

Responses to Reviewer 2 (2nd round) -- *responses in italics*, reviewer comments in normal font)

First of all, we thank Reviewer 2 for the helpful comments and the careful reading of the revised manuscript. We have revised the manuscript to address essentially all the comments, as described in the point-by-point responses below.

Review of revised manuscript, “Sea ice break-up and freeze-up indicators for users of the Arctic coastal environment” submitted for publication in *The Cryosphere*.

This revised manuscript addresses many of my original comments, thanks for this. However, the text could still use some grammar/spelling corrections. The file with track changes lacks line numbers which would have made my reviewer’s job easier. There are still missing explanations that should go in the Data and Methods section. My main comment is that the landfast ice influence is still unclear to me, and I feel that it requires further analysis if this is to remain a key finding. I recommend another round of major revisions.

Figure 1 is much improved, thanks. Unfortunately, unless I am misinterpreting it, it still falls short in supporting a key finding of this work, namely that landfast ice strongly influences seasonal ice breakup & freezing timing. Most of the trios of sea ice concentration cells in Figure 1b lie outside of the median landfast zone, which is a problem for your finding, isn’t it? Further, while in theory it’s a good idea to partition your locations into those that are or are not strongly influenced by landfast ice, e.g. on page 23:

According to Figure 2 (formerly Figure 1), The selected grid cells for only three of the ten coastal sites lie outside the landfast ice: Chukchi Sea, St. Lawrence Island, and Utqiagvik. As noted in the text, the Chukchi Sea site was intentionally chosen to be well offshore of the coast in order to provide a “control” site. The St. Lawrence Island grid cells were also chosen intentionally to be outside the landfast ice area for reasons related to stakeholder uses of the offshore waters, as described on p. 15. For Utqiagvik, we summarize on p. 11 (bottom) – p. 12 (top) the results of a sensitivity analysis that showed there was negligible change in the computed metrics when the three pixels were given a one-cell displacement perpendicular to the coastline.

“The break-up start date at the coast is later than for the MASIE regions for Prudhoe (Beaufort Sea), Utqiagvik (Chukchi Sea), Tiksi (Laptev Sea), and both Canadian locations: Churchill (Hudson Bay) and Clyde River (Baffin Bay). These sites are all Arctic coastal locations at which varying extents of landfast ice are present. By contrast, the coastal locations have earlier break-up start dates (relative to their corresponding MASIE regions) at St. Lawrence Island (Bering Sea), Mestersvig (Greenland Sea) and the Bering Strait (Chukchi Sea). These locations are less prone to experience a buildup of landfast ice during the winter.”

your interpretation of Figure 1 is puzzling, e.g. (1) The trio of sea ice concentration cells near Utqiagvik lie outside of both median and maximum landfast ice shown in Figure 1, so “varying extents of landfast ice” are NOT present there, and (2) The trio near Mestersvig lie within the maximum landfast ice zone: why is it included in the “less prone” group? I also find your new Discussion section overly long, out of place (I think a shorter version should reside in the Introduction), and not adequately tied to your specific results. Thus I think that more analysis is

required to adequately support your hypothesis of strong landfast ice influence. For example, you could show a multi-year mean daily or monthly time series of landfast ice concentration or extent averaged over each sea ice concentration cell trio, with vertical bars showing interannual variability using standard deviation or quartiles, etc. This would provide information on the seasonal timing of when landfast ice comes and goes, in addition to supporting “less” or “more” “prone to landfast ice” statements.

We have responded in several ways to this general comment. First, we have shortened the Discussion section, both by moving some material to the Introduction and by eliminating some non-essential text. In particular, the background material on landfast ice and the pan-Arctic map of landfast ice distribution (formerly Figure 15) have been moved to Section 1. Second, in order to more substantively address the role of fast ice, we have added a new table keyed to the impact of landfast ice. This new table (Table 5) lists the sites in approximate order of decreasing lag between break-up at the local sites and the broader MASIE regions. The table also indicates in Column 2 whether landfast ice appears to play a role in the lag. The accompanying text clarifies the role of landfast ice at a few sites questioned by the reviewer (e.g., Mestersvig and St. Lawrence Island, Chukchii Sea). We note that the correspondence between landfast ice and the lag of break-up is evident but not perfect, and that the exceptions (Pevek, Sabetta) provide opportunities for further research to test the hypothesis presented in our discussion.

I don't think a “track changes” file should include your internal comments.

We have removed all internal comments from this change-tracked revision (Revision #2).

Page 4:

1) In the abstract, the primary objective “is;” here it “was.” Probably “is” is best for both.

Changed as suggested – see revised abstract.

2) “A subcomponent of this overall objective” -> “A secondary objective”

Changed as suggested – see bottom paragraph on p. 6.

Page 5:

1) “In the construction of this dataset, the NASA Team algorithm (Cavalieri et al., 1984) and the NASA Bootstrap algorithm (Comiso et al., 1986) were used to process the microwave brightness temperatures into a consistent time series of daily sea ice concentrations.” No, this is incorrect. NSIDC 0051 is pure NASA Team. This sentence refers to the CDR which evidently you did not use.

Text has been corrected to cite only the NASA Team algorithm.

2) “Prior to applying these definitions,” What definitions?

Reference to “these definitions” has been removed.

3) What is a “generic mean filter?” Do you mean a boxcar filter?

Yes, we used a boxcar filter and now state this explicitly.

Page 6:

- 1) “based on past studies and subsequent sensitivity tests” -> based on past sensitivity tests
- 2) “at by testing various values and selecting values” -> at by selecting values
- 3) “with break-up and defined freeze-up dates” -> with break-up and freeze-up dates

(1)-(3) have all been corrected.

4) “had the best agreement with years of indigenous observations” This needs a much more extensive explanation, or perhaps reference to a previous publication.

p. 7 (bottom) – p. 8 (top), we now provide two supporting references in this statement: “The definitions build on those used by Johnson and Eicken (2016; hereafter denoted as J&E), which were informed by Indigenous experts’ observations of ice use and ice hazards in coastal Alaska, and relate to planning and decision-making at the community-scale (Eicken et al., 2014)”.

5) “The selected values were those that generally maximized the number of such years across the various coastal locations and MASIE regions.” I think you’ve already said this and so this can be cut.

Repetitive statement has been deleted.

Page 9: “the relevance of local sea ice to uses by communities” ?? not sure what is meant here
A clarifying statement has been added, with examples (p. 11, top paragraph).

Page 12:

1) “innermost extent of the landfast ice does not always coincide with the coastline.” You should include a sentence that tells the reader that you are assuming that this is always wrong. IE landfast ice always DOES coincide with the coastline, is that correct?

Yes, that is correct. We have added this clarification to the text on p. 14, bottom paragraph.

2) What is the “nearshore edge?” You just implied (and should make explicit) that there is no “nearshore edge” ie that landfast ice coincides with the coastline, correct? So I am confused.

We have reworded this to refer to the “inner boundary” of landfast ice (p. 14, bottom).

Page 12:

Cut “(8)” near the bottom

(8) has been removed.

Page 14:

“The computation of the indicators was done for the term local” “m” -> “n” x 2

Corrected to “ten” (p. 17, line 4).

Page 17:

1) “dynamic -> “dynamic”

Corrected.

2) “corresponding indicators used by Bliss et al. (2019)” This material should go in the Methods section. Did you make any modifications to the Bliss method?

We have moved summary of the the Bliss et al. (2019) algorithm to Section 2 (p. 8, 2nd para.). On p. 19, where we introduce the comparison to Bliss et al. by simply stating the two concentration thresholds used by Bliss et al.

Page 20:

“a sufficient number of years” What is “sufficient?” This should be in the Methods section.

We have revised this statement to say that the Central Arctic metric was defined for “fewer than half (p. 22, bottom).

Page 22:

“assess the relationship between the local indicators and those for the broader MASIE regions containing the coastal locations” I think this is useful, but some caveats are in order. Your diagnostic parameters were tuned to coastal conditions, and even vetted by coastal users, yes? So, you should caution the reader that applying them to a large area that includes far-offshore regions may be a misapplication. IE your method may likely be "better" for coastal applications but it is not necessarily better (and could be worse) for either regional or pan-Arctic studies.

The reviewer makes a good point here, so we added the caveat about the local vs. MASIE indicators (p. 24, bottom – p. 25, top).

Page 26:

“These are regions in which it is common for ice to form along the coast in autumn, with the ice edge advancing offshore to meet the expanding main ice pack as freeze-up progresses.” Where is your proof of this? Perhaps my suggestion of moving your Discussion material to the Introduction could help solve this problem.

In order to support this point about the progression of freeze-up, we added a new figure (Figure 12) containing examples. We prefer to keep it in Section 3 because it fits the flow of that section as we transition from the break-up to freeze-up indicators.

Page 29:

1) “middle months of the break-up and freeze-up seasons (June and November, respectively)” These seasons should be defined in the Methods section.

In Section 2 (Methods), we have added text clarifying the definitions of the break-up and freeze-up seasons, including our choice of June and November as representative months. See p. 8, middle paragraph.

2) “Pacific hemisphere” This should be defined in the Methods section. I notice that Tiksi is included.

We have added the definition of the Pacific Hemisphere on p. 31 (near bottom of page).

Page 35:

1) As noted above, I suggest shortening this material and moving it to the Introduction.

As noted above, we have shortened this section and moved parts of it (including the background information on fast ice) to Section 1 (p. 5-6).

2) “lengthening of the open water season” Previously you discussed shortening of the ice season; you did not discuss lengthening of the open water season.

The correspondence between a “lengthening open water season” and a “shortening of the sea ice season” is not stated explicitly on p. 5 (start of bottom paragraph).

3) “The timing of break-up and freeze-up relates to the proximity to the coast” I cannot find where you showed this.

We have reworded this statement to say that the MASIE regions are centered farther from shore than the coastal grid cells (p. 37, first para.).

Page 36:

1) “In most cases, these differences can be related to the presence of landfast ice” I don’t think you’ve proven this.

We have attempted to support this statement by adding a new Table (Table 5) and discussion of it (p. 37-39). This new table lists the sites in approximate order of decreasing lag between break-up at the local sites and the broader MASIE regions. The accompanying text clarifies the role of landfast ice at a few sites questioned by the reviewer (e.g., Mestersvig and St. Lawrence Island, Chukchii Sea). We note that the correspondence between landfast ice and the lag of break-up is evident but not perfect, and that the exceptions (Pevek, Sabetta) provide opportunities for further research to test the hypothesis presented in our discussion.

2) Is Figure 15 necessary? I suggest not: I think Figure 1 is sufficient.

In view of the expanded discussion of landfast ice in the paper, together with the limited geographic areas shown in the old Figure 1 (now Figure 2), we prefer to retain the pan-Arctic map in the original Figure 15. However, in response to the reviewer's comment, we have stripped it down to a single panel. We have also moved it to an "up front" position as the new Figure 1 in order to set the stage for our discussion of the role of landfast ice, especially with regard to the timing of break-up.

Page 42:

"The sea ice indicators used here are based on local ice climatologies informed by community ice use" This is stated several times in the manuscript, but never discussed in detail. A reference to a previous publication is provided that perhaps used this community input. Two thoughts come to mind:

First, this needs more explanation here, given the number of modifications relative to the older work. IE were these modifications also "informed by community ice users?" In what way exactly?

As explained in detail in Section 2 (Methods) of the paper, the modifications made relative to the prior study in the Alaskan Arctic were minor and meant to ensure broader applicability of thresholds, such as at locations further south where seasonal indicators need to be distinguished from fluctuations in ice edge position during the winter ice season.

Second, is it true that this "community input" was solely from Alaskan local users? If yes, then is it good practice to develop sea ice diagnostics as done here and then apply them without modification to pan-Arctic coastal locations? This seems possibly ill-advised: any comments?

The input to define key characteristics of the seasonal ice cycle was solely from Alaskan local users. However, the types of ice uses and ice hazards in this region, as reflected in the definition of key seasonal indicators, are of broader applicability to other locations in the Arctic. Specifically, the commonalities in coastal populations using coastal ice cover (both drifting and landfast) as a platform for a range of activities, and to whom sea ice poses a hazard for boat and marine vessel traffic, are such that they justify the approach taken in this study to extrapolate from the Alaskan Arctic (with a range of ice conditions representative of the broader Arctic) to the pan-Arctic scale. Revision of the text in the conclusions section in response to the reviewer's comments clarifies and provides further perspective on this point.

Page 43: "from the author on request." Is data are available this sufficient for The Cryosphere?

All or our computed dates are presented in the paper's figures (Figures 6-7, 9-11, 13). We could also include these vales in tables in the Supplementary Material section if the journal so desires. However, Supplementary Material is already table-heavy with its listings of slopes and significance levels, so we believe the additional listings would make the SM unwieldy with little value added.