Summary

The selection of an appropriate fracture criterion for glacier ice is important for simulations of glacier hazards, including local collapse events and the larger-scale contribution of calved ice to global sea level rise. Brondex et al. make opportunistic use of a controlled cavity drainage as a natural experiment for ice damage criteria. They assert that a Hayhurst or Maximum Principal Stress criterion is the most effective for simulating crevasse initiation, while Coulomb and von Mises criteria are less effective. The authors work solely with a nonlinear viscous rheology and therefore do not directly simulate crevasse initiation, nor propagation, relying instead on a continuum damage mechanics framework to approximate the effect of crevasses. This methodological choice sets up an uncomfortable tension between fine-scale mechanics and applications to ice-sheet-scale modeling.

Overall, the manuscript is well written and organized. The mathematical frameworks in the Appendix are described well. However, the framing and motivation of some arguments is a bit muddled. The manuscript would be stronger if revised to be more focused; see General Comment 1, below.

Finally, I apologize to the authors for their long wait for review comments. The review request arrived while I was in the field and I did not see it for several weeks afterward — my fault. Thanks everyone for your patience in this process.
All the best,
Lizz Ultee

General comments

1. It seems that the authors want to find which failure criterion is most suitable for simulating crevasse initiation in short-term, local settings. However, they use a continuum approach, which is commonly used for large-scale ice sheet modeling, and by definition inconsistent with the initiation and propagation of crevasses. As far as I can tell, there is no explicit crevasse "initiation" here. See comments on L452, L493-496.
    - I suggest the authors explain what crevasse "initiation" means in their framework. For example, do they simulate damage evolution from 0 to non-zero values? Does this require them to "seed" (i.e. intervene to initiate) damage, or does it arise from the model?
    - I suggest that the authors choose either
        - clarifying that their analysis pertains to best practices for short-term, local simulation of fracturing ice bodies using continuum methods, or
        - elaborating much more on how their results might be translated to simulating "bulk calving" in large-scale ice sheet models. See comment on L505-507.
2. The authors briefly consider elastic deformation, then move on with Nye-Glen viscous simulations. I think viscoelastic simulations may be more appropriate to fit the observations they present — see comment on L341-342. However, I am not sure whether that is in line with the goals of the manuscript.
3. The authors' commentary on earlier works goes a bit beyond what their quantitative analysis can support. I suggest that the authors carefully consider the goals of their manuscript and revise accordingly.
    - The authors discuss Ultee et al 2020 — previous work of mine — for several paragraphs. While I appreciate their attention to my work, and I don't disagree with some of their points, their methods do not support a direct comparison. For example, Brondex et al.

consider linear elastic, linear viscous, and Nye-Glen viscous cases; my work mainly considered linear viscoelastic rheology, which is not treated here. Further, Brondex et al. use a more computationally expensive approach and therefore cannot sample as wide a parameter space as we did in Ultee et al 2020 (especially w.r.t. the Young's modulus and the Maxwell time). As a result, the stress thresholds Brondex et al. produce cannot be compared directly with mine. See comments on L436-437, L449-451.

  - If a direct comparison is the goal, then the methods must be adjusted to support it.
  - If a direct comparison is *not* among the goals of the manuscript, then I suggest the authors revise to bring their commentary in line with their goals.

- Note that similar criticism may apply to the authors' discussion of other papers; I am simply the most familiar with my own work.

Specific comments (line by line)

- L69-70: Possible scale problem here? The dimensions of the entire glacier (a few hundred meters wide/long) are on the order of crevasses that form on ice sheet outlet glaciers, and as the authors note later, roughly the same size as the large cavities at Skaftá. The stresses that can be produced in this setting are necessarily much lower.
- L74-82: Very cool setting for a field experiment! Nice find and nice description.
- Table 1 and throughout: typo, "Hayurst" should be "Hayhurst". Repeated in header of section 3.2.4 and elsewhere in the text.
- L199, Eqn 1: What is $Ice \sigma_{III}$? Possible failure of a subscript "Ice" somewhere?
- L215-216: It is sensible that most glacier and ice sheet studies do not consider compressive failure, because ice is much stronger in compression than in tension (Petrovic, 2003).
- L254-256: "We argue that using lower Young's modulus values to fit observations in a purely elastic model may overlook the viscous component of the observed deformation (see Sect 5.1)." — Well, sure, but this goes both ways. Using high values of the Young's modulus, as the authors do in Sect 5.1, will tend to underestimate the Maxwell time and thus overemphasize the role of viscous deformation in a material assumed to be viscoelastic.
- L280: "All pumping sequences are simulated with a one-day timestep, while the periods in between are simulated with a five-day timestep." — Here it would be helpful to address your assumption about the Maxwell time up front. One- to five-day time steps are longer than the Maxwell time you use, which motivates a simulation that includes only viscous effects on these time scales. Consider moving Figure 8 earlier?
- L302-303: Please split this "For example, …" into two sentences. The "A (resp. B) is X (rest. Y)" sentence structure is hard to parse.
- L336-340: Could you clarify what kind of uplift you see in the data? I was imagining uplift outside the edges of the cavity, as when an elastic or viscoelastic beam or plate has its center pushed down and its outer edges lift up (see e.g. Banwell et al 2013 for ice shelf case)…but I wonder if you're actually referring to uplift from the cavity refilling with water and floating the overlying ice? In the first case, it is expected that some of the stakes would be subsiding while neighboring stakes show uplift.
- L341-342: I had to read it a few times, but okay, yes, I see that the elastic displacement being two orders of magnitude too small is an argument to include viscous effects. However, I wondered about linear viscoelasticity — an initial elastic drop followed by continued viscous settling. The Nye-Glen viscous model gets the right order of magnitude deformation, but we see in Figure 7 that its velocity during the pumping operations is generally too high. I

wondered if this was a result of instantaneous stresses that immediately drive fast creep, with no elastic phase to take up the highest stresses…and if so, whether a true viscoelastic model might do a better job capturing observed cavity roof velocities.

- L389-390: “[MPS] does not offer quantitative insights, such as crevasse concentration to be expected locally. In the framework of CDM, this aspect is quantified through the damage variable D…” — A bit confusing. I typically think of damage in the vertical dimension, analogous to crevasse penetration, reaching 1 when the ice column can no longer support any load (Borstad et al., 2012). By contrast, the expected crevasse spacing in the horizontal dimension(s) is a function of the ice thickness, depth of existing crevasses, and material properties of the ice (reviewed in Colgan et al., 2016).
- L404: “…the subsequent slowdown associated with the cavity refill in late spring/summer 2011.” — So the cavity roof was still subsiding (negative Vz) even during refill?
- L416-418: “In contrast, peaks of $\sigma_{eq, vM}$ …” — Rephrase for clarity? This sentence is hard to parse.
- L436-437: “[Ultee et al.’s 2020] estimate relies on the assumption that [deformation] is predominantly elastic.” — I wouldn’t say so. We used a linear viscoelastic model for which the best-fit Maxwell time is on the order of days, so it follows from the setup of that model that deformation was predominantly elastic. However, we also considered Glen’s law displacement and reached similar conclusions (see Discussion section).
- L443-444: If you use a high Young’s modulus, of course you are going to get short Maxwell times. It would be cleaner to argue here that previous authors deduced low values of Young’s modulus from purely elastic fits to observations, and those fits are artificially low because they did not account for any creep. This argues for a viscoelastic framework, which is consistent with higher Young’s modulus and shorter Maxwell times (Reeh et al. 2003).
- L449-451: “…we argue that Ultee et al. (2020) significantly underestimated the viscous contribution to the observed surface deformation, leading to an overestimation of stress and, as a consequence, an overestimation of ice tensile strength.” — Contrary to this assertion, the parameter space we tested corresponds to Maxwell times of order 0.3-4100 hours (see Ultee et al. 2020, Table 1). We address the overestimation of stresses in our Discussion section, together with many earlier authors who have pointed out that the instantaneous elastic stresses produced in a viscoelastic model are upper bounds on what would be produced in nature (e.g. MacAyeal & Sergienko 2013). Thus, this point is not entirely relevant, and not particularly useful for your manuscript, at least as I understand it. If you wish to make this point, I suggest revising to support it better and to align it more clearly with the goals of the manuscript. See General Comments, above.
- L452: “[Our results] suggest that crevasse initiation in initially intact ice is governed by viscous processes.” — This doesn’t follow from the analysis you’ve presented. There is no crevasse “initiation” in the simulations presented.
- L462-465: It is worth questioning whether any of these methods are valid, since none include explicit crevasses, and the formation of crevasses would be assumed to concentrate (and thus “relieve”) stress from surrounding areas (Rice 1968; Weertman 1973). I include my own past work in this criticism. Anyway, if you raise this point, it would be good to comment also on how it applies to your methods.
- L475-482: Consider commenting explicitly here on whether failure criteria should be based on Cauchy stress or deviatoric stress. This is a common point of confusion in ice sheet applications that I have seen in the literature. It seems you are alluding to this distinction and could communicate more clearly by naming it explicitly.
- L493-496: The notion of crevasse *initiation* being governed by viscous processes seems muddled to me. Crevasse initiation is a fundamentally discontinuous phenomenon, no? A

crevasse initiates when a discontinuity—the crevasse—forms in a medium formerly approximated as continuous. Consider reframing for clarity.

- L505-507: In my view, most people accept that bulk calving will be implemented differently from local-scale collapse events. Perhaps your manuscript would do best to focus more directly on the latter?

Figures

- Fig 1: “Surface Energy Balance station (pink triangle)” — Not seeing a pink triangle. Is it the one nearest the cavity on panel (a)? That one appears orange to me, the same as the other two triangle markers.

- Fig 6: Please refer back to where each equivalent stress is defined in Section 3. I was initially very confused about why MPS was the only plot that showed compressive stresses in the center of the roof, where we are quite certain the stress should be compressive. But in fact, this is not giving local effective stress but rather the value of the maximum principal stress, the Hayhurst stress, etc., which would be compared with the failure threshold. Right?

- Fig 7: “Blue dots indicate mean vertical velocities derived from stake measurements, computed between two measurement dates” — Which two measurement dates? What’s the temporal resolution here?

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