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Spatial and temporal patterns of fracture in confined landfast sea ice

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Response to Anonymous Referee #2

We would like to thank Referee #2 for providing constructive comments, which have helped improve the quality of the paper. We present below our detailed responses to the comments in **blue**.

REVIEW of "Spatial and temporal patterns of fracture in confined landfast sea ice" by Loewen et al.

DECISION: Minor revisions

GENERAL COMMENTS

This manuscript presents an analysis of sea ice fractures in Admiralty Inlet, Nunavut using remote sensing data and Inuit Knowledge. This is an admirable study that aims to work with both scientific and Inuit Knowledge and learn something from both. The study highlights key diagnostic features of sea ice fractures and associations with local morphology that should be used to guide future theoretical and modelling research. The paper is very well written and readable, and aside from a few minor comments below I recommend publication in The Cryosphere.

We thank the reviewer for the constructive comments.

COMMENTS

Eq. 1, Lateral and bottom stresses. The study shows that fractures tend to occur near promontories, inlets and river mouths and that these geographic features may concentrate ice stresses at certain times of the year leading to preferential fracture formation there. It would be nice to illustrate how these features are represented in tau_l and tau_b (the lateral and bottom stress terms in equation 1). I do very much like framing the study around this equation as one can very clearly see how an imbalance of external forcing (the right hand side) would lead to ice stress (the left hand side). In this equation the air and ocean stresses are clear (and illustrated with an equation showing proportionality to the square of wind speed/ocean current) and the role of sea surface slope is obvious. However, the bottom and lateral stress terms are explained only through some qualitative textual descriptions. It would be useful to know how these scale e.g. tau_b with water depth? tau_l with some measure of coastline complexity? I know this is not simple to do as there are no

unique parameterizations for these stresses, but given that your main argument hangs on this (i.e promontories, inlets and river mouths lead to bottom/lateral stress) it is important to see more explicitly the role that coastal morphology plays in the scaling of these stress terms.

Thank you for this suggestion. Since basal and lateral stress terms for sea ice are not as well constrained as air and ocean stresses, we didn't describe these processes in detail, but we agree that it would be worthwhile to explain more explicitly how coastal morphology plays a role in scaling these stress terms. In summary:

Current formulations of basal stress (Lemieux et al., 2015, 2016) describe basal stress resulting from grounded ridges as a function of water depth and keel depth, which in turn is parameterized from the ice thickness distribution. In Admiralty Inlet, water depths increase rapidly away from shore, such that any grounded ridging will occur close to the shoreline and can therefore be treated as a contribution to the lateral stress.

Lateral stress has been formulated as a function of the length of the coastline interacting with the ice and a 'lateral static drag coefficient' which is not currently well constrained physically and is used as a tuning parameter in Liu et al. (2022). Presumably, a coastline that has more undulations would have a higher coefficient than a smooth coastline and the presence of grounded ridges near the coast would also increase this coefficient. However, further work is needed to understand these physical mechanisms.

We will add this text to Section 2 to explain how the coastal morphology relates to ice stress.

Lines 261 and 389. "2 air temperature" should be "2 m air temperature".

Thank you for identifying the missing units in these sentences.

Line 315-316. "…the nearest shoreline point or shoreline point that was deemed to be near a promontory." This statement is unclear… should any "shoreline point that was deemed to be near a promontory" be simply a subset of all "shoreline points"? So it's not clear to me how this statement is different from simply saying "…the nearest shoreline point."

Thank you for identifying this ambiguity. This sentence will be clarified to explain that we did two separate calculations: 1. Calculating the distance from each fracture endpoint to the nearest shoreline point (any shoreline point), and

2. Calculating the distance from each fracture endpoint to the nearest promontory point (subset of all shoreline points).

Line 347. Define “secondary inlet mouth”.

We will clarify ‘secondary inlet mouth’ as smaller inlets that feed into Admiralty Inlet (e.g., Elwin Inlet, Strathcona Sound, etc.).

Eq. 8. Define tau_i. Is this different from tau_k in Eq. 9? If it is wind or currents are these directed along or across the “length” of the ice?

Thank you for identifying the discrepancy between tau_i and tau_k – these should be the same variable (where ‘i' indicates this may be a wind or ocean drag term). Since we use the König and Beatty (2011) analytical 1D model, it is assumed the winds/currents are directed along the length of the ice (i.e., perpendicular to the fixed shoreline).

Lines 427-428 and Fig. 7. Are these peaks really significant enough to call them out quantitatively in the text? If so, why not also mention the peaks at 9 and 16 km? Possibly this is just a broad distribution with no true “peaks” but a small sample size and noise data.

We will alter text a line 427 to “distance intervals of 2-4 km, 7-8, 9-13 km, and 16-19 km between fractures occurred most frequently across all years examined (Figure 7)” to better reflect the data presented. It is true that there may not be any ‘true peaks’ as the reviewer suggests, but we have no evidence of this currently so we do not wish to speculate at this time.

Fig. 10. There is no blue line in panel (a) despite the figure caption referencing one.

The figure caption will be updated to remove the reference to scaled values for calculated tensile strength to ensure consistency.

Fig. 11b. I don’t understand what has been done here… Is this the difference across years, for each day of the year? If so which years are differenced, you simply mention a range (2018-2023). Is it 2023 minus 2018? Also, why split out the positive and negative differences into different distributions? Really it is a single sample of many Delta T and so should be represented by a single distribution (for each month). Otherwise it looks like there is both warming and cooling while in reality the mean change may be zero.

Figure 11b represents all the daily average temperature differences (lagged difference of the time series, e.g., (T_{avg}^{day1}-T_{avg}^{day0}), (T_{avg}^{day2}-T_{avg}^{day1}), etc.), for all years, split by month. These details will be added to Section 6.2.2.

Warming and cooling events are split because, as the reviewer mentioned, if you plot them together the mean change is close to zero, but here we are trying to show comparatively how many warming vs cooling events occur in each month, and whether certain months have stronger warming or cooling events than others.

For example, in December, the average warming event is warmer (in absolute value) than the average cooling event, but December also experiences the most extreme cooling events of the year. If the daily average temperature differences were plotted from e.g. -15°C to +15°C, it would be harder to see this comparison.

In response to the other reviewer's suggestion, we will use split violins to more clearly show the differences between warming and cooling for each month.

Fig. 12c,d. Axis labels say "width" while figure caption says "length".

This figure caption will be updated to more accurately reflect the figure.

Lines 500-5001. You ignore the join effect of currents and wind. However, given that you know that currents (certainly tides) and winds will co-occur I think it is important to demonstrate their joint effect. This would provide a more realistic minimum wind/current speed required to produce fracture.

An additional panel will be added to Figure 12 to show combined effect of wind and current speeds, assuming a tensile strength of 5 kPa.

References:

Lemieux, J.-F., Tremblay, L. B., Dupont, F., Plante, M., Smith, G. C., & Dumont, D. (2015). A basal stress parameterization for modeling landfast ice. *Journal of Geophysical Research: Oceans*, *120*(4), 3157–3173. https://doi.org/10.1002/2014JC010678.

Lemieux, J.-F., Dupont, F., Blain, P., Roy, F., Smith, G. C., & Flato, G. M. (2016). Improving the simulation of landfast ice by combining tensile strength and a parameterization for grounded ridges. *Journal of Geophysical Research: Oceans*, *121*(10), 7354–7368. https://doi.org/10.1002/2016JC012006.

Liu, Y., Losch, M., Hutter, N., & Mu, L. (2022). A New Parameterization of Coastal Drag to Simulate Landfast Ice in Deep Marginal Seas in the Arctic. *Journal of Geophysical Research: Oceans*, *127*(6), e2022JC018413. https://doi.org/10.1029/2022JC018413.