

Interactive comment on “Poor performance of a common crevasse model at marine-terminating glaciers” by Ellyn M. Enderlin and Timothy C. Bartholomaus

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Response to Reviewer 2: We thank the 2nd reviewer for their careful review and for their helpful suggestions in improving the clarity and impact of our manuscript. We appreciate that the reviewer finds our work interesting, novel, and compelling, and is convinced by our main conclusions: that local strain rates are unrelated to the pattern of crevasses found along 19 Greenland outlet glaciers. This second review also notes some of the foundational work we cite that came to similar conclusions, albeit with a more modest dataset (that by Mottram and Benn, 2009), and we thank the reviewer for drawing our attention back to the work of Colgan et al., 2016, and van der Veen, 2009.

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We intend to reference their discussion of crevasse depths and their V-shapes in our revision as well as Liu et al., 2015 (doi:10.1007/s11430-013-4796-x), who estimated crevasse depths in Antarctica using ICESat data and the assumption of V-shaped geometries.

As with the first-submitted review, the second reviewer's dominant concern (specific comment 1) is that the V-shaped bottom estimates are likely to be minimum bounds. In reality, fractures likely extend below the bottom of V-shaped crevasses, although they are nearly impossible to measure over any kind of significant spatial scale. This is an important point that we plan to make more forcefully and directly in our revision. As pointed out by the reviewer, we already discuss the implications of the V-shaped assumption to some extent on lines 110-124, but we will expand on this discussion to make the potential under-estimation of fracture depths more explicit. However, it is important to note that even if fractures penetrate below the 'open' portion of crevasses, this caveat does little to alter the main thrust of our work—that the Nye model fails to reproduce observed patterns of crevasse depths. For example, in our Figure 4b-h, we show the mis-match between our observed crevasse depths and modeled crevasse depths. One of the most important messages of this work is that even if we augment the crevasse depth model so that the average modeled depths are similar to observations, the spatial variations in observed depths still cannot be explained by the model. Modeled depths are not predictive of observed depths and no plausible modification of the observed crevasse depth data will bring the observations into accord with the model. We will modify the manuscript so that the potential under-estimation of crevasse depths due to the assumption of V-shaped geometries is more clear and to emphasize that this potential under-estimation does not alter our primary finding: crevasse depth models that rely only on local strain rates cannot reproduce observed crevasse depths.

In addition, the second reviewer asks for clarification at several points, and suggests a number of grammatical modifications. We expect that our resolution of these points will increase comprehension and impact. Specifically, with regard to the points of clarifi-

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cation, regarding specific comment 2, these falsely identified crevasses are crevasses expected to occur in the model, but that are absent from the observations. Regarding comment 3, at this point, we only had intended to reinforce the fact that the Nye model does not predict crevasses in compressional zones, as the reviewer writes. Regarding comment 4, Ingia Isbrae did undergo widespread increase in strain rates associated with its terminus retreat. We propose to clarify and expand on each of these points in our revision.

In conclusion, we appreciate the care and interest expressed by the second reviewer. They have raised important points that do not challenge the central conclusions of our study, but will improve clarity of the manuscript when addressed in revision.

Response to the Editor and both reviewers: Dear Editor, We have now received two constructive reviews of our manuscript. Both reviewers raised important methodological questions about our ability to measure the bottom of a crevasse by measuring or extrapolating the bottom of its V-shaped surface expression. We feel that this concern is an important one, and we look forward to addressing it through the addition of modest qualifying text. However this concern does not challenge our central conclusion: crevasse depth models that rely only on local strain rates cannot reproduce observed crevasse depths, even when tuned to minimize the bias in crevasse depths. As we and the reviewers have pointed out, a more limited study of a glacier in Iceland has come to a similar conclusion (Mottram and Benn, 2009), however, to quote reviewer 2, " the persistent use of the Nye model as a fracture criterion suggests that the ice modelling community has yet to absorb this message!" Local stresses, associated with local strain rates, are not correlated with locally observed crevasses. The Nye model is insufficient to reproduce crevassing, and therefore it is insufficient to model iceberg calving either.

In a revised submission, we are confident that we can address this important concern without dulling the impact of our paper. We also look forward to incorporating suggested changes to the text, clarifying the places where our intent had been unclear,

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and reformatting several of the figures.

Thank you for your consideration and we look forward to receiving your direction regarding the next steps of review.

Sincerely, Ellyn Enderlin and Timothy Bartholomaus

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-128>, 2019.

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