

We would like to thank the editor Evgeny Podolskiy, as well as Sarah Wells-Moran and Lizz Ultee, for their careful reading of our revised manuscript and of our admittedly long rebuttal. We greatly appreciate the time and effort they have invested in providing constructive comments that have significantly improved this work.

## Referee #1: Sarah Wells-Moran

### General comments

The majority of changes greatly improved the readability of the paper and made the conclusions much clearer. Super interesting paper that adds good insights to the current literature. Well done!

Thank you very much for this note.

### Specific comments

I appreciate the authors' clarifications in the revised manuscript that the linear viscous simulations do not represent physical glacier flow and are intended to illustrate the mechanism by which non-linearity concentrates stresses which can lead to crevasse formation. If I am understanding correctly, is this the sole purpose of the linear viscous simulations? If so, this point may be better served without muddying the waters with the linear viscous simulations in the main text. In their response, the authors clearly (and excellently) explain in one sentence the mechanism they are trying to highlight: "This rheological property leads to stress being transferred from highly deformed, and thus more fluid, ice to adjacent regions of stiffer ice, a process that is essential to understanding crevasse formation." This is well supported in the literature and could be further supported in the manuscript through highlighting how the value of  $n$  impacts the equation for dynamic viscosity (Equation A7). When  $n$  is 1, the equation reduces to  $\eta = 1/(2E_D A)$ , which explains the authors' findings that  $A$  becomes a simple scaling factor in the modeled stress response. When  $n > 1$ , a larger effective strain rate results in a lower viscosity, which then results in the mechanism the authors describe. Including the linear viscous simulations in the main text may create additional confusion rather than clarification when there is a simpler way of supporting this point. Figure 3 may be better served having the results of the viscoelastic simulations in it rather than the linear viscous simulation

We understand the reviewer's point. However, we believe that presenting the results of the linear viscous simulations in the main manuscript helps the reader to better appreciate the stress transfer associated with the non-linear, rate-thinning rheology of ice, not only qualitatively (for which, as suggested by the reviewer, a textual explanation might have been sufficient) but also quantitatively. In particular, inspection of Fig. 3 shows that the magnitude of this effect is actually very strong. Based on this argument, and considering the editor's statement, we prefer to leave the manuscript unchanged on this point.

I love figure S11 and the increased explanation of CDM vs LEFM! It really helped clarify for me why MPS and Hayhurst provided better fits than vM and Coulomb. I also really appreciated the extra explanations of what maximum principal stress being 0 meant.

Thank you for this note.

### Technical Corrections

Line 398- "the maximum principal stress shift to zero" - shifts to zero

Corrected.

## Referee #2: Lizz Ultee

### Summary

In their revised study of glacier ice failure criteria, Brondex et al have addressed most of the key points raised in previous review. I have a few remaining comments on the revised manuscript, listed below. These should be straightforward to address.

All the best,  
Lizz Ultee

### General comments

1. The authors have conducted additional viscoelastic simulations and concluded that their original Nye-Glen approach was sufficient. I agree with their decision to put the new simulations in a supplement. Their effort has made the manuscript stronger. Nicely done.
2. I agree with the authors' assessment of Ultee et al 2020 in the revised section 5.2. This revision is satisfactory. I suggest one specific revision below.

[Thank you for these notes.](#)

### Specific comments (actionable)

Lines refer to the track-changes version of the manuscript.

L242-243: "However, [most studies] consider only tensile stresses, even at depth." — I would remove 'however' and suggest rewording to "Most studies consider only tensile strength, even at depth, because ice is stronger in compression than in tension (Petrovic, 2003)." The current wording makes it unclear what your study will do versus what's usually done.

[We have reformulated the sentence according to reviewer's suggestion.](#)

L391-391: "pressurize water exerting an upward force on the roof, as visible in...(Fig S8)" —Consider also referencing main text Figure 7. You referred to Fig 7 in your response to reviewers, but not here in the manuscript text

[There is now a reference to Fig. 7.](#)

L446-447: "...this crevasse could have started to initiate earlier, ...not reported by those operating in the field" — Is it possible the crevasse could have initiated at depth and then erupted to the surface? That mechanism would align with discussion in Colgan et al. 2016.

[What we intended to explain here is why the surface stress peak associated with the crevasse at the upstream end of the AA' profile on 18 October 2010 reaches  \$\sim 165\$  kPa, while all other stress peaks associated with crevasses are in the range 100–130 kPa at the end of the drainage operation. A plausible explanation is that this crevasse began forming earlier, as soon as the stress in this area first reached the 100–130 kPa threshold, which, according to Fig. 6, occurred in mid to late September 2010. Therefore, to make this point clearer, we have revised the original sentence as follows: "However, this crevasse could have started to initiate earlier in the pumping operation, when peak stresses were lower \(around mid September 2010, Fig. 6\), although this..."](#)

L487-489: "...adopting an inappropriate failure criterion in a CDM framework can lead to large errors in the modeled velocities..." — This is an interesting conclusion, which increases the need for work like yours. Consider highlighting and expanding on it in discussion Section 5.3, "What failure criterion?"

[Section 5.3 now starts as follows: "Results presented in Sect. 4.4 highlight the strong feedback of damage on ice viscosity, which makes the choice of an appropriate failure criterion critical in a CDM framework."](#)

L570: "the geometry of the [Skaftá] cavity is poorly constrained" — I would suggest revising this to indicate that the limitation is with the simplistic geometric assumptions in Ultee et al. 2020, rather than the observational

constraints. The cavity geometry was actually quite well constrained by repeat radar and by a borehole drilled at the center in 2015. I include figure 33 from Gudmundsson et al. 2018 showing the geometry. The reason to test a range of values in Ultee et al. 2020 is because we were assuming a radially symmetric plate or beam, and the actual ice thickness was not constant across the cavity roof, as you can see from the Gudmundsson figure. So we ended up with a range of simulations sampling different idealized cases, which as you correctly note, directly affected the load on the cavity roof... none of which are as faithful a representation of the observed geometry as what you have done for your case.

Thank you for this precision and for the Figure provided. We have reformulated as follows: "Because the considered plate is radially symmetric, whereas the actual ice thickness was not uniform across the cavity roof,...".

### **Specific comments (no action requested)**

L211-215: "These criteria...the equivalent stress, that is, a scalar quantity that..." — Good revision. This makes the framing of this section and related figures clearer. (No action requested here, just a positive note.)

L316-318: "...these timesteps are longer than the Maxwell time expected...and are therefore consistent with our choice [of focus]" — Yes, good revision. This addresses my comment about Maxwell time motivation from the previous review. (No action requested here, just a positive note.)

L636-638: "Although this may be an acceptable approach..." — Yes, good revision. This addresses my comment from the previous review about making clear the goal and applicability of your work. (No action requested here, just a positive note.)

We thank the reviewer for these three comments and for the suggestions provided during the previous round of review, which indeed helped improve the quality of our manuscript.