Reviewer 2

RC2: 'Comment on tc-2022-21', Anonymous Referee #2, 11 Mar 2022

Reviewer comments are in regular font; *author responses in italics*

Review of Walsh et al., "Sea ice break-up and freeze-up indicators for users of the Arctic coastal environment," submitted for publication in The Cryosphere.

This manuscript describes a nice study focused on the difference in ice break-up and freeze-up dates between near-coastal locations vs larger scale regional averages. The paper is generally well-written and the figures are clear. The results are convincing and will have an impact on how such analyses are used for coastal sea ice science. I do have some questions and comments, so I recommend for publication with major revisions.

Lines 63-64: As noted below, Bliss does not use this type of definition. So there are a variety of methods.

We will modify the text to say "Sea ice concentration thresholds have been used in various studies to determine the dates of sea ice opening, retreat, advance and closing. For example, Bliss et al., (2019) define dates of opening and retreat as, respectively, the last days on which the sea ice concentration drops below 80% and 15% before the summer minimum. Corresponding metrics are used by Bliss et al. for the dates of advance and closing."

Lines 77-78: The 25 km PMW products provide the longest time record. However, we have at least 20 years of higher resolution AMSR data as well. Do you think your analysis would differ if you used that? I suggest you acknowledge that higher resolution PMW data are available and that they might be uniquely useful for examining coastal science.

The higher-resolution AMSR data would resolve the near-shore sea ice distribution that is a focus of this study. We will add the following after lines 77-78 (original line numbers): "While higher-resolution permitting finer resolution of coastal sea ice is available from sensors such as AMSR (Advanced Microwave Radiometric Sounder), the record lengths are sufficiently shorter (about 20 years for AMSR) that our trend analysis would be severely limited by a reliance on such products".

Line 94: You've got a typo here. NSIDC-0051 is the NASA Team data set. You want to write NSIDC-G02202, the CDR data set. And also note that there is a Version 4 available: was this not available when you started this work?

We did use the data from NSIDC-0051. We will delete the reference to the NOAA/NSIDC Climate Data Record. The documentation for this dataset (https://nsidc.org/data/nsidc-0051) *points to the use of the NASA Team algorithm. The analysis described here was initiated prior to the availability of Version 4 of NSIDC-G02202.*

Line 107: "Examples include…" I suggest you clearly write out all modifications from the previous algorithm, so your methods can be potentially reproduced by others.

This comment is similar to one from Reviewer 1. In response, we will add a paragraph (following the previous Line 112) summarizing the changes in each indicator relative to the criteria used by Johnson and Eicken (2016). In brief, the following modifications were incorporated into the algorithms:

Break-up start:
- minimum sic threshold created at 15% (vs. last day exceeding Jan-Feb mean minus 2σ)
- undefined if average summer sic > 40% (vs. no such criterion in J&E)
- undefined if subsequent breakup end date not defined

Break-up end:
- first time sic below threshold for 2 weeks instead of last day below threshold
(vs. last exceeding larger of Aug-Sep mean or 15%)
- minimum threshold 50% instead of 15%
- undefined if break-up start not defined

Freeze-up start:
- first day on which sic exceeds Aug-Sep average by 1σ (as in J&E)
- undefined if mean summer sic > 25%
- undefined if subsequent freeze-up end not defined

Freeze-up end:
- first time sic above threshold for following 2 weeks instead of first day above threshold
(threshold is Jan-Feb average minus 10%, as in J&E)
- thresholds imposed: Minimum (15%) and maximum (50%)
- undefined if sic always exceeds threshold

Lines 110-111: I don't think v4 of the CDR has missing days.

See response to comment on Line 94. We did not use the CDR.

Line 111: "a spatial and then temporal smoothing" Please provide all details.

The following will be added in the revised manuscript: "The daily sea ice concentration values were spatially smoothed using a generic mean filter with a square footprint of 3 x 3 grid cells. This data was then temporally smoothed three times in sequence using a Hann window"

Table 1: There are a large number of seemingly arbitrary numbers here. Why 25%, or 40%, or 50%? Why two standard deviations, or one standard deviation? Why minus 10%? First, why did you pick these, and second, what if you vary these numbers?

The 10% threshold is based on prior work (Johnson and Eicken, 2016) in which sensitivities were explored. The 25%, 40% and 50% thresholds in Table 1 were arrived at by testing various values and selecting values that maximized the number of years with break-up and freeze-up dates defined. The selected values were those that generally maximized the number of such years across the various coastal locations and MASIE regions.

* You are providing the lat/lon of these coastal communities/stations. Why? They are not used anywhere. Instead, perhaps you can provide the exact grid cell numbers of the 3 cells for each location.

In the interest of a general readership (one that does not work specifically with the NSIDC passive microwave products), we believe that the latitude and longitude will be more useful to readers than the grid cell coordinates. The latter have the disadvantage that they will vary with the sea ice dataset and its resolution, while the latitude and longitude are site-specific and fixed.

* Mestersvig seems to be decommissioned, as far as I can tell. Why did you choose this?

While the permanent military function at Mestersvig has been discontinued, the site has been used as recently as 2015 for military exercises to test the responsiveness of Denmark's military and the cold-weather functionality of its equipment. Since the airport (with its 1800 meter airstrip) and other infrastructure, further use of this site is possible, if not probable.

Figure 1:

* Could you rotate all panels so that north is upward, as is usual?

In a revised version of this figure, we have rotated the panels for consistent north-south orientation and have added a northward-pointing "N" arrow.

* “Bering Strait coast” is an odd name for this location. Really it is “NE Chukchi Sea,” right? Also, these 3 grid cells are farther away from the coast than the other locations. Why?

This location is actually in the southern Chukchi Sea, as will be apparent in our revised Figure 1, which (per the suggestion of Reviewer 1) shows all the locations listed in Table 2. We have changed the name of this site in the Table to simply “Chukchi Sea”. This location is indeed farther from the coast than the other sites, and the location was intentionally selected to be farther offshore in order to provide a non-coastal counter-example to the other sites, all of which are adjacent to a coast.

* I suggest adding distance or lat/lon markers along the horizontal and vertical axes of each panel.

As noted above (2nd preceding bullet point), we have reoriented all panels in Figure 1 so that north is consistently at the top of each panel. We have also added a distance scale indicator for the entire figure

More on Figure 1: It took me a few minutes to understand the basic strategy here, so I suggest that you make this clearer. You show these coastal locations with red dots, but really they are never used. However, this distracted me and I thought you were going to compare coastal station information with these three grid cells. No, in fact you are using these 3 grid cells as proxies for coastal conditions. I suggest you say this explicitly. And then I am wondering:

We will expand the caption to state explicitly that the red dots (black dots in revised version) represent the actual locations of coastal communities, while the offshore squares represent the grid cells of the passive-microwave-derived sea ice concentrations used in computing the break-up and freeze-up metrics, expecting them to be reasonable estimates for the sea-ice indicators at each coastal location.

1) Why not choose SIC cells right at the coast? Yes, there could be coastal contamination, but I think the CDR has eliminated or much reduced this. At least you should check this.

In our examination of the coastal regions in NSIDC-0051, we found that there is indeed still some contamination present in grid cells adjacent to the coastline. For this reason, we chose grid cells that were as close as possible to the coastline without being at the coast (i.e., we required that there be a one-cell offshore displacement of any selected grid cell).

2) Why not validate these CDR SIC values with coastal information when available? EG Utqiagvik has a sea ice radar, doesn't it?

There is a sea ice radar at Utqiaġvik, but its range is less than 25 km so it does not "see" beyond the grid cell immediately adjacent to the coast. The northern Alaska coast is an example of a region in which coastal contamination led to non-zero ice concentrations at coastal grid cells when sea ice was not present (e.g., mid-September of years in the final decade of the dataset).

3) What if you chose a trio of CDR SIC cells that were at another location near the coast within a MASIE region? Do you think you would get a different result? I suggest that you take one or two MASIE regions and test this, ie take 3 different near-coastal locations within a MASIE region and run your analysis. How different are your results?

In response to this comment, we actually experimented with the grid cell selections at Utqiaġvik and Sabetta. The trends and interannual variations of the break-up start and freeze-up end showed no notable changes when the three selected grid cells were displaced offshore or alongshore by one grid cell.

4) You interpret many results in the context of landfast ice. I think you need to show that the CDR SIC cells you have chosen (because they are offshore) are actually in the landfast ice zone at each location. EG you could add a typical landfast ice contour (when it's there) to each panel of Figure 1. Or better, some kind of interannual min/max measure of landfast ice extent relative to the cells.

This concern was also raised by Reviewer 1. In response, we are augmenting this section with additional discussion (placed in a new Discussion section, as suggested by Reviewer 1) and a new figure showing the maximum spatial extent of landfast ice during June (the middle of the break-up period). This map is based on the charts of the U.S. National Ice Center for the 35 years ending 2007. We have superimposed on this map the locations of our coastal sites, thereby providing a basis for our assessment of a role of landfast ice in delaying the break-up.

Line 163: Typo: "Figure 1" -> "Figure 2"

Will be corrected in revision.

Lines 165-167: Why did you include MASIE regions that are not near your 10 locations, eg "Central Arctic?"

The original text was intended to list the entire set of Arctic and subarctic regions in Figure 2. In the revision, we will omit from these lines the regions that we do not use in this study (Regions 6, 9 and 11).

Line 193: Typo: "Table 2" -> "Table 1"

Will be corrected in revision.

Figure 3: It might be useful to remind the reader in this caption that break-up start only exists when there's break-up end, and same for freeze-up, thus only two panels are needed in this figure.

This reminder will be added to the caption of Figure 3.

Lines 213-216: This is interesting; I think Bliss vs J&E is in some ways analogous to NASA Team vs NASA Bootstrap. IE Team uses fixed tie points, while Bootstrap uses "dynamic" ie time/space varying tie points. There is value in each method (although generally the latter seems to be more accurate).

We will add a statement pointing to this analogy between the algorithms and the J&E vs. Bliss differences.

Figure 4: I had to look up "violin plots" but there is a Wikipedia page so ok. Still, it might be nice to write one sentence explaining what this is. Also:

We will restructure the opening of the paragraph (originally beginning on Line 232) to explain what a violin plot actually is.

* What are the black strips within each histogram? Are they gaps where there's no data for that day of year?

The thin black lines represent the observations themselves, i.e., the indicator dates for each year. The black strips are clusters of lines representing groups of similar values in the distribution, i.e., in this case they represent a narrow range of dates in which multiple years had their break-up or freeze-up dates. We will add this information near the start of the paragraph introducing the violin plots.

* Perhaps you want to mark the mode or mean on each histogram, and write it as text?

We are adding a new table (Table 3) listing the mode (peak of distribution) of the dates in the violin plots   For each metric and location, the table includes the corresponding values from J&E and Bliss et al.

Figure 5: I suggest writing in text the two slopes in each panel within each panel (or you could create a separate table). And this goes for all subsequent similar figures with trend lines. Perhaps one big new table with all slopes for all such figures together?

Since legible font would clutter the 12-panel figures, we will list the slopes a new table (Table 4) for Figures 5-6 and a similar table (Table 5) for Figures 8-11.

Line 377: Typo: "…and negative…" ie add "and"

"and" will be inserted in revision.

Line 387: Typo: "Figure 12" -> "Figure 13"

Will be corrected to "Figure 13" in revision.

Line 411: Here is a main result: The variance at three grid cells within a large region is higher than for the regional mean. This is useful in the context of coastal science, and it is very reasonable i.e., intuitive. I might propose that it is too reasonable, i.e., isn't this expected that variance increases when you focus on a small subregion? Actually, this could be true in some cases and the opposite might be true in others (eg if you picked three grid cells in a "boring" place with low variance). What if you took a MASIE region and made a map of some factor analysis parameter that would illustrate this? Would it show higher variance near the coast and lower variance toward the perennial ice pack? This seems important for you in order to really show that your result is not so obvious.

We had some difficulty in interpreting this comment, but we tried to address it by performing a factor analysis on the ice concentrations within a MASIE region (Figure 1's Region 2, the Chukchi Sea). The strongest factor loadings were found in the south, where more years experience freeze-up and break-up than in the north. As the reviewer notes, this is not surprising because the variance is greater in the south than in the north. We will add a statement in the text *after the statement beginning Line 411 (original submission), pointing out that the difference in variance indeed contributes to the difference in explained for the local and regional indicators. However, we do not find it so obvious that the* <u>*percentage*</u> *of variance explained by the first factor should be greater for the more localized metrics.*