

# Reviewer #1

## **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 have changed 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 provide our updates corresponding 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 assumptions 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 have provided 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  $\tau_{yy} \approx 0$ , 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 have maintained discussion of LEFM and LEFM-based studies in the manuscript. We have described 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 where we introduce LEFM (Section 2.4). We have also re-raised the noted violation (a) where we recommend Calculation F\_EF-SM-1 (5.1, 5.3, 5.6). Regardless of what LEFM studies choose to do with resistive stress, the difference will be significant and follow the pattern shown with the zero stress approximation.

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\_EF-SM-1 to calc. E\_EP-SM-1 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 have left the crevasse penetration plots 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 have noted the qualitative impact on table 3 and figure 4 as we did for LEFM.

We generated a crevasse penetration map with calc. F\_EF-SM-1 with the force balance model applied, and show the difference to calc. F\_EF-SM-1 with only the zero stress approximation. We also tested velocity misfit from damage with calc. F\_EF-SM-1 and force balance and compare to the original calc. F\_EF-SM-1 and calc.

E\_EF-SM-1 results. These results were included in supplement Section S7 and noted in Section 4.4.1 in the main text.

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 was briefly discussed in the methods section (3.3) consistent with the statements below. The purpose of modeling in this paper was primarily to assess whether calc. E\_EP-SM-1 or F\_EF-SM-1 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.

- 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. We assessed  $n=4$  for the Larsen B remnant (Scar inlet) and PIG with the same modeling workflow as supplementary material (S6) and noted the findings in Sections 4.4.1 and 5.1.
- ii. We have clarified in the text that this uncertainty follows the above uncertainty, as microscale processes are temperature dependent and the function for flow factor will have error accordingly.

- iii. We have noted the lack of temperature constraints in the methods section.
- iv. No. We now reference (Coffey et al., 2024) in noting the difference in predictions caused.
- v. No continuum stress balance equations including full Stokes will be a good representation of fractures and rifts. We now note that rifts are being treated as continuous.
- vi. This is a limitation of the ice sheet model selected, ISSM, and discussion of this limitation has been added.
- vii. We now state which inversion coefficients were used.
- viii. We were able to improve this inversion result by initializing with predicted crevasse penetration. We note this in section 4.4.

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 have masked the rifts from figures 5 and 6 with a minimum thickness (150m) 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. We have added Strain rate maps for the Scar Inlet and PIG shelves to the supplement (Fig. S1).

- 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 changed 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 gave the following mapping from the old to the new system:

Calc. A = A\_E0-SF-0

Calc. B = B\_E0-SM-0

Calc. C = C\_EP-SM-0

Calc. D = D\_EF-SM-0

Calc. E = E\_EP-SM-1

Calc. F = 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 made the suggested plots for the Larsen B remnant (or Scar inlet) and PIG shelves and included them with figure 4. In conjunction with the recommendation from reviewer #2 to make code available for reproducing Figure 4, we added code to make these plots to the Jupyter notebook.

### **Specific Comments and Technical Corrections**

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 removed 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”  
[Made suggested change.](#)
  - d. Line 38: New paragraph at Finally, maybe drop that word choice.  
[We added the recommended split between processes that drive uncertainty in sea-level rise projections and how crevasse depths are used in parametrizations of those processes \(and dropped “Finally”\).](#)
  - 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 dropped energy balance and ended with a stronger statement on importance of crevasse depths for these parametrizations of important processes.](#)
4. Line 54: I would add Horizontal Force Balance (see main point 2).  
[We have modified and added to 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) ...

We also discuss force balance as introduction section 2.3.

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 made 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), applied to crevasses by (Weertman, 1973) and many subsequent researchers, 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 and have made the following change.



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 clarified 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 added a sentence pointing out the change in lithostatic pressure with distance causes flow.
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 have noted these effects with these citations.
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 made the section more succinct and described the increase in compressive stress with elevation from lithostatic pressure minus water pressure as “net compression” because we needed to distinguish between this and lithostatic pressure alone for surface crevasses.
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 foreshadowed 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 rewrote this section consistent with our response to main point 1 addressing (b). Discussion of whether SSA is appropriate for grounded ice crevasses (a) has moved to when we present SSA (section 2.5.1) and discussion of Enderlin & Bartholomaus (2020) and Mottram & Benn (2009) (section 5.3).

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 have done so with a new section 2.5.1 and have introduced the need for the calculation path from strain rate to resistive stress for remote sensing based studies here as well.

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 cited Amaral et al. (2020) here, who neglected vertical strain rate. We also noted that neglected vertical strain rate (assuming it to be zero) will cause a density decrease according to new equation 21. We noted that prior to crevasses, density should not change, but that when crevasses are present it might.

18. Line 178: Move your chosen approximate momentum equation (SSA) up earlier when defining your stresses.

We are now discussing SSA earlier in the text (in our new section 2.5.1).

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 moved the equation for nye's generalization (now Equation 17) to directly after the 1d version of Glen's flow law (Equation 16). We dropped mention of viscosity throughout the paper as we do not need to use effective viscosity anywhere.

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?

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 have now explained this difference between notation in glaciological and the true definition ( $\tau_1 > \tau_2 > \tau_3$ ) 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 ( $\sigma_n(z)$  in Equation 5) for surface and basal crevasses.

NEW: Equations 29 and 30 can be substituted into Equation 28 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 made these plots and added them to Fig. 4 in the main text.

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 updated to -2 C for all analysis.

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 added clarification here (constant temperature is assumed) and defined rigidity ( $B = A^{(-1/n)}$ ) following equation 16.

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 foreshadowed why shelves are selected at the start of 3.2.1 as this would not yet be obvious. We added emphasis to why shelves are studied and that basal crevasses drive in 3.2.2.

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 (Ochowat et al., 2024) . Scar inlet is less likely to cause confusion with the landfast ice in the embayment, so we switched so Scar Inlet 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 provided an updated short qualitative description in the main text and discussed further with a figure in supplement Section S4.

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 noted the ratio with our temperature assumptions.

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\_E0-SM-0. Normalizing against a calculation that is not the most "true" seems misleading. Normalizing against calc. F\_EF-SM-1 however would recast the plot and obscure direction of crevasse size change across flow states. For this reason, we maintained 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 included our updated stress calculation labels.

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 masked out the rifts to better avoid drawing attention to non-meaningful results. We moved the cross section in Fig. 7 to avoid the northern shear margin which appears to be partially rift.

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. We addressed in the updated introduction on LEFM (Section 2.4) and did not readdress here.

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 rewrote the temperature section to improve clarity.

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 rewrote paragraph 3 in section 4.4 to focus on these takeaways.

41. Lines 516-18: You can remove this example and just cite them as being unclear.

We have done so, and made a clarification between modeling studies that use the physical basis of the zero stress approximation with directly calculated Cauchy stress and those that used a stress calculation in the text (section 5.2) and table 4.

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 included discussion of flow direction results (no crevasses in some shear margins zones) in section 5.1 and 6.

43. Line 545: see main point 1.

We reiterated the broken LEFM assumption here (section 5.3).

44. Can you add Wilner et al., 2023 to your table 4?

We added this. Because Wilner (testing the calving law in Pollard et al. (2015)) uses the divergence of velocity rather than resistive stress in 3D or its 2D version, we added a row.

We also added several studies reviewed since the initial submission (Hulbe et al., 2016; Scott et al., 2010) and classified studies that used the zero stress approximation’s physical meaning in models (Clayton et al., 2022; Huth et al., 2021).

45. Section 5.6: see main point 1.1 and 1.2.

We noted the violation of plain strain here (1.1) here. We did not note mixed mode (1.2) with the rationale regarding shear crevasse formation from our updated LEFM introduction (section 2.4).

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 have now clarified this here and earlier (at Table 3).

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 have pointed back to the updated figure 4.

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 cited the result from Choi et al. (2018).

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

We have added definitions for damage and resistive stress.





## Reviewer #2

This article provides a detailed review of how resistive stress is calculated for crevasse depth evaluation in various articles in the literature and show that they result in different crevasse depth predictions. As a part of the study, the authors considered both idealized cases and real cases of Antarctic ice shelves, especially those with shear margins. I really appreciated the systematic study in this article, given that my research has focused on exploring the crevasse models in the literature. I believe the article is of interest to the ice sheet modeling community and I will be happy to see it published in this journal. The article is well written and organized (and amazingly I could not find a single typo in text). However, I have several comments listed below, most requiring minor changes or clarifications but I have a couple major comments at the end related to stress evaluation using planar remote sensing data. While I have several articles of mine referenced in my review, I leave it up to the author's discretion to cite or not cite them in their paper.

-- Ravindra Duddu

We thank Dr. Ravindra Duddu for his assessment that this study systematically shows how assumptions in resistive stress calculations impact crevasse depth predictions. We note our revisions based on individual comments inline below.

### Detailed Comments:

Line 51 - I suggest your say zero stress theory instead of Nye crevasse formulation. Originally, Nye (1957) did not include the effect of water pressure, but was later introduced by Jezek (1984).

Per this comment and similar from reviewer #1, we have changed this throughout the manuscript.

Line 61 – If I remember correctly, Enderlin and Bartholomaus (2020) used observed surface strain rates to calculate stress in grounded glaciers. If the basal boundary is not free slip, then the stress variation with depth in grounded glaciers is not linear. Please clarify this point that the resistive stress is not a constant if boundary condition is not free slip.

That is correct. We have added discussion of where the shallow shelf approximation holds up for grounded ice to Section 2.5.1 and to where we discuss the impact of crevasse depth calculation on Enderlin & Bartholomaus (2020) in section 5.3.

Line 81 – The term “stress differences” could be misunderstood. I think it is better to say, “viscous flow is driven by deviatoric stress, which is the component of Cauchy stress that

does not cause volume change during deformation; whereas brittle failure is governed by the Cauchy stress.”

We have incorporated this recommendation as:

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.

Line 82 – Consider replacing the terms “biaxial” with “equi-biaxial” and “triaxial” with “equi-triaxial” in the paper, as you are referring to the cases when the stress magnitudes are equal. The terms biaxial and triaxial do not imply the stresses are the same in various directions.

Thank you for this suggestion for improving clarity; we have updated line 82 accordingly and have prepended the “equi” throughout the rest of the manuscript when indicating magnitudes are equal.

Line 90 – I do not agree with the statement “in areas with simple stress states, such as on an ice shelf or near an ice cliff ...” Due to free surface effects near an ice cliff of a grounded glacier or floating ice shelf the stress state is not simple. Only in the far field do the stress becomes independent of the horizontal direction and one can use force balance calculation to determine resistive stress. Not sure what I am missing. Please clarify this point.

Thank you for pointing this out. What we were getting at is that stress states are more frequently assumed for ice cliffs (Bassis & Walker, 2011) and longitudinal extension dominated areas of shelves (Millstein et al., 2022). We agree it is incorrect to say that the stress state is truly simple and have removed the sentence as the above point isn’t necessary in the paragraph. We have added, “In remote-sensing or field-measurements based workflows,” to the following sentence as the introduction for calculating resistive stress from strain rates.

Line 94 and 98 – Change in ice rigidity due to firn layer are important, as you noted. Sorry for the self-promotion, but I would encourage you to read our recent papers (Gao et al., 2023; Clayton et al., 2024), which examine the influence of firn layer on crevasse propagation in glaciers and ice shelves. In Gao et al. (2023) we show that in ice shelves considering depth-varying density due to the firn layer changes the buoyancy depth and leads to deeper penetration. In Clayton et al., (2024) we consider both depth-varying Young’s modulus and

density and derive analytical solutions and conduct analytical LEFM studies. We found that the inclusion of depth-dependent density influences the resistive stress and can thwart or promote deeper crevasse propagation depending on the glacier and ocean water heights, which is more nuanced than the description by van der Veen (1998a).

Gao, Y., Ghosh, G., Jiménez, S., & Duddu, R. (2023). A finite-element-based cohesive zone model of water-filled surface crevasse propagation in floating ice tongues. *Computing in Science & Engineering*, vol. 25, no. 3, pp. 8-16, May-June 2023, doi: 10.1109/MCSE.2023.3315661

Clayton, T., Duddu, R., Hageman, T., & Martínez-Pañeda, E. (2024). The influence of firn layer material properties on surface crevasse propagation in glaciers and ice shelves. *The Cryosphere*, 18(12), 5573-5593. <https://doi.org/10.5194/tc-18-5573-2024>, 2024.

We appreciate references to recent literature that considered additional firn effects on crevasses. We have added both to the citation with van der Veen (1998a). We also updated the sentence to recognize the impact on stress from deformation as well as density.

OLD: For surface crevasses, firn density is an important consideration as it can significantly change the lithostatic pressure near the surface (van der Veen, 1998a).

NEW: For surface crevasses, firn properties are an important consideration as they can significantly change the lithostatic pressure and resistive stress near the surface (Clayton et al., 2024; Gao et al., 2023; van der Veen, 1998a).

Line 124 – You write height above buoyancy twice. Instead say: “where  $\rho_{pw}$  is the density of the proglacial water (lake or ocean) and  $H_{ab}$  is the height above buoyancy defined as” and get rid of repetition.

Suggestion taken verbatim.

Line 130 – Perhaps you should mention that Eq. (4) is only valid in 1D.

We have updated the sentence before equation (4):

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

Line 136 – Better to say “... but considers the stress singularity at the crevasse tip ...” Stress concentration is bounded and occurs around holes and U-shaped notches. At the sharp crack tip in LEFM theoretically there is a stress singularity.

Thank you for the correction; the suggestion was taken verbatim.

Line 175 – In Eq. (8), (9) and others, whenever the subscripts are not indices but rather descriptors like “eff” or “eef, planar” you must use  $\text{\textit{eff}}$  or  $\text{\mathrm{eef}}$ . Only indices that are symbols taking numerical values are italicized.

We have updated this throughout the manuscript.

Line 197 – You can mention that  $\tau_1$  and  $\tau_2$  in Eq. (13) are invariants with coordinate transformation, whereas  $\tau_{\text{flow dir}}$  is not in Eq. (12).

We have added the following sentence after the sentence that defined  $\tau_1$  and  $\tau_2$ .

NEW: The principal stresses are invariants with coordinate transformation, while the flow direction stress is not.

Line 266 to 268 – Calculating surface and basal depths using different rigidities based on temperature differences is a bit ad hoc. To see how reasonable it is, a full Stokes FEM simulation could be conducted to obtain the stress field and then the depth where tensile stress becomes zero can be taken as the crevasse depth.

We appreciate this suggestion and, while we agree that this is an ad hoc assumption, we did not see other options as less ad hoc and went with the easiest thing to implement. We think future work could potentially develop damage fields based both on predictions of ice softening and crevasses to try to better understand the degree to which damage is accounting for each factor and would need better evaluation of ad hoc assumptions involved in basal crevasse heights particularly. For this work however, where the pattern of damage from crevasses alone is being tested via modeling as a small add on, we believe this to be beyond our scope.

Line 303 to 304 – In this discussion about inverting for damage, it can be noted that damage in the form of crevasses introduces anisotropy. In Huth et al. (2021) we show that when this anisotropy is considered we get more realistic rift propagation. A comment can be added to state that inversion for isotropic viscosity has the limitation attention that it applies the effect of crevasse damage equally in all directions.

Huth, A., Duddu, R., & Smith, B. (2021). A generalized interpolation material point method for shallow ice shelves. 2: Anisotropic nonlocal damage mechanics and rift propagation. *Journal of Advances in Modeling Earth Systems*, 13(8), e2020MS002292. <https://doi.org/10.1029/2020MS002292>

We agree with this point made here and by reviewer #1. Isotropic damage is a limitation of our chosen ice sheet model, ISSM, and we have clarified and addressed this limitation with following addition at the end of the second to last paragraph in Section 3.3 (line 423-429):

NEW: Damage, both calculated from crevasse penetration and with inversion, is implemented assuming isotropy. The reduction in load bearing area from crevasses would be expected to be directional and anisotropic damage laws have been shown to better capture tabular iceberg calving (Huth et al., 2021).

Table 3 – You are calculating these at a point on the glacier by assuming that the strain rate is uniform. Is that right or did I misunderstand, please clarify.

That is correct. To add clarity in the text, we have added to the second sentence of the paragraph (line 315):

OLD: To compare the stress calculations in these flow types, the magnitudes of each strain rate component are held constant.

NEW: To compare the stress calculations in these idealized flow types, the same magnitude is used for each strain rate component ( $\dot{\epsilon}_{xx}$ ,  $\dot{\epsilon}_{yy}$ ,  $\dot{\epsilon}_{xy}$ ), which are assumed to be constant through thickness.

We have also added the following sentence to the end of the Table 3 caption:

NEW: Strain rate is assumed to be constant through thickness.

Line 333 – Please clarify the phrase “... but the ratio of depths between calculation will be identical” Does mean ratios taken column wise or row wise.

This sentence refers to Figure 4. We have added the sentence following the noted sentence.

NEW: For example, surface crevasse depth predictions for a strain rate state corresponding to -1 on the x-axis with Calc. B\_E0-SM-1, C\_EP-SM-0, and D\_EF-SM-0 will still yield a depth twice that of Calc. E\_EP-SM-1 and F\_EF-SM-1.

Line 351 – Would be good to include the function plotted in Figure 4 in the Appendix, for the sake of reproducibility. Or maybe you can share the code used to generate these plots.

We have updated the Yupyter notebook linked under code availability to reproduce all plots in Fig. 4.

Line 388 - Perhaps, my only major concern is that the evaluation of stress from observed strain rates in the region of a crack is physically meaningless. This has not been particularly mentioned in the paper. To elaborate, the observed strain rate in an area where there is a crack will be large and the stress evaluated using the Glen's law will be large, so you may rightly predict a full depth crevasse. However, the true Cauchy stress there will be zero because the ice rigidity becomes zero in an open crevasse. Therefore, it is important to

point out that while one can use this approach, the evaluated stress is a trial stress (borrowing this term from plasticity) and not the true stress.

This comment is consistent with others from reviewer #1 regarding rifts in particular; we have now made note of this in Section 3.2.1 introducing the trial stress terminology. We also masked out the rifts based on a thickness criterion in Figures 5 and 6 to reduce emphasis on these regions where a prediction of full crevasse penetration is trivial.

Line 400 to 410 – The argument comparing Larsen B and Stancomb-Wills shelves are a bit difficult to understand. Also, why did you not include the cross-section thickness and crevasse penetration depth plots in Figure 8, just like in Figure 7. This would perhaps make it easy to follow the differences.

We added all plots to the Brunt/Stancomb-Wills figure but moved it to the supplement as Fig. S2. We did this as the Brunt/Stancomb-Wills only really serves as a counterexample of what the velocity profile of a rift shear margin can look like. With this, we have also removed that paragraph that specifically considered the Brunt.

Figure 9 could also be clearer if the crevasse depth plot like in Figure 7E was included. Also, by looking at the REMA, how are you able to tell whether crevasses are full depth or not. Please clarify this for the general reader.

We have moved the plan view cross section plots from old Fig. 9 to Fig. 8 (replacing the Brunt/Stancomb-Wills figure). We have added the  $e_2/e_1$  and crevasse penetration cross section plots (Fig. 7) to Fig. 9. We have removed the claim that crevasse penetration is not full in 2014 but suggest that the clear breakup in 2018 (if coming from when crevasse penetration becomes full) would suggest full crevasse penetration was not present in 2014.

Line 443 – The statement “The assumption that the stress calculation that gives the best modeled velocity ...” is fine but going to back to my previous comment this is just a trial stress whereas the true stress in a fully crevassed region is zero as the rigidity goes to zero, whereas the strain rate will be large.

Adding to the discussion before, this point is certainly fully true of rifts where any calculation would predict full penetration and the limitation on maximum damage will apply. We added that clarification to the methods section (3.3) but ultimately did not re-raise the caveat here.

In Figure 10A, I would recommend plotting the root mean squared of the velocity misfit rather than the average. The average would not be an accurate measure. Also, if the nodal velocity misfit is the least with inversion, then why do we need to use calculation F using

observed strain rates. Can we not just invert for damage and obtain damage and make estimates of crevasse depth.

We used the mean absolute error, if the concern is that positive and negative error were allowed to cancel. We have now clarified this in the figure and throughout the text.

The first goal with the modeling work was to assess whether calc. E\_EP-SM-1 or F\_EF-SM-1 had a stronger tie to bulk rheology to recommend that calculation for applications where modeling could not or may not be performed (field measurements, remote sensing based workflows). A second point is that there are downsides to the inversion including non-uniqueness and being a catch all for temperature error, rheology error, and crevasses. The forward calculation of course will be affected by these error sources as well, but in a more traceable way.

We rewrote the second to last paragraph in section 4.4 to clarify that the primary goal was calc. E\_EP-SM-1 vs calc. F\_EF-SM-1 and noted the approach to the success of inversion as a minor subsequent point.

Line 462 to 465 – Please clarify what you mean by excess velocity. I am also confused by the comment “This may indicate that the damage in the spreading flow region ...” Velocity is not a measure of strain rate or damage, instead the symmetric gradient of velocity could be used.

We are making the case that the high velocity misfit near the terminus is a result of over-prediction of crevasse penetration from Calc. E via overly increased strain rate approaching the terminus. To improve clarity, we have adjusted the sentence on line 461.

OLD: The calculation E correlation plot (Fig. 11A) shows that it predicts excess velocity for the fastest-moving ice near the terminus.

NEW: The modeled velocity correlation plot for damage from calculation E (Fig. 11A) shows that it predicts excess velocity for the fastest-moving ice near the terminus.

Figure 11 – Explain why in the 0 - 400 m/yr range the observed velocity is greater than the modeled velocity and why this happens in both cases A and C.

If this comment is in regard to the dense line of points that falls just bull the -20% error line, that is likely the blue patch (Fig. 11b and d) to the right of the right shear margin. This may indicate the shear margin is too weak in both cases and is not pulling on the slow-moving ice enough. We have now noted this region and added that explanation in the second paragraph of Section 4.4.1.



Section 5.2 – As I am reading, I feel like there is a distinction between studies using planar remote sensing data and ice sheet models that was not clearly identified. While you are right about the usage of resistive stress in the formulas with planar remote sensing data, with modeling studies one can evaluate the full stress and identify where it becomes zero in the domain and determine the crevasse depth directly based on zero stress theory. This is what is done I believe in Todd et al. (2018), where they use a full Stokes model that calculates velocity and pressure from mass and momentum balance. In Clayton et al. (2022), we use the momentum balance to calculate the elastic stress in a Maxwell viscoelastic solid and then determine the zero-stress based crevasse depth directly from the stress distribution.

Clayton, T., Duddu, R., Siegert, M., & Martinez-Paneda, E. (2022). A stress-based poro-damage phase field model for hydrofracturing of creeping glaciers and ice shelves. *Engineering Fracture Mechanics*, 272, 108693. <https://doi.org/10.1016/j.engfracmech.2022.108693>

Thank you for this comment, we agree this point warrants more clarification. We concur with this distinction but would add the following complexity. In studies that implement the crevasse depth calving law in ice sheet models, approaches split between what could be described as applying the crevasse depth law as a calving parametrization or as a physical criterion. (Choi et al., 2018) and (Wilner et al., 2023), both SSA modeling studies of calving laws, do not find when the maximum principal stress (and assumed zero stress approx. crevasse tip) go to zero in the ice column, but instead use the equations from Nick et al., (2010) (equations 1 and 2 in this manuscript) with deviatoric stress component(s) subbed in for  $R_{xx}$ . In the Sun et al. (2017) damage law, it appears that the maximum principal deviatoric stress is used again leaning towards the parametrization version. In general, the framework we would discuss this distinction in would be:

- Field and remote sensing measurements of strain rate: a calculation of the types shown (e.g. A through F) in this study is unavoidable.
- Modeling studies:
  - Zero stress approximation as parametrization: will also assume the form of one of these calculations, though it may bypass aspects like effective strain rate.
  - Physical basis of zero stress approximation: bypasses the need for using one of our calculation versions.

A corresponding question would be whether to classify modeling studies using the “physical basis” approach like Todd et al. (2018) and In Clayton et al. (2022) as calculation F. Where SSA flow is a perfect assumption, these studies would be identical in our

understanding. For inter-study comparison, this is perhaps useful so long as we are clear about the fact that modeling studies can bypass the calculation paths we lay out and handle more complex stress states if full stokes, visco-elastic, etc.

We have now added a condensed discussion of the above framework in sections 5.2 (the classification table). We included studies that directly calculated crevasse depths in the table but noted them with asterisks to distinguish from studies that calculate resistive stress as laid it in this study.

Line 534 – The calculation F is recommended for use by the authors, which is reasonable if dealing with planar remote sensing data. With SSA models once can use resistive stress and the crevasse depth formula based on the approach of Sun et al. (2017) or simply once can calculate the depth varying stress using the pressure formula (Huth et al., 2023). Further, principal stress can be obtained from the eigenvalues of the 3 x 3 full stress matrix.

Huth, A., Duddu, R., Smith, B., & Sergienko, O. (2023). Simulating the processes controlling ice-shelf rift paths using damage mechanics. *Journal of Glaciology*, 69(278):1915-1928. <https://doi.org/10.1017/jog.2023.71>

Following the above discussion, we could rephrase the recommendation:

OLD: We recommend calculation F based on its physical basis and success in recreating ice sheet velocity patterns when implemented as damage.

NEW: For studies using calculating crevasse depths from observed strain rates, we recommend calculation F based on its physical basis and success in recreating ice sheet velocity patterns when implemented as damage. For studies implementing the crevasse depth calving law or damage laws based on the zero stress approximation, we recommend following the physical basis of the zero stress approximation (crevasse tips reach where the maximum principal stress from the Cauchy tensor reaches zero), which calculation F reproduces for the assumption of SSA flow.

Line 541 – Perhaps, I am repeating this statement. I believe the stress calculations are not valid in Mottram and Benn (2009) or Enderlin and Bartholomaus (2020) as they study grounded glaciers, unless they are free slip at the base. For glaciers frozen to the bed the stress is not linear. In Jimenez and Duddu (2018) we used a cubic function to fit to the stress profile. I would encourage the authors to study grounded glaciers and the effect of boundary conditions and how this changes the Nye depth. This could be a quick study that could be added to this paper.

We agree with this point. We are curious how much error it would cause for dry surface crevasses that are shallow relative to ice thickness. For deep surface crevasses from

meltwater, certainly this will be a major factor. We began working on a study to assess this as recommended. Ultimately, we felt that for relatively shallow surface crevasses in tidewater glaciers, a case could be made from the literature that assuming constant velocity with depth is not likely to cause significant error (added to Sections 2.5.1 and 5.3). Developing conditions for when this assumption breaks down in terms of parameters like predicted crevasse depth, ice thickness, fraction of force balance taken by basal drag, and valley cross section aspect ratio would be valuable to develop approximate guard rails for this assumption, but we believe this to be out of scope as a supplement to this study.

In the acknowledgements, only funding for the DEMs generation is mentioned, but it is not clear how the authors were funded to conduct this study.

Thank you for pointing this out. We have now included our funding sources.

## References

- Amaral, T., Bartholomaus, T. C., & Enderlin, E. M. (2020). Evaluation of Iceberg Calving Models Against Observations From Greenland Outlet Glaciers. *JOURNAL OF GEOPHYSICAL RESEARCH-EARTH SURFACE*, 125(6), e2019JF005444.  
<https://doi.org/10.1029/2019JF005444>
- Anderson, T. L. (2005). *Fracture mechanics: fundamentals and applications* (3rd ed.). Boca Raton, FL: Taylor & Francis.
- Bassis, J. N., & Walker, C. C. (2011). Upper and lower limits on the stability of calving glaciers from the yield strength envelope of ice. *Proceedings of the Royal Society A: Mathematical, Physical and Engineering Sciences*, 468(2140), 913–931.  
<https://doi.org/10.1098/rspa.2011.0422>
- Borstad, C., Khazendar, A., Scheuchl, B., Morlighem, M., Larour, E., & Rignot, E. (2016). A constitutive framework for predicting weakening and reduced buttressing of ice shelves based on observations of the progressive deterioration of the remnant Larsen B Ice Shelf. *Geophysical Research Letters*, 43(5), 2027–2035.  
<https://doi.org/10.1002/2015GL067365>
- Choi, Y., Morlighem, M., Wood, M., & Bondzio, J. H. (2018). Comparison of four calving laws to model Greenland outlet glaciers. *The Cryosphere*, 12(12), 3735–3746.  
<https://doi.org/10.5194/tc-12-3735-2018>
- Clayton, T., Duddu, R., Siegert, M., & Martínez-Pañeda, E. (2022). A stress-based poro-damage phase field model for hydrofracturing of creeping glaciers and ice shelves.

*Engineering Fracture Mechanics*, 272, 108693.

<https://doi.org/10.1016/j.engfracmech.2022.108693>

Clayton, T., Duddu, R., Hageman, T., & Martínez-Pañeda, E. (2024). The influence of firn layer material properties on surface crevasse propagation in glaciers and ice shelves. *The Cryosphere*, 18(12), 5573–5593. <https://doi.org/10.5194/tc-18-5573-2024>

Coffey, N. B., Lai, C.-Y., Wang, Y., Buck, W. R., Surawy-Stepney, T., & Hogg, A. E. (2024). Theoretical stability of ice shelf basal crevasses with a vertical temperature profile. *Journal of Glaciology*, 70, e64. <https://doi.org/10.1017/jog.2024.52>

Colgan, W., Rajaram, H., Abdalati, W., McCutchan, C., Mottram, R., Moussavi, M. S., & Grigsby, S. (2016). Glacier crevasses; observations, models, and mass balance implications. *Reviews of Geophysics* (1985), 54(1), 119–161. <https://doi.org/10.1002/2015RG000504>

Cuffey, K. M., & Patterson, W. S. B. (2010). *The Physics of Glaciers* (4th ed.). Burlington, MA: Elsevier.

Enderlin, E. M., & Bartholomäus, T. C. (2020). Sharp contrasts in observed and modeled crevasse patterns at Greenland's marine terminating glaciers. *The Cryosphere*, 14(11), 4121–4133. <https://doi.org/10.5194/tc-14-4121-2020>

Fürst, J. J., Durand, G., Gillet-Chaulet, F., Tavard, L., Rankl, M., Braun, M., & Gagliardini, O. (2016). The safety band of Antarctic ice shelves. *Nature Climate Change*, 6(5), 479–482. <https://doi.org/10.1038/nclimate2912>

Gao, Y., Ghosh, G., Jiménez, S., & Duddu, R. (2023). A Finite-Element-Based Cohesive Zone Model of Water-Filled Surface Crevasse Propagation in Floating Ice Tongues.

*Computing in Science & Engineering*, 25(3), 8–16.

<https://doi.org/10.1109/MCSE.2023.3315661>

Gudmundsson, G. H. (2013). Ice-shelf buttressing and the stability of marine ice sheets.

*The Cryosphere*, 7(2), 647–655. <https://doi.org/10.5194/tc-7-647-2013>

Hulbe, C. L., Klinger, M., Masterson, M., Catania, G., Cruikshank, K., & Bugni, A. (2016).

Tidal bending and strand cracks at the Kamb Ice Stream grounding line, West Antarctica. *Journal of Glaciology*, 62(235), 816–824.

<https://doi.org/10.1017/jog.2016.74>

Huth, A., Duddu, R., & Smith, B. (2021). A Generalized Interpolation Material Point Method for Shallow Ice Shelves. 2: Anisotropic Nonlocal Damage Mechanics and Rift

Propagation. *Journal of Advances in Modeling Earth Systems*, 13(8),

e2020MS002292. <https://doi.org/10.1029/2020MS002292>

MacAyeal, D. R. (1989). Large-scale ice flow over a viscous basal sediment: Theory and application to ice stream B, Antarctica. *Journal of Geophysical Research: Solid*

*Earth*, 94(B4), 4071–4087. <https://doi.org/10.1029/JB094iB04p04071>

Millstein, J. D., Minchew, B. M., & Pegler, S. S. (2022). Ice viscosity is more sensitive to

stress than commonly assumed. *Communications Earth & Environment*, 3(1), 1–7.

<https://doi.org/10.1038/s43247-022-00385-x>

- Mottram, R. H., & Benn, D. I. (2009). Testing crevasse-depth models: a field study at Breiðamerkurjökull, Iceland. *Journal of Glaciology*, 55(192), 746–752.  
<https://doi.org/10.3189/002214309789470905>
- Nick, F., Veen, C. J., Vieli, A., & Benn, D. (2010). A physically based calving model applied to marine outlet glaciers and implications for the glacier dynamics. *Journal of Glaciology*, 56. <https://doi.org/10.3189/002214310794457344>
- Nye, J. F. (1955). Comments on Dr. Loewe's Letter and Notes on Crevasses. *Journal of Glaciology*, 2(17), 512–514. <https://doi.org/10.3189/S0022143000032652>
- Nye, J. F. (1957). The Distribution of Stress and Velocity in Glaciers and Ice-Sheets. *Proceedings of the Royal Society of London. Series A, Mathematical and Physical Sciences*, 239(1216), 113–133.
- Ochwat, N. E., Scambos, T. A., Banwell, A. F., Anderson, R. S., MacLennan, M. L., Picard, G., et al. (2024). Triggers of the 2022 Larsen B multi-year landfast sea ice breakout and initial glacier response. *The Cryosphere*, 18(4), 1709–1731.  
<https://doi.org/10.5194/tc-18-1709-2024>
- Pollard, D., DeConto, R. M., & Alley, R. B. (2015). Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure. *Earth and Planetary Science Letters*, 412, 112–121. <https://doi.org/10.1016/j.epsl.2014.12.035>
- Roger Buck, W. (2023). The role of fresh water in driving ice shelf crevassing, rifting and calving. *Earth and Planetary Science Letters*, 624, 118444.  
<https://doi.org/10.1016/j.epsl.2023.118444>

- Schoof, C. (2007). Ice sheet grounding line dynamics: Steady states, stability, and hysteresis. *Journal of Geophysical Research: Earth Surface*, 112(F3).  
<https://doi.org/10.1029/2006JF000664>
- Scott, J. B. T., Smith, A. M., Bingham, R. G., & Vaughan, D. G. (2010). Crevasses triggered on Pine Island Glacier, West Antarctica, by drilling through an exceptional melt layer. *Annals of Glaciology*, 51(55), 65–70. <https://doi.org/10.3189/172756410791392763>
- Sun, S., Cornford, S. L., Moore, J. C., Gladstone, R., & Zhao, L. (2017). Ice shelf fracture parameterization in an ice sheet model. *The Cryosphere*, 11(6), 2543–2554.  
<https://doi.org/10.5194/tc-11-2543-2017>
- Van Wyk de Vries, M., Lea, J. M., & Ashmore, D. W. (2023). Crevasse density, orientation and temporal variability at Narsap Sermia, Greenland. *Journal of Glaciology*, 69(277), 1125–1137. <https://doi.org/10.1017/jog.2023.3>
- van der Veen, C. J. (1998). Fracture mechanics approach to penetration of surface crevasses on glaciers. *Cold Regions Science and Technology*, 27(1), 31–47.  
[https://doi.org/10.1016/S0165-232X\(97\)00022-0](https://doi.org/10.1016/S0165-232X(97)00022-0)
- van der Veen, C. J. (1999). Crevasses on glaciers 1. *Polar Geography*, 23(3), 213–245.  
<https://doi.org/10.1080/10889379909377677>
- Weertman, J. (1973). Can a water-filled crevasse reach the bottom surface of a glacier? *International Association of Scientific Hydrology--Association Internationale d'Hydrologie Scientifique (International Union of Geodesy and Geophysics--Union de Geodesie et de Geophysique Internationale), Publication. [Louvain]*, 95, 139–145.



Wilner, J. A., Morlighem, M., & Cheng, G. (2023). Evaluation of four calving laws for Antarctic ice shelves. *The Cryosphere*, 17(11), 4889–4901.  
<https://doi.org/10.5194/tc-17-4889-2023>