

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.

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.
- 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.
- 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.
- 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?"
- 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.

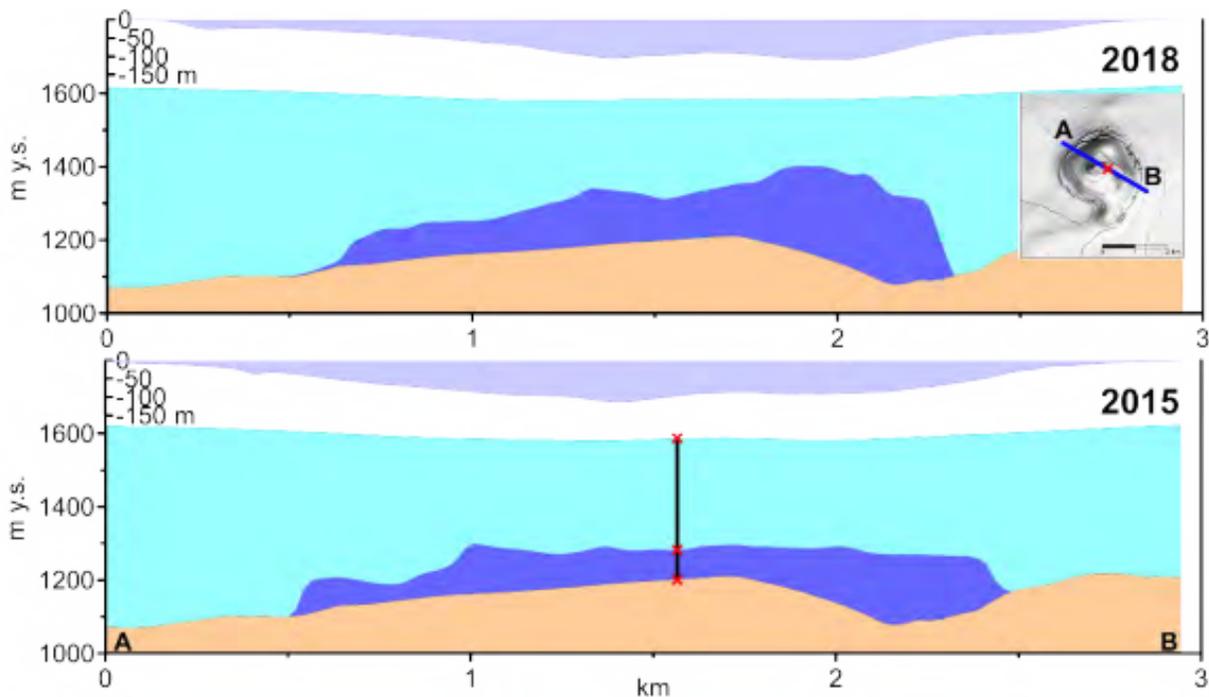


Figure reproduced from Gudmundsson et al 2018; caption automatically translated by Google Translate: Cross-section of the glacier for the 2015 and 2018 runs from A to B according to ice telescope and GPS measurements, see location in the thumbnail on the right. For comparison, the elevation change (upper graph of each image) in the corresponding run. All axes (elevation, distance and elevation change) are in the same proportions. The line in the middle of the lower image shows the location of the Icelandic Meteorological Office borehole and the measured height of the bottom, ice-water and surface layer (obtained from Þorsteinn Þorsteinsson) four days after the ice telescope measurement in the spring of 2015 (the location of the profile and borehole is in the thumbnail). The local discrepancy between the water bubble thickness and elevation change can be caused by various factors, but in general it can be expected that the decrease is less near the caldera center than the water bubble thickness indicates due to ice flow from the sides of the caldera inwards towards the center. A greater decrease in the center of the caldera during the 2015 run than the water bubble thickness the previous spring could indicate that there was melting at the bottom from spring 2015 until the first elevation map after the run was measured (October 10).

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.)

References in this review

Guðmundsson, M, Magnússon, E, Högnadóttir, T, Pálsson, F and Rossi, C (2018). Hættumat vegna jökulhlaupa í Skaftá: Skaftárkatlar, saga og þróun 1938–2018 [Hazard assessment for jökulhlaups in Skaftá: The Skaftá Cauldrons, history and development]. Technical Report Rep. RH-16-2019, Institute of Earth Sciences, University of Iceland, available online at https://www.vedur.is/media/vedurstofan-utgafa-2018/VI_2018_017_rs.pdf

Petrovic, J.J. (2003). Review: Mechanical properties of ice and snow. *Journal of Materials Science* 38, 1–6. <https://doi.org/10.1023/A:1021134128038>