Review of "Sharp contrasts in observed and modeled crevasse patterns at Greenland's marine terminating glaciers"
by Enderlin and Bartholomaus, for *The Cryosphere Discussions*

Summary of manuscript

This iteration of the Greenland airborne crevasse depth analysis acknowledges the limitations to the observations. The analysis is mostly the same as presented before, with additions to the discussion and changes to the conclusions. The new conclusion – that advection and/or blunting influence crevasse depth but are not included in the usual simple models tested here – is reasonable, but strangely does not appear in the abstract. The revision still does not incorporate the known uncertainties in the crevasse depth dataset, which therefore still looks like noise to me. I suggest that the authors remove too-narrow crevasses (where the ATM cannot sense the tip of the V) from their dataset – perhaps I should have been more specific, in previous reviews, that this should have been done. This should be simple to implement using the data the authors already have.

Overall, the paper is improved from its earlier versions, but suffers from new issues of clarity and organization, and lingering science problems.

Major comments

1. The description of the modeling analysis is convoluted and spread across multiple sections. Equations are presented in the Introduction and built on in the Methods. A formula for the LEFM model is never given; I think it is intertwined with the Nye formulation, but I am not sure. A standard approach would define modeled crevasse depth $d_X =$ for each model X in the Methods. This would make it easy to compare the factors and adjustments that each model incorporates.

2. The abstract ends with a punt: "We therefore suggest that additional analyses of the controls on crevassing are performed prior to implementation of either crevassing model within ice sheet models." It seems that the authors land on advection (line 412) as the source of the misfit between estimated and modeled crevasse depth, although they do not quantitatively explore it. Adding the advection hypothesis to the abstract, and supporting it with a back of the envelope calculation in the paper itself, would strengthen it.

    - Advection estimate: Figure 5a, Nye Minimal or Nye Critical model, shows compressive regions up to $\sim$1 km long. With estimated flow speeds 1 km/yr, this means that crevasses that form upstream must persist for $\sim$1 year on their transit through compressive areas. This is an entirely reasonable lifetime for a crevasse, even on an outlet glacier. This could be supported with observations (e.g., tracking in satellite images) or cited (I am not sure of a reference) or left as a statement (which I would believe).

3. Deformation enhancement factor: The name is misleading. The term D is presented as a reduction in viscosity due to fractures, roughly in line with Borstad et al. (2016). However, Borstad did a full L-curve analysis to balance smoothness in their D field against the misfit

that D attempts to minimize. What is presented here, on the other hand, is pure misfit (Equation 4). It is disingenuous to calculate misfit and name it something else, effectively ascribing all of the misfit to a particular process. The authors do seem to acknowledge this: "such detailed tuning is neither physically motivated nor practical" (line 230) and "the optimal deformation enhancement and water depth tuning parameters found here have no physical basis" (line 387), but they continue with their analysis anyway. Unfortunately, this sort of dismissing of warning signs is a recurring pattern in the analysis.

4. A priori information on crevasse depth: The authors fold their crevasse depth observations into the LEFM model (line 251), which they find improves the agreement with the data. Of course it does. However, they need not (and should not) have done this, as the LEFM model is an analytic function of crevasse depth with no real complications. Even the $F(\lambda)$ term, which is the apparent source of complication where the authors therefore inserted their observed crevasse depths, is a simple polynomial function of crevasse depth (see van der Veen 1998, Equation 6). This is easily solved for crevasse depth (especially when crevasses are shallow compared to the ice thickness), as with the other simple models used.

5. Contradictions in conclusions: The authors state that "Broadly, we see no clear, consistent patterns in either the crevasse density or depths as one moves from the glacier interior towards the glacier terminus" (line 256). This is in conflict with the manuscript's title and with other statements in the paper. For instance, they say that the models "fail to reproduce realistic along-flow variations in crevasse depth" (line 354) and refer to "the observed along-flow increase in maximum crevasse depths at over half of our glaciers" (line 398) and to "large-scale spatial patterns in crevasse depths" (line 402). I am left confused as to whether the authors find along-flow trends or patterns, or not.

6. Figure 4 shows the continuing limitations of these depth estimates. The maximum crevasse depth observed in along-flow bins approaches the modeled depth as the bins get larger (e.g., panel h). (A minor note: The caption should say which model (minimal Nye) was used.) It seems likely to me that the deepest crevasses (panel h) are also the widest crevasses, which gives the ATM a better look and increases its chance of observing the true point of the V. I believe the authors have the data to test this. If so, they could (and should) exclude crevasses whose width-to-depth ratio is smaller than what the ATM can observe (see my first review). Throwing out meaningless data would undoubtedly improve the analysis.

Minor comments

Lines 12–14 – This sentence says that, with a little embellishment, the observations agree with themselves.

Line 131 – This notation is unusual and informal and should be fixed.

Line 167 – The mean depth difference between scans is reported as a negative value. It should be positive, which calls into question how this difference was calculated. The given explanation for the difference between scans (changes in crevasse wall slopes) is unlikely – crevasse geometries should not change meaningfully over a few hours in plug flow regimes. I think it is more likely that this relatively small depth difference arises from how the crevasse depths were calculated from oblique observations of the crevasse walls.

Line 275 – Strain is incorrectly defined here as the spatial integration of strain rate. This may be a typo (easily fixed) or, if the analysis was actually performed this way, a bigger flaw that would affect the results.

Lines 300, 320 – “oscillation” requires a period or some regularity, which is not seen here. A better description would be “variation” or “fluctuation”.

Lines 380–381 – “Viscous” and “ductile” are not antonyms. “Brittle” is the opposite of “ductile”.

Lines 383–384 – I agree that large spatial gradients in the water table are unrealistic. But the water table shouldn’t follow “regional patterns in meltwater runoff”, especially at the 100-m scale; instead, it should follow basal hydrology, which is variable across short length scales, even on outlet glaciers. It’s neither feasible nor useful to compare the crevasse water depths to patterns in basal water pressure, but I thought I could point out this error.

Equation 1 – This should be written as $d_{modeled} = ...$ and the depth should be named d_{LEFM} or some other name to distinguish it from the other $d_{modeled}$ (Nye model) in Equation 2. All equations should be moved out of the Introduction section.

Equation 2 – What values were used for $\dot{\epsilon}_{crit}$ and A? I believe these choices are described in the Methods, so this would be another benefit to moving equations to Methods.

Equation 5 – Use of Glen’s Flow Law on crevassed ice is problematic because opening of the crevasses (which is reflected in the measured strain rates) relieves stresses in a way that the continuum-based law does not capture. (This is related to the deformation enhancement, D, that immediately precedes this section of the manuscript, so I’m surprised the authors didn’t catch, or at least comment on, this.) Thus, the calculated stresses (Equation 5) are higher than the stresses actually sustained in the ice.

Figure S1 – The thick lines and high volume of data on each panel makes the data hard to see.