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Spatial and temporal patterns of fracture in confined landfast sea ice

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Response to Anonymous Referee #1

We would like to thank Referee #1 for providing constructive comments, which have helped improve the quality of the paper. We present below our detailed responses to the comments in **blue**.

General comments

The manuscript Spatial and temporal patterns of fracture in confined landfast sea ice by Loewen et al. investigates fracture patterns in the Admiralty Inlet of the Canadian Arctic Archipelago. The manuscript builds on a fracture dataset developed in the authors' earlier study. This manuscript summarizes the spatial and temporal aspects of the fractures, and examines potential drivers of these. Local knowledge is incorporated with scientific methods, the study has clear motivation and user groups that can benefit from the results. In these respects, the manuscript is an excellent continuation and application of the previously developed dataset. There is novelty both in the focus on a confined landfast ice environment and in the integration of local knowledge into the analysis. The methodological choices are generally well justified, and limitations are appropriately discussed.

However, the manuscript could use a bit of condensing, and I have several minor comments and suggestions that are detailed below. Overall, I consider the manuscript a valuable contribution that improves our understanding of the spatial and temporal behaviour of fractures in confined landfast sea ice.

> **We thank the reviewer for the constructive comments. We have identified areas where the manuscript could be condensed in response to some of the detailed comments mentioned below.**

Specific comments

The study is partly motivated by the needs of the local people, and Inuit knowledge is heavily incorporated in the early sections. It would make sense to include a concluding sentence in the conclusions as well.

> **Thank you for this suggestion. Below are our suggested modifications to the conclusions (changes italicized and underlined):**

In this paper, we examined the spatial and temporal patterns of fractures in seasonal landfast ice in Admiralty Inlet, Nunavut, a deep water *fjord by bringing together Inuit Knowledge, satellite and in-situ observations, and academic understandings of sea ice physics*. Similar patterns are likely to occur in other confined waterways in polar regions such as the Canadian Arctic Archipelago, *with implications for local communities and Arctic shipping and industry.*

....

While ongoing changes in the Arctic climate are expected to reshape the conditions under which fractures form, the results of this study summarize the current understanding of fractures in confined landfast ice and identify priorities for further observations and modelling to answer outstanding questions. *Developing collaborative research partnerships have the potential to bring complementary expertise to address challenging scientific questions benefiting all partners.*

Section 3 effectively includes the local sea ice terminology, but adds cognitive load to the reader. To ease reading, consider repeating the English translation in parentheses more often, even if it feels a bit repetitive, especially when the word has been introduced a paragraph or more ago. This would be particularly helpful in Section 7.4.

The English translation of the terms *aajurait/aajuraq* and *nagguti* will be repeated when re-introduced in new paragraphs in Section 3 and Section 7.4 to remind the reader of the definitions of these key features which are referenced several times throughout the paper.

In Section 5.2.4, were all the manually identified points identified just by looking at Sentinel-2 imagery, or was additional data used, particularly for the river mouths?

All 'special' shoreline points (promontory points, inlet mouths, and river mouths) within the area of interest were identified manually using Sentinel-2 imagery. Various indices of curvature were tested in an attempt to automate the identification of promontory points, but none were found effective for accurately identifying the features we were trying to capture.

Natural Resources Canada does have a river database (National Hydro Network), but it is difficult to tell from this dataset alone which rivers begin to flow when ice is still present. If all river mouths identified from this dataset were used in this analysis, nearly all shoreline points (particularly along the western shore) could be considered river mouths. Instead, we identified river

mouths as rivers that could be seen to cause visible melting in the nearshore ice from satellite imagery in June and July in several years.

We will add a sentence to explain why other datasets were not used to identify the 'special' shoreline points in Section 5.2.4.

How representative are the years in 1959 to 1966 and 1970 of modern sea ice thickness conditions? At line 222 it is stated that the ice is thinning in the Admiralty Inlet. Are there Canadian Ice Service thickness data for other months? If so, you could compare those with the SmartQAMUTIK data (e.g. for January), to assess how applicable the old data is of the current conditions in this location.

Thank you for the suggestion. We examined monthly average ice thicknesses from the Canadian Ice Service (CIS) ice thickness data from 1959-1966 and 1970 and average monthly ice thicknesses are 15-30 cm thinner now (2021-2022) than 50-60 years ago. Only the December ice thickness value came from the older CIS dataset since SmartICE data were not available for this month, so we will reduce this value by 22.5 cm (mid-point of 15-30 cm) to account for recent reductions in ice thickness. This modification will be explained in Section 5.3.1.

In Section 6.1.4, what could explain the formation of east shore fractures in both early and late season, while west shore-originating fractures form mainly in late spring? Related to this, Section 6.1.5 on fracture growth appears somewhat qualitative (which the authors acknowledge). Given the apparent uncertainty in tracking the evolution of individual fractures, the authors could consider merging this section with another, and making it shorter. Table 3 might be more appropriate for the supplementary material, if retained. In addition, the table presents precise percentages despite the apparent uncertainties of the method. E.g. fractures with initial category short (east) end to the category short (west) and vice versa, highlighting potential ambiguity in how fracture continuity and growth direction were determined.

These are important points, and it is true there is some uncertainty in tracking the evolution of individual fractures due to the nature of the dataset.

One hypothesis for fractures originating along the western shore forming mainly in late spring is that their development is linked to the initiation of melt streams and rivers that deposit water into Admiralty Inlet. Satellite imagery suggests that melt streams along the western shore have more of an effect on the sea ice than melt streams forming along the eastern shore, possibly because ice caps are present along the western shore which are absent along the eastern shore. This description will be added to Section 6.1.4.

We agree with the suggestion to move Table 3 to an appendix, shorten Section 6.1.5, and merging this section with Section 6.1.4. We believe retaining some description of fracture growth direction is important, as this has direct implications for understanding the potential processes that lead to fracture formation.

Although there are uncertainties in the method, the values reported in Table 3 come from absolute counts of fractures identified in each category. The percentages represent the proportion of fractures in each outcome category relative to the total number of fractures that started in the corresponding initial category.

The Discussion section nicely addresses the applicability and limitations of the study!

Thank you – we appreciate the positive comment.

Fracture widening is an important factor for traveling on ice. However, since the methods/data in this study do not address it, does it deserve its own section (7.5)? Additionally, Figure 13 seems redundant in this context. Could this topic be saved for a possible future paper on fracture widening?

We think the topic of fracture widening should be mentioned here, due to the influence fracture widening has on ice mobility and subsequent fracture formation elsewhere in the inlet. It is a process that is relevant to Inuit as navigating around wide fractures (e.g., >1-2 m) is more challenging than crossing narrow fractures. It is also relevant to discuss how for explaining some of the limitations of the methods used in this paper related to any future determination of fracture width.

In order to move towards the goal of shortening the paper, we propose to remove Section 7.5 and move Figure 13 to an appendix but add text about the mechanical role of fracture width in Section 7.4 along with a short sentence that discusses methodological challenges to flag this for future study.

I expected some discussion of the floe edge and variation/changes in fractures near it, as this was raised as a major concern (lines 50-59). While one cannot address everything in a single paper, would it make sense to include the ranges of floe edges (e.g. in Figure 5) and add a sentence or two on the distance of the first fractures from the floe edge, or whatever aspect the authors find makes the most sense? Alternatively, this could be left for another study, in which case its importance should be less emphasized in the introduction.

We will add the locations of the floe edge in each month for all years in Figure 5 and a sentence describing the approximate distance of the first fracture from the floe edge for each month in Section 6.1.1.

Technical corrections

L72-74: Are all the points listed in these lines covered by the manuscript? The paper delivers well on the recurring-pattern and process-understanding components, but less convincingly on the deviations and future climate sensitivity. It may be worth rephrasing these statements to better reflect the study.

This sentence will be rephrased to better reflect the study.

L186: me who?

This will be updated to 'the first author'.

L260: Since the airport is mentioned, even though its data are not used in the analysis, its location (or that of Adams Sound) could be pointed from the map of Figure 2. E.g. referring to 14 in the text.

The location of the airport will be described more fully in the updated text by referring to the placenames shown in Figure 2.

L265-267: Is the citation correct? Currently the DOI points to an analysis and forecast product.

This citation will be corrected.

L271: A verb missing?

This sentence will be corrected.

L287: Every 100 m?

This unit will be added.

L460: Should it be Table 3?

That is correct – thank you for identifying this error.

L524-527: Nicely formulated!

Thank you!

Figure 2: What is the relevance of Iqaluit in the left map? The map on the right is rather busy, and many of the place names do not appear to be used in the text. Consider limiting

the labels to locations that are mentioned in the manuscript. In addition, this figure could be referenced when mentioning Ikpiarjuk when it is first introduced (L50).

Iqaluit was the only major city that could be found within the inset area of interest, so this location was included as a reference point to orient the reader. We will add an Ikpiarjuk label and scale bar to the inset map to give an idea of the distance between these two locations. Placenames that are not referenced in the text will be removed, and this figure will be referenced when Ikpiarjuk is first introduced.

Figure 5: This figure (or Figure 4) seems like a strong candidate for a highlight figure. If so, would it be appropriate to note in the caption as well that in April-May the fracture occurrence frequency shown here might be too low due to methodological limitations discussed in the manuscript?

The Figure 5 caption will be updated to highlight the fact that low fracture occurrence frequency in April and May may be low due to methodological limitations.

Figure 9b: This plot is quite difficult to read for months other than June and December, and it does not appear to provide substantially more information than 9a. Would it be sufficient to replace the points and error markers in 9a with boxplots and omit the plot with histograms?

The suggestion for omitting the histogram and replacing the point and error markers with boxplots of fracture duration is useful. We elected to replace 9b with the boxplots to avoid making 9a too busy.

Figure 10: Please check that the plots and the figure caption are consistent. For example, I do not see any blue in 10a. In addition, the variable v_b in 10b is v_v in the caption.

The typo in v_v in the plot will be corrected and the figure caption will be updated to remove the reference to scaled values for calculated tensile strength to ensure consistency with what is shown in the graph.

Figure 11: I suggest reducing the height of plot 11a to allow December in 11b to be displayed fully. Have you considered using split violins? That could make the figure to be slightly narrower and potentially easier to interpret.

Thank you for these useful suggestions. We will reduce the height of Figure 11a to make 11b displayed more fully and use split violins to more clearly show the differences between warming and cooling for each month.