

DETAILED REVIEWER COMMENTS:

Abstract: “reduced access to subsistence hunting species” should be reworded. Its not the species that are doing the hunting.

Abstract: The abstract doesn’t seem to connect to the title. The title references “impacts” but the abstract describes that the main results pertain to summarizing sea ice trends.

Pg 1, Line 20: Suggest changing to “while changes in the seasonality and extent influence the migration of...”

Pg 2, Lines 3-6: The first two sentences are very vague and unclear. What are the challenges of the placed-based nature of climate change? The second sentence is confusing and seems to go off topic by referencing how research is to provide benefits. What potential benefits are you talking about?

Pg 2, Lines 11-13: While it may be true that the timing of break-up is more important than ice thickness to a local community, the authors should also consider that the definitions that scientists use to define break-up, which are determined in part by the observational methods and limitations, may also be very detached from how a community observes and defines break-up. Therefore, it is not only the variable that’s important but also the definition and observation of that variable.

Pg 2, line 15: What is meant by rotten ice?

Pg 2, lines 16-17: It is not clear what is meant by “this type of metric”. Also, I don’t understand how the authors successfully made the argument that incorporating indigenous knowledge allows of the use of large-scale datasets to examine local impacts. Is it that local experts are able to embed their local observations within longer-term climate records?

Pg 2, Line 31: “with varying levels of dependence on subsistence activities, such as susceptibility to coastal erosion and interaction with the offshore oil and gas industry” How are susceptibility to coastal erosion and interaction with industry examples for dependence on subsistence? This sentence needs rewritten.

Figure 1: Does the grid cell in the Bering Strait overlap with the Diomed Islands? Since this is the only map in the paper, the paper will be improved by an improved map that shows the community locations in a bit more details.

Pg 3, Line 4: Please reference the passive microwave satellite record if that is in fact what you are referring to.

Pg 3, line 7: correct to “a different number of”

Pg 3, line 19: The paper would benefit by an expanded methodology. For example, it is not clear why “ the maximum concentration...was extracted from within this area”? Why was the maximum extracted and not the average value. Was data analyzed across the entire annual cycle?

Pg 3, line 22: Instead of calling it freeze-up and break-up, I wonder if more accurate terms may be “ice-on” and “ice-off”. I recognize that these are not necessarily common disciplinary terms, but since you are dealing with relatively small study areas, ice coverage can cross the threshold

quickly due to a shift in wind and thus may have nothing to do with a real phase change (which is implied by “freeze-up”).

Pg 4, line 1-3: The methodology used to treat the years at Barrow when the ice coverage didn't drop below 30% is not clear to me. Please explain in greater detail how you were able to use the 45% or 60% threshold for these years, and integrate back into the longterm dataset. Also, I cannot easily see (too small) within the right-most panel in Figure 2 which years are when the ice never dropped below 30%.

Pg 4, line 2: This paragraph is about freeze-up yet it references “break-up dates”.

Pg 4, line 7: The linear trend at Barrow for break-up was not calculated also. This should be stated.

Pg 4, Line 10: “Kotzebue shows 132% of the variance of freeze-up day for Shishmaref, and 108% the variance of break-up day.” I understand what is being said here, but the wording needs to be clearer.

Figure 5: Please be clearer in the presentation. Red is the number of extreme storms during the open water period. Here, how and where is the open water period defined? Is this also using a 30% threshold?

Pg 5, Lines 26-34: This analysis of the “days left for whaling” is close to meaningless. With a nominal start date of April 15, 80 days, which is what is shown for recent years, would theoretically allow for whaling through the beginning of July. The bowhead hunt never really went too far past early June. It is true that the ideal ice conditions for ice-based spring hunting is being shortened, but these results do not reflect those trends. Looking at the earlier years, the authors show between 140-180 days for whaling, which would put the hunt all the way into fall, which doesn't make sense. This analysis seems to imply that ice is the only important piece to whaling. The authors acknowledge this somewhat by saying that “the end of whaling season does not necessarily coincide with the break-up of the landfast ice or the retreat of ice from the coast” and further note that their analysis may not capture the finer-scale resolution required to track landfast ice. This is an understatement. This analysis bears little relevance to landfast ice, and especially from the perspective of how a community may use landfast ice.

Pg 6, lines 8-12: This paragraph seems to overlook that the community of Utqiagvik is already adapting by hunting more in fall. This should be discussed.

Pg 6, line 24-27: These statements are not well-supported and may be inaccurate. Does Clarke's paper reference changes in hunting? I suspect not. The bowheads for the BCB stock begin to arrive in the Beaufort in late April/early May, not August. (Perhaps the authors are trying to say that bowheads migrating west from the eastern Beaufort are arriving to the western Beaufort near Pt. Barrow earlier in Fall?) Also, I am not sure there is any literature that shows that the bowheads passage through Bering Strait is tied to local ice conditions there. If there is, it should be referenced. This statement seems quite speculative.

Section 4.3: This entire section that discusses impacts on marine mammals, which rely on large regions and migrate through diverse ice conditions, seems disconnected from the results of this paper, which focus on local conditions near specific communities.

Pg 8, Line 8: The authors point out that their simple definition of transition seasons based on sea

ice concentration thresholds may be problematic. I agree, and suggest that the authors think carefully about what new data and evidence they can introduce to this paper to make their quantitative results better provide a relevant context for the discussion in this paper on impacts to communities.

Section 5.1: A great explanation of the methodology to recreate the BSI from the HSIA data should be included in the methodology section.

Figures 7 & 8: Why are the upper limits of the BSI shown in Figure 7 not represented in Figure 8 (e.g., values above 1000)? Does Figure 8 correspond to a subset of earlier years?

Pg 8, line 30: change to “are very likely”

Pg 9, line 20: Is there any evidence that increased shipping is leading to more goods, and a greater diversity of goods, to Arctic communities?

Pg 10, line 20: This conclusion regarding the traditional spring hunt being cut in half due to ice conditions is not accurate and is not well supported by the data presented in this paper. See my earlier comments. This conclusion, above all else in this paper, should not be published.

Pg 10, line 24: There is absolutely no evidence presented in this paper or relevant literature cited that indicates a change in bowhead whale migrations.

Pg 11, line 5: Where is the evidence that hunters are evaluating risk differently than in the past? The claim that hunters today walk on thinner ice than they used to because of the pressures of environmental change and hunting regulations seems over-simplified and perhaps altogether inaccurate.