

The Cryosphere Discuss., referee comment RC1  
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## Comment on tc-2022-21

Anonymous Referee #1

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Referee comment on "Sea ice break-up and freeze-up indicators for users of the Arctic coastal environment" by John E. Walsh et al., The Cryosphere Discuss.,  
<https://doi.org/10.5194/tc-2022-21-RC1>, 2022

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### 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.

### **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?

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 the authors 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.

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.

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.

Line 127: No need for the apostrophe in "weeks"

Line 154: Typo in "Indigenous community"

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.

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?

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.

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

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.

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.

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?

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.

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.

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?

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 Bafin 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.

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.