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 respond to 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 will change this verbiage to "zero stress approximation" 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 would address this in the discussion section (5.3) where we consider this study and (Mottram & Benn, 2009). We provide specifics below in response to your comment about line 541. We will also note the impact of considering stress variation with depth for grounded ice in section 2.1 of the background where we discuss other factors that impact crevasse depth predictions like firn and temperature profile.

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 will prepend 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{} or \mathrm{}. Only indices that are symbols taking numerical values are italicized.

We will update this throughout all the equations.

Line 197 – You can mention that tau_1 and tau_2 in Eq. (13) are invariants with coordinate transformation, whereas tau_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 will clarify and address this limitation with following addition at line 307 (end of the second to last paragraph):

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). Our use of damage is to coarsely compare the bulk rheology implications of different stress calculations, so we move forward with this isotropic assumption.

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 ($\dot{\boldsymbol{\varepsilon}}_{xx}$, $\dot{\boldsymbol{\varepsilon}}_{yy}$ $\dot{\boldsymbol{\varepsilon}}_{xy}$), 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, C, and D will still yield a depth twice that of Calc. E and F.

(Per comment from reviewer #1, the calculation names will be updated to a code that gives information about the assumptions involved.)

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.

For publication, we would add this as a new section in the Jupyter notebook that presently reproduces the crevasse penetration plots. The new section would make the figure 4 plot as well as the minimum principal strain rate / maximum principal strain rate plots of Larsen B / PIG as recommended by reviewer #1.

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 plan to better clarify this and would introduce then use the terminology "trial stress" because we agree it helps in explanation. Additionally, we plan to mask out the rifts based on a thickness and/or strain rate criterion in Figures 5 and 6 to reduce emphasis on these regions where a prediction of full crevasse penetration is accurate by self-guaranteed. We recognize that large crevasses that have not formed rifts are still subject to this.

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.

Cross-section thickness and crevasse penetration plots were not included because we did not find that we referenced them in the current explanation provided. We will add cross-section thickness and crevasse penetration plots back in. Additionally, we would rewrite a few sentences that try to discuss too much at once in this section.

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.

A crevasse depth cross section like Figure 7E will be added to the figure. The argument is that the apparent discontinuous ice in 2018 is the result of complete crevasse penetration and the continuous appearance of ice in 2014 suggests crevasse penetration was partial except for the rifts. We would soften the claim that this guarantees there wasn't complete crevasse penetration prior to 2018, as it is possible that penetration was complete but had not yet led to the fragmentation in 2018 for some reason.

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 were any calculation would predict full penetration and the limitation on maximum damage will apply. We will point that out in this section.

In Figure 10A, I would recommend plotting the root mean squared of the velocity misfit rather than the average. The average would not be accurate measure. Also, if the nodal velocity misfit is the least with inversion, they 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 will clarify this in the figure as well as its caption and discussion in text.

The first goal with the modeling work was to assess whether calc. E or F 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 would add clarification (in sections 3.3 and 4.4) about the purpose of the no damage and inverse simulations as bookends to contextualize the forward calculation results as well as discussion about the strengths / weaknesses of crevasses from inversion versus forward calculation.

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 (Figure 11B and 11D) 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 will add that to the explanation in 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 us the equations from Nick et al., (2010) (equations 1 and 2 in this manuscript) with deviatoric stress component(s) subbed in for Rxx. 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 would add a condensed discussion of the above framework in sections 5.2 (the classification table), 5.4 (discussion of CD law impact), and/or 5.5 (discussion of damage law impact).

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 would assume SIA with varying ice thicknesses and calculate surface crevasse depths with the zero stress approximation, as recommended, to assess this. We would put this work in the supplement and note its findings as relevant to the Mottram and Benn (2009) and Enderlin and Bartholomaus (2020) studies in the discussion section here.

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 will include our funding sources.

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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., 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

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