deformation, brittle failure is driven by the Cauchy stress itself.

8. Line 82: provide a citation for ice not being able to flow in triaxial tension.
Following a helpful point from review #2 we will clarify this as "equi-triaxial" tension. If sigma_1=sigma_2=sigma_3, all deviatoric stress terms are zero. We wanted to point out that an incoherency that would arise from using deviatoric stress for brittle failure is that ice would not be predicted to fail for that scenario.
9. Lines 82-3: Lithostatic pressure is essential to all glacier deformation. Lithostatic pressure is what creates the driving stress (e.g. \rho_i g \partial_x s) in SSA and is what drives vertical shear ice flow in SIA. Take away gravity as a body force and nothing drives glacier flow. It is often referred to as a viscous gravity current.
Thank you for noting that our wording was confusing, we will clarify the text to include your comment.
10. Lines 89-90: Near an ice cliff (or ice front) there will likely be vertical shear effects. Not so simple.
Consistent with a similar comment from reviewer #2, we have removed this sentence and modified the following one for continuity.
11. Lines 92-4: Consider citing relevant literature: Gao et al., 2023 (firn), Coffey et al., 2024 (temperature), Meng et al., 2024 (poroelasticity).
We will do so.
12. Lines 99: meltwater in a surface crevasse. A pool of meltwater, or a small lake, will add a vertical force downwards on the ice surface (e.g. MacAyeal et al., 2015)
Correction taken verbatim.

13. Lines 103-7 sentence: can be much more succinct. This can be visualized in the supplement of Buck and Lai 2021, or the Appendices of Coffey et al., 2024. Also, be more specific - compressive stress vs lithostatic stress? I recommend lithostatic, unless you are talking about sources of buttressing providing compression.
    We will make the sentence more succinct and use “lithostatic” as suggested.
14. Equation 4: Is this resistive stress only along-flow or crevasse-normal? Otherwise, you should include the second invariant of strain rate in your calculation (see Appendix A of Coffey et al., 2024). I realize you discuss more later on about calculating resistive stress, but make a point of what that equation in Nick et al., 2010 is missing early on and what you want to change about it.
    We have noted that the resistive stress here is the 1D form

    OLD: The resistive stress, R_{xx}, in Nick et al. (2010) is given as...

    NEW: The resistive stress, R_{xx}, in Nick et al. (2010) is the one-dimensional form and is given as...

    We will foreshadow by adding the following to the end of the paragraph:

    NEW: Assessing three-dimensional implementations of equation 4 that consider effective strain rate (ice softening from multiple directions of deformation) and lateral stress is the primary focus of our work.

15. In case you strongly disagree and want to keep the LEFM portions of this paper,
    a. Line 135: Say what boundary conditions are unphysical and what applications (ice shelves) must be changed.
    b. Line 143: The resistive stress is not the same because of the plane strain assumption of the Mode I LEFM result in Tada.

    We will address these consistent with our discussion of LEFM following main point 1 and will revise L135 and L143 to include your suggestions.
16. Section 2.4.1: In general, please define the whole Cauchy stress tensor, resistive stress tensor, the relation between deviatoric stress and strain rate, etc. Lead by example in being thorough with your stress definitions.
    We will do so with an additional section prior to what is presently 2.4.1. This section will cover the Cauchy stress tensor, deviatoric stress tensor, and resistive stress tensor. It will also introduce why this calculation path is necessary, namely we can measure strain rate directly in the field and from velocity products which gives deviatoric stresses through the flow law. And then full stress versus thickness is recovered by calculating resistive stress from deviatoric stress and adding lithostatic pressure (and sometimes water pressure) back in.

17. Line 171: provide a citation, or argue that individuals in the field have done this (cite them) and state your opinion on the matter. If you write out the full expression from mass conservation with variable density, what is the relation between strain rates and density?
   We will cite at least Amaral et al. (2020) here, who neglected vertical strain rate. We will note that neglected vertical strain rate (assuming it to be zero) will cause a density decrease and write the lagrangian form of continuity (to avoid confusion with advected density change) in terms of strain rates d rho / d t = - rho(e1 + e2 + e2). For crevasse formation in the absence of firn, we will recommend upholding incompressibility as likely the most physically consistent.
18. Line 178: Move your chosen approximate momentum equation (SSA) up earlier when defining your stresses.
   We will discuss the SSA assumption and its necessity when working from surface velocity in the new section 2.4.1 where we introduce the stress tensor.
19. Equation 10: Move this up, and use another equality to show that the product of the first two terms is what you are calling viscosity.
   We will move it to after equation six and define effective viscosity immediately after.
20. Lines 185-8: Choi et al., 2018 and Lai et al., 2020. You don't need a new sentence about the Lai et al., 2020 application.
   Sentence about application removed.
21. Lines 195: can you provide a citation for where you get the jargon planar stress tensor?
   We are trying to balance consistency with glaciological literature and accuracy with continuum mechanics definitions. Glaciological literature (e.g. van der Veen, 1999) will define tau_1 and tau_2 from the upper-left 2x2 matrix (x, y) as the maximum and minimum principal stresses. Recognizing the 3d state, tau_zz could end up being the maximum or minimum principal stress in some cases. Maximum / minimum principal stress from the planar tensor was our idea for efficiently balancing these targets. Instead, we will just explain this difference between notation in glaciological vs continuum mechanics literature and then use tau_1 and tau_2 rather than introducing terminology that could cause confusion with the formal meaning of plane strain.
22. Line 203: cite SSA with neglecting vertical shear stresses.
   We have noted SSA as the reason for neglecting bridging stress with citation (MacAyeal, 1989).
23. Equations 18 and 19: Do you mean at the crack tips? Otherwise \sigma_{zz} should be a function of z.
   Added "at the crack tip".

24. Line 215: You will not get the full stress as a function of depth unless you use \sigma_{zz} (z). This is clear from the (z) component of Stokes flow, removing vertical shear stress terms.
    We rewrote this to clarify we are solving for the surface crevasse depth and basal crevasse height where the full stress is zero.
    OLD: Equations 18 and 19 can be substituted into Equation 17 to find the full stress as a function of depth (σ_n (z) in Equation 5) for surface and basal crevasses.
    NEW: Equations 18 and 19 can be substituted into Equation 17 to find the full stress at the crack tip as a function of surface crevasse depth and basal crevasse height.
25. Lines 230, 526: Add Bassis and Walker to this list.
    We originally chose not to include Bassis and Walker because they use the depth-averaged boundary condition rather than calculations from strain rates. As noted in main point 4, we do not think they used deviatoric stress rather than the resistive stress.
26. Line 253: shelf (typo).
    Fixed! Thank you.
27. Section 3.1: Make these plots! It would be so useful! Even if they go in the appendix, they are the basis for how you understand the bizarre geometries of real ice shelves. I know you have Table 3, but following main point 6, it would be helpful.
    Thank you for this suggestion, we plan to make these plots and add them to the main text as noted below main point 6.
28. Lines 267-8: Since the ocean is saltwater, the freezing point is roughly -2 C. Why do you use 0 instead of -2?
    This was a carryover from old analysis; we would update to -2 C.
29. Line 271: Write the rigidity function and say what you have used to interpolate temperature between the surface and the bed. This significantly affects crevasse height, see Lai et al., 2020 and Coffey et al., 2024.
    We will add clarification here (constant temperature is assumed), define rigidity (B = A^(-1/n)) following equation 6. We also plan to add a more thorough description of temperature assumptions in the supplement, as we note in response to a comment further down.
30. Line 277: The theory you chose suggests that surface crevasses alone don't really matter for making a large damage variable and don't drive calving without water. I would be more forthcoming about why ice shelves are a natural environment to study crevasses (removing basal drag), and that basal crevasses are likely the driver of calving, as they have received far less attention in terms of number of papers.
    We will foreshadow why shelves are selected at the start of 3.2.1 as this would not yet be obvious; we could have shown the surface crevasse penetration patterns on

grounded ice for example. We will add emphasis to why shelves are studied and that basal crevasses drive in 3.2.2 and 3.3.

31. Line 279: Isn't the Larsen B remnant multi-year landfast sea ice (Ochowat et al., 2023) instead of glacial ice? Would it have different fracture properties? It also collapsed from surface meltwater in 2022 (Ochowat et al., 2023) - wouldn't this affect your modeling if the surface crevasses had meltwater in them, or if there were surface meltwater ponds again?
    We are working with the southern end of the Larsen B that never collapsed. This remaining continuous shelf has been called both the Larsen B remnant (Borstad et al., 2016) and the Scar Inlet (Ochwat et al., 2024) . In hindsight, Scar inlet is less likely to cause confusion with the landfast ice in the embayment, and we would switch to Scar inlet as the primary title throughout the manuscript.
32. Paragraph starting with line 285: good logic! Well written.
    Thank you.
33. Paragraph 293: It is unclear what exactly you are doing with temperature. In 293, you say it is constant with depth. In 301, it is quadratic, with a 5-degree shin at 1/3rd of the ice thickness discussed in Line S19. There's a lot of discussion about tuning and it's very unclear what the overall effect is - you warm bias temperature, and you tune temperature to match velocity with calculation F (shouldn't your damage calculations with calculation E by definition be worse?). Place some of this in S2 if you feel it is detailed.
    We will provide a short qualitative description in the main text and move a detailed explanation of how temperature is treated for the crevasse penetration calculations, for tuning with a given damage field from calculatoins E / F, for inversions, and for the no damage case.
34. Line 321: for isothermal ice and the Zero Stress theory, you should be able to predict just how much larger the basal crevasses are than the surface crevasses by computing the ratio of basal to surface crevasses. I would recommend doing so.
    We will note the theoretical ratio.
35. Table 3: Please make some of this nondimensional. I don't know how to contextualize these other than relative to each other but not a fraction of the ice thickness.
    We agree fraction of ice thickness does not make sense as zero stress approximation crevasse depths / heights are independent of ice thickness for a given resistive stress and that producing arbitrary crevasse sizes is unfortunate. The only option we see is normalizing against calc. B. Normalizing against a calculation that is not the most "true" seems misleading. Normalizing against calc. F however would recast the plot and obscure direction of crevasse size change across flow

states. For this reason, we would prefer to maintain as is with a reasonable value of strain rate so that predicted sizes are not unrealistic.

36. Figure 4: non-dimensionalize y-axis, change labels to be physically relevant.
    Same as above. We would add additional labels to the x-axis (-1: pure shear, 0: longitudinal spreading, 1: equi-biaxial spreading).
37. Lines 358 & 390, Figures 5 & 9: as discussed in the main points 3.2.1, every theory will predict rifts if they exist in the data because of the reduced thickness and velocity anomaly. These should either not be included in your analyses or you should treat them carefully.
    As described under main point 3.2.1, we will mask out the rifts to better avoid drawing attention to non-meaningful results. The regions corresponding to lines 358 and 390 are shear margins not the rifts adjacent to them. We will clarify this point in the text. The northern shear margin is partially failed like PIG 2018, and we will double check that the cross section is not going through the more fracture regions.
38. Section 4.3: Is it valid to use the Zero Stress approximation for shear cracks in addition to tensile cracks?
    Following onto our comments regarding the application of LEFM in shear areas, we would argue that cracks aligned ~45 degrees from the flow direction are tensile cracks in areas of shear deformation. Additional complexity would again apply as deformation reorients crevasses, but as a first pass we think zero stress is reasonable.
39. Line 442: "tuned … across the domain" for calculation F? This is unclear.
    A single depth-averaged temperature is assumed across the domain and is tuned for minimum velocity misfit with the pattern of damage applied on top. We will improve clarity in line 442.
40. Line 447, Figure 10: You should be clear about the point of the inversions. The way it is presented, if I want to match observations, I should just use the inversions, no need for calculation E or F. But I doubt that's what you want to say - presumably, it is that you can't do better than the inversion, and the reader should measure your calculations (E & F) velocity misfit relative to no damage. You should state this more explicitly as it is unclear during a first fast read how to interpret your results.
    The takeaways we want are:
    1. Calculation F outperforms calculation E giving some evidence that calculation F has a stronger connection to real basal crevasse height patterns
    2. Like you point out: Given the bookends of no damage and the best possible damage pattern for matching velocity (inversion), damage from crevasses far outperforms no damage and gets fairly close to inversions.

We would reorder the third paragraph in 4.4 to better cover these two points in this order / priority.

41. Lines 516-18: You can remove this example and just cite them as being unclear.
    We will do so.
42. Lines 522-3: With flow-direction versus maximum principal stress direction, did this alter the conclusions of this study? What is the order of magnitude of this distinction?
    We did not discuss the flow direction calculation after noting the obvious, no crevasses in shear margins, in the results section. We would reiterate that finding here.
43. Line 545: see main point 1.
    We will reiterate the broken LEFM assumption here.
44. Can you add Wilner et al., 2023 to your table 4?
    Yes, however, because Wilner uses the divergence of velocity rather than resistive stress in 3D or its 2D version, it will get its own row.
45. Section 5.6: see main point 1.1 and 1.2.
    Consistent with our discussion after main points 1.1 and 1.2, we would raise the violated assumption regarding lateral stress as well as potential for mixed mode in shear margins. The differences pointed out between calculations will still apply. We would soften our suggestion to use calculation F as applying the most accurate crevasse perpendicular stress but being subject to these sources of uncertainty.
46. Line 590: the variation by a factor of 2 - is this more or less than confusing the deviatoric and resistive stress? Isn't this something that should be clear from the start of the study?
    We will clarify early on that using a deviatoric stress rather than resistive would be a factor of 2 error (depending on strain rate state). And that this study's finding is that even when using a form of resistive stress, assumptions in the calculation steps can still lead to a factor of 2 error for the zero stress approximation.
47. Line 590: The regions of difference between stress calculations on ice shelves is the major new finding and citing a figure to go along and show those differences would be very helpful in your figure 4 - see main point 6.
    We will point back to the updated figure 4 / new figure with the new plots.
48. Lines 604-5: re physical basis for the Zero Stress approximation, see main point 2.
    We will include in the conclusion reiteration of how Buck's Force Balance Method, particularly in longitudinal flow, is truer to the criterion being no tensile strength.
49. Lines 607-8: cite someone or provide a supplemental figure for these points about convexity of the ice front.

We would discuss terminus shapes from (Choi et al., 2018), plots of the maximum and minimum principal stresses in (Benn et al., 2023), and potentially make plots showing distance from surface crevasse tip to waterline and/or crevasse penetration near the terminus for some of the stress calculations.

50. Table A2: might be useful to have equations in this table as well. Specifically for damage, defining resistive stress, etc.

We would review Table A2 and add at least the suggested equations.

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