

Reviewer comments are in regular font, *our draft responses in italics*

Reviewer 1

RC1: ['Comment on tc-2022-21'](#), Anonymous Referee #1, 09 Mar 2022

To the Authors,

This is an interesting and relevant study that recognizes how coastal zones possess unique sea ice regimes, and works to develop more suitable indicators to identify freeze up and break up. Your methods are promising for future studies to adopt in coastal zones experiencing seasonal ice cover, and the broad spatial and temporal coverage of this study makes it applicable to a broad audience of scientists focusing on sea ice from the Laptev Sea to Baffin Bay. However, this paper has significant problems that I recommend are addressed before publication. These problems are explained in detail in my line-by-line feedback (below), but the decision-making that went in to cell placement in the MASIE regions are not clear, which has a ripple effect in how the results are interpreted, since the freeze-up and break-up that occurs within these cells (covering no greater than 75 square kilometers in surface area) are interpreted in the broader geographical context of the MASIE regions they fall under. Until the methodology is better explained for why the cell locations were selected, the subsequent interpretation of results remains weak. It also inhibits the replicability of these methods in future studies. In the results, correlations are assumed, justified with qualitative descriptions of the coastal sea ice regimes. For example, the later break up dates are attributed to the lag in landfast ice break up compared to drift ice. However, there is no attempt to validate (through existing datasets or literature, see feedback on line 457) that the later break ups in these cells are indeed landfast ice, and not some other coastal process unique to the region of study. Correlations between freeze up and break up trends and coastal geography needs to have been determined by yourself, or strongly supported by citing previous studies. Results interpretation is also an issue, as this generally occurs in the Results section without citations, and a Discussion section is not included. The significance of your findings is not impactful without better referencing of the literature, and neglecting to do so inhibits the contribution this makes toward expanding the body of knowledge about the cryosphere, which is the stated purpose of TC as a journal. These problems must be corrected with additional writing and reorganization of the manuscript, as well as potentially requiring additional data analysis for validation of results interpretation if it is not supportable by previous studies. Therefore, I recommend this manuscript be accepted pending major revisions.

The reviewer lists three major concerns that need to be addressed: (1) the basis for the selection of locations for cell placement, (2) the validation of the claims about the role of landfast ice and (3) the need for additional references to the published literature in the Results section. The revised manuscript will respond to each of these concerns, as described below in the point-by-point responses. In particular, we will provide clarity and a justification for the selection of the grid cells offshore of each local site; we have prepared a new section on landfast ice (including a figure on its spatial distribution and the addition of landfast ice masks to the panels in Figure 1) to support our claims about the role of landfast ice in the indicator offset; and we have

identified a number of references for inclusion in a new Discussion section (Section 4) in order to provide context for our findings.

Line by Line feedback

Line 53: A brief definition of how we define “offshore” would be useful for the uninitiated. Is this a certain distance from the coastline? A bathymetric contour?

“Offshore” was used here to refer to the nearshore marine environment because it is often used by the stakeholders in coastal communities. The wording will be changed to “nearshore marine” in the revised manuscript to make it clearer to TC readers.

Line 107: I could be misunderstanding this paragraph, but are the authors saying that these definitions are revised to be more appropriate to offshore environments, which is the focus of this study? If so, why are the authors focused on revising criteria to broaden the applicability to “non-coastal” areas, which are not the focus of this study? Or is this a typo of ‘near-coastal’, which is a term used in this paper? In any case, what’s clear is the authors are revising these metrics to consider break up and freeze up in an environment that was previously unaccounted for. In which case I would suggest an explanation of what the criteria used to be, how the criteria has been changed in this study, and how this change is better suited for the environment the new criteria is being applied to. This would probably work best as additional columns in table 1, where information on the revised criteria is already provided.

There was indeed a typo, as the text was referring to coastal areas. The typo will be corrected in the revision. In response to the second part of the comment, we will add a new paragraph and table 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 (sic = sea ice concentration; σ = standard deviation):

Break-up start:

- *minimum sic threshold created at 15% (vs. J&E’s 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 is below threshold (larger of Aug-Sep mean or 15%) for 2 weeks)
(vs. first day below threshold in J&E)*
- *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*

Line 113: This paper is written with a clear objective in mind and would benefit from a consistent articulation of it to be used throughout the paper. There are three areas where an objective is articulated, but the wording varies. Line 88-90 states the objective of this paper is to examine the timing of freeze/break up as key constraints for human/ecosystem activity. Line 113 says a key objective of the work is to compare various dates at nearshore locations with corresponding metrics for broader (non-coastal?) areas of the AO and subarctic seas. Line 449 says the primary objective is to use local-based indicators to construct freeze/break up indicators at near-coastal locations relevant to stakeholders. These objectives are not contradictory, but they're not consistent. Is there a difference between a key objective and a primary objective? Perhaps the former a subcomponent of the latter? The objective on Line 88 specifies Arctic settings, but the objective on Line 113 expands this to the Arctic and Subarctic. Table 2 confirms the study includes both Arctic and Subarctic locations. It may seem like nit-picking but keeping the objective wording consistent and referencing it consistently throughout the paper will be immensely helpful to the reader. I think Line 449 objective is the strongest in terms of clarity, and should be provided in the introduction (I would recommend specifying Arctic/Subarctic coastal environments in the wording of this). If the key objective referenced in Line 113 is indeed a subcomponent of an overall objective, it should be clarified as such, and also provided in the Introduction. The authors could refer to them as Primary Objective (PO) and Specific Objectives (SO1, SO2, etc.), and refer to them throughout the paper using these abbreviations.

We agree that there was a fragmented statement of the objective in the original submission. The revision will follow the reviewer's suggestions and state the primary objective consistently (from the original Lines 449-451) in the abstract, the Introduction and the Conclusion: "The primary objective of this study is to use locally-based metrics to construct indicators of break-up and freeze-up at near-coastal locations in which sea ice has high stakeholder relevance".

Line 118: Is it possible to clarify what "close" is? I understand the authors can't use adjacent passive microwave cells due to land contamination, but maybe the next cell over? In which case since the spatial resolution is 25km would it be fair to say the authors chose cells beyond 25km from the coastline? Phrasing it this way is consistent with how the cells appear to be distributed in Figure 1. However it is interesting to see that the cells are much further away from the coastline in the Chukchi Sea compared to the other study locations. What informed this decision?

Provide a sentence or two explaining the exact considerations that went into cell selection beyond saying the cells are close to the coastline.

The reviewer's interpretation is correct: Because cells adjacent to the coast were excluded, the selected grid cells satisfied the criterion that they were the cells closest to the coast but centered at least 25 km from the coast. The revised text will explicitly state this criterion. The Chukchi grid cells represent an exception chosen specifically to allow for comparison with a location farther from the coast (offshore from the continental shelf and unaffected by landfast ice).

Line 127: No need for the apostrophe in “weeks”

Apostrophe will be removed.

Line 154: Typo in “Indigenous community”

Typo will be corrected.

Line 157: An example of why elaboration on why the authors chose the cell locations they did would be helpful (per my feedback on line 118) The significance of St. Lawrence Island in the Bering Sea is, per Table 1, due the location of indigenous communities. However, because the communities of Savoonga and Gambell are situated on the Island's northern coasts, I am curious why the authors chose cells to the south of the island for this study area? Does it pertain to hunting locations for these communities? These cells are also proximate to the southern polynya that keeps the ocean beyond the island's barrier islands relatively ice-free. Did this factor into the authors decision to select this spot? Providing rationale for cell location choice could even be included as an additional column in Table 1, if the rationale varies by study location.

The near-shore locations were selected to provide a geographically distributed set of sites at which sea ice affects coastal users. The affected users were chosen to be a mix of coastal villages, industry and military sites, as indicated in Table 1. There is admittedly some subjectivity in the selection of the sites, and we will state this in the revised text, but we believe that the geographical distribution provides a defensible rationale for the selection. The revised text will emphasize that geographical distribution and impacted human activities were the primary considerations in the site selection.

Line 160: Is it possible to re-cite so we know exactly which of the cited literature is relevant to break up and freeze up metrics?

We will add references to Markus et al. (2009), Johnson and Eicken (2016), Bliss and Anderson (2018), Peng et al. (2018), Bliss et al. (2019) and Smith and Jahn (2019).

Line 170: Is it possible to include the cell locations in Figure 2? Certain regions (e.g. Baffin Bay, Bering Sea) are very large, and the authors are looking at very specific areas within them. Perhaps adding them as bright red/yellow stars denoting general cell location could help readers to connect Figure 1 with Figure 2. Also, I notice the Canadian Archipelago (9) is missing in the figure caption. Please fix this.

We have made a revised version of Figure 2 with black dots at the locations of the 10 sites on which the study focuses. We have also added the site locations with place-names to Figures 3 and 4 from the original submission, as well as the new figure showing the landfast ice distribution. This suggestion is much appreciated because the broad geographical distribution is now readily apparent in the revised figures.

Line 288: Another Study Objective that can be referenced using consistent terminology (see my feedback regarding Line 113).

As noted in the earlier response (Line 113), we will revise the stated objectives for consistency. The statement here (Line 288) will be reworded to conform to the statement in Lines 449-451.

Line 302: St. Lawrence Island is generally encircled by landfast ice between January and May/June, and can exceed 25 kilometers off the northern coast. However, the cells are located south of the island, where the presence of a latent heat polynya inhibits the seaward advancement of the landfast ice edge beyond the barrier islands. This polynya also keeps the area relatively free of drift ice. At least one of the three cells are within the polynya boundary, which generally does not extend east of the island's southeast cape. Was this a consideration in the interpretation of the results? Rather than due to being free of landfast ice, earlier break up could be detected in this region because the wind-driven advection of sea ice out of the cell's locations compared to other areas in the MASIE region, which is the Bering Sea in its entirety. Again, given the size of the MASIE subregions, I'm not sure why this particular area for the Bering Sea was selected for cell placement unless the authors provide information regarding what factors were considered beyond proximity to the coastline.

The grid cells off St. Lawrence Island were chosen to reflect timing and location of subsistence harvests by the communities of Gambell and Savoonga. Because of extensive ice coverage, including landfast ice, north and northwest of the island, both communities traditionally conduct bowhead whale harvests at hunting camps on the south side of the island once spring ice break-up is underway (Noongwook et al., 2007). These sites also reflect the seasonal migration of whales in waters south of the island with the seasonal retreat of the ice cover (Noongwook et al.,

2007), modulated somewhat by the presence of a polynya south and southwest of the island (Krupnik et al., 2010; Noongwook et al., 2007). Traditional walrus harvest practices on St. Lawrence Island await the tail end of the bowhead whale hunt (Kapsch et al., 2010), with timing of spring ice break-up south of the island as the driving factor. These practices motivated our selection of grid cells southeast of the island. We have revised Figure 1 to show the location of landfast ice in the vicinity of each coastal location, including St. Lawrence Island, where the new figure shows landfast ice confined to the northern coastal region – consistent with the frequent presence of the polynya south of the island, as noted by the reviewer.

New references:

Kapsch, M.L., Eicken, H. and Robards, M., 2010. Sea ice distribution and ice use by indigenous walrus hunters on St. Lawrence Island, Alaska. In SIKU: Knowing Our Ice (pp. 115-144). Springer, Dordrecht.

Krupnik, I., Apangalook, L. and Apangalook, P., 2010. “It’s Cold, but Not Cold Enough”: Observing Ice and Climate Change in Gambell, Alaska, in IPY 2007–2008 and Beyond. In SIKU: Knowing Our Ice (pp. 81-114). Springer, Dordrecht.

Noongwook, G., Native Village of Savoonga, Native Village of Gambell, Huntington, H.P. and George, J.C., 2007. Traditional knowledge of the bowhead whale (Balaena mysticetus) around St. Lawrence Island, Alaska. Arctic, pp.47-54.

Line 304: Interpretation of results is generally best confined to the Discussion section. That being said, the wording is not clear in how landfast ice is a key determinant of the timing of break up. I assume the authors are saying the break up start dates are later in the coastal regions than the MASIE regions because landfast ice generally persists later than areas dominated by drift ice? In which case this needs to be clarified with more specific wording. “Landfast ice generally persists in coastal areas longer than drift ice at the end of the season, and is therefore a key determinant in the timing of later break up onset relative to the broader sector of the seasonal ice zone”. Something like that which is more clear. However, unique geography of each region (e.g. the polynya off of St. Lawrence Island) complicates speaking in such general terms about the role landfast ice plays in comparatively later break up detection in coastal regions versus MASIE regions. I recommend some sort of effort to validate that this is the case. This can be accomplished by referencing concurrent datasets such as ice charts or satellite imagery, or even cited literature with qualitative descriptions of where/when landfast ice is located relative to cell placement. It is insufficient to say whether or not the general area — not even the specific area occupied by the cells — is prone to landfast ice build up.

The need for more specificity about landfast ice is a valid criticism of the original submission, so we are augmenting this section with additional discussion (placed in a new Discussion section, as suggested) and a new figure shows 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 ten local sites, thereby providing a basis for our assessment of the potential role of landfast ice in

delaying the break-up. In addition, we have revised Figure 1 to include landfast ice masks for each panel.

Line 323: Regarding Figure 8, when the timing of sea ice events are being studied, especially onset and break up, some studies will consider September 1st of the previous year to be the Day 1, and August 31 of the following year to be day 365 (e.g. September 1st 1996 - August 31 1997). Because Break Ups in this figure are generally between Days 60 - 180, it appears January 1st is considered Day 1. This is fine, but the authors may want to specify this in the Figure 8 caption, to avoid confusion for any readers expecting September 1 to be Day 1. Also, I notice the y-axis scales are not standardized. Was this done intentionally? I can see how standardizing the y axis to the Sabetta / Kara Sea, which has the largest spread of values, may make it hard to interpret plots with a tighter spread of y axis values (e.g. Tiksi / Laptev Sea) by pushing the points closer together. If it is possible to standardize the y-axis, I would recommend it. However if this makes it difficult to interpret the plots, it's okay to leave the y-axis as is and make mention of this in the caption. Also, it is difficult to see the standard error portions of the MASIE region with the current color. Can the authors choose a darker shade of pink so that the SE stands out better?

The revised caption will clarify that the day number is the Day of the Year, and we will add this information to the text in Section 3. We will also add to the caption a statement that the scales on the y-axes vary regionally in order to make the various panels more reader-friendly. Finally, we have darkened the shading in each panel in order to make the standard error more apparent.

Line 330: Since this figure is showing the same thing as Figure 8 for the break up dates and does not require a caption, it would be better to combine them into a single panel of figures, 8a, and 8b, (8c, and 8d for freeze up) with one caption. This may be disregarded if the figure becomes too big. However the lack of captions for Figures 9, 10, and 11 due to their similarity makes me think it would be better to just combine them.

We tried a layout of the figure as suggested by the reviewer. However, with 40 panels, it was decidedly reader-unfriendly, and it will not fit on a single journal page (at least legibly). As a compromise, we have expanded the headers to highlight the differences among the figures, and we will expand the captions of Figures 9-11 so that the reader has the essential information below each figure.

Line 397: I am wondering why the authors chose to put the x axis ticks in the middle of the graph instead of the bottom? Please correct.

Figure 13 has been redrafted so that the x-axis scale is at the bottom of each panel.

Line 443: I did notice a lot of results interpretation included in the results section, and was expecting to see this in a Discussion section. Why is this section omitted, and we go right from Results to Conclusion?

The revision will include a separate section (Section 4) entitled Discussion. This new section contains the interpretation sections that were formerly in Section 3, and it also includes an expanded discussion of the role of landfast ice based on the new landfast ice figure and our response to the reviewer's comment on Line 304 and Line 457 (see following response).

Line 457: The authors are saying later break ups in the study areas are due to the lag of landfast ice break up. However, given the relatively small area these cells occupy compared to the larger region the authors say are "known to have extensive landfast ice" makes this connection weak. Landfast ice can be highly spatially heterogenous, even in regions that are known to have large extents of cover. I recommend the authors find a way to validate that the later sea ice break up is indeed due to landfast ice. This may be accomplished with existing landfast ice datasets. The Canadian Ice Service provides sea ice charts for the northern coast of Alaska, the Canadian Archipelago, Hudson Bay, and Baffin Bay, including the timing and location of landfast ice regions. Simply taking a sample of later break ups and confirming the cells are occupied by landfast ice during this time would strengthen the connection between later break ups and landfast ice.

As noted in the response to the reviewer's comment on Line 304, we are adding some substantiation of the role of landfast ice in a new Discussion section. This section will include a new figure showing the spatial extent of landfast ice during June (the middle of the break-up period) in the form of maximum landfast ice extent. We have superimposed on this map the locations of our local sites, thereby enabling a demonstration of the correspondence between delayed break-up and the presence of landfast ice and providing a basis for our assessment of a role of landfast ice in delaying the break-up. This new material supports the previous statement about the role of landfast ice in most instances, although there are a few exceptions that we will point out.

Line 458: The main benefit of including a discussion section is to interpret and contextualize the results with the broader body of scientific literature. Right now, there's no discussion section, and results interpretation in the conclusion section does not cite any literature where results are interpreted. There is plenty of literature explaining how shallow bathymetry and freshwater inputs facilitate earlier sea ice freeze up in coastal zones. These results are consistent with the findings in that literature, why is it not cited? The omission of a discussion section and lack of literature cited in results interpretation prevents readers from connecting this work with the broader body of scientific knowledge. It weakens this manuscript from serving as the basis for future research. This is a significant problem that needs to be addressed before this manuscript is suitable for publication. Any writing interpreting the findings in the Results section should be moved to a discussion section, with cited literature throughout.

We agree with this assessment, and our response is threefold: (1) the addition of a new Discussion section (Section 4) in which the interpretations formerly under “Results” are placed, and (2) an expansion of the material on landfast ice and its relation to delayed break-up, and (3) the addition of several new references on previous studies of landfast ice and its role in the timing of break-up. These additional references enable us to better place our results into the context of prior work.