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.

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.

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.
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?

  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.

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?
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.
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.

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.
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.
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.
    b. Line 31: define buttressing with citations.
    c. Line 36: I would not equate calving with shelf collapse. “Both can result in” rather than “the result can be the same”
    d. Line 38: New paragraph at Finally, maybe drop that word choice.
    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.
4. Line 54: I would add Horizontal Force Balance (see main point 2).
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, $K_{Ic} = 0$.
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.
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.
8. Line 82: provide a citation for ice not being able to flow in triaxial tension.
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.

10. Lines 89-90: Near an ice cliff (or ice front) there will likely be vertical shear effects. Not so simple.
11. Lines 92-4: Consider citing relevant literature: Gao et al., 2023 (firn), Coffey et al., 2024 (temperature), Meng et al., 2024 (poroelasticity).
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)
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.
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.
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.
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.
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?
18. Line 178: Move your chosen approximate momentum equation (SSA) up earlier when defining your stresses.
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.
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.

21. Lines 195: can you provide a citation for where you get the jargon planar stress tensor?
22. Line 203: cite SSA with neglecting vertical shear stresses.
23. Equations 18 and 19: Do you mean at the crack tips? Otherwise \sigma_{zz} should be a function of z.
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.
25. Lines 230, 526: Add Bassis and Walker to this list.
26. Line 253: shelf (typo).
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.
28. Lines 267-8: Since the ocean is saltwater, the freezing point is roughly -2 C. Why do you use 0 instead of -2?
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.
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.
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?
32. Paragraph starting with line 285: good logic! Well written.
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 shift 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.

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.
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.
36. Figure 4: non-dimensionalize y-axis, change labels to be physically relevant.
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.
38. Section 4.3: Is it valid to use the Zero Stress approximation for shear cracks in addition to tensile cracks?
39. Line 442: "tuned … across the domain" for calculation F? This is unclear.
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.
41. Lines 516-18: You can remove this example and just cite them as being unclear.
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?
43. Line 545: see main point 1.
44. Can you add Wilner et al., 2023 to your table 4?
45. Section 5.6: see main point 1.1 and 1.2.
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?

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.
48. Lines 604-5: re physical basis for the Zero Stress approximation, see main point 2.
49. Lines 607-8: cite someone or provide a supplemental figure for these points about convexity of the ice front.
50. Table A2: might be useful to have equations in this table as well. Specifically for damage, defining resistive stress, etc.

References

Benn et al., 2007

Buck, 2023

Buck and Lai, 2021

Coffey et al., 2024

Duddu et al., 2020

Furst et al., 2016

Gao et al., 2023

Huth et al., 2021

MacAyeal et al., 2015

Meng et al., 2024

Nick et al., 2010

Nye, 1955

Ochowat et al., 2022

Tada et al., 1973

Van der Veen, 1998a

Van der Veen, 1998b

Wearing et al., 2016

Wilner et al., 2023