Reply to Referee #3: Interactive and Supplementary Comments

Impacts of a lengthening open water season on Alaskan coastal communities

Rolph *et al.* (2017), tc-2017-211

We would like to thank Referee #3 for his/her constructive comments, which have helped improve the quality of the paper. We present below our detailed responses to the comments in orange.

Interactive comments
Anonymous Referee #3
Received and published: 1 December 2017

1. Does the paper address relevant scientific questions within the scope of TC?
Yes
We are glad to hear the reviewer agrees that the scientific questions raised in this paper are within the scope of TC.

2. Does the paper present novel concepts, ideas, tools, or data?
The ideas are important and novel, but are not well developed.
Please see the responses to Reviewers #1, Question 2, and #2, Question 3. In summary, we have attempted to provide a more unified framework by including an additional variable (wind) to develop community-informed metrics that were not in the original submission. The newly added indices include the number false freeze-ups and break-ups during each seasonal transition, the number of geomorphologically-significant wind events over open water, fractions of days deemed 'too windy' for subsistence hunting via boat, freeze-up/break-up timing, and open water duration provide methods. The methods and indices developed here can be applied not just to the three Alaska communities examined in this study, but also anywhere else along other coastlines affected by sea ice. This applicability of our methods developed here to other places we feel is an important tool to share with the Cryosphere community.

3. Are substantial conclusions reached?
No. The paper draws conclusions (many of which may be inaccurate) from a weak analysis.
Please see the responses to Reviewer #1 and Reviewer #2, Question 3. These responses demonstrate new results we have added to the manuscript which relate directly to our conclusions. We have found that there is an increased number of combined false freeze-up/break-ups during the transitions between ice-covered and open water seasons in each community, as well as a significant increase of open water periods with winds too strong for subsistence hunters to hunt successfully via boat. Our conclusions that the changing sea ice conditions (which are related to the change in windy conditions over the expanded open water period) are directly impacting Alaska coastal communities ability to maintain a subsistence lifestyle as well as (related) challenges they face in travel over ice or water in the increasingly erratic transition season.

4. Are the scientific methods and assumptions valid and clearly outlined?
No
We appreciate the reviewers comments here, and accordingly we have added a new subsection for each method and assumptions used when developing each index, and feel this greatly improves the organization in this context. Please see also our response to Reviewer #1, Question 4, and our supplementary responses to Reviewer #2. In summary, we have outlined how we have used each

reference pertaining to the appropriate index, and explicitly how we have used the climate-related thresholds (e.g. sea ice concentration or wind speed) in the development of each index timeseries.

5. Are the results sufficient to support the interpretations and conclusions?
No
Please see the example figure provided in our response to Reviewer #2 for Question #3. We have restructured the manuscript such that each result of our multiple indices calculated for each community has its own corresponding subsections in each the Results section and Discussion section. This improves the organization of the paper and allows for an easier-to-follow connection from the results to interpretation of the results.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
No
Please see our responses to Reviewers #1 and 2, Question 6.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
Yes
We are glad to hear this was clear in the manuscript and look forward to feedback if the reviewer feels this was not addressed for some reason in the new manuscript.

8. Does the title clearly reflect the contents of the paper?
Yes, although I don't think the paper is well positioned to properly discuss or draw conclusions about the full scope of impacts that they attempt to address.
Please see our response to Reviewer #1, Question 8: Our title is "Impacts of a lengthening open water season on Alaskan coastal communities: deriving locally-relevant indices from large-scale datasets and community knowledge ." In our updated version in response to the reviewers' comments, we chose to develop indices that relate directly to impacts on Alaskan coastal communities. The trends found in most of the indices are also directly related to the lengthening open water season, which we believe is a justification for including "open water season" in the title.

9. Does the abstract provide a concise and complete summary?
Somewhat
We have updated the abstract so that it concisely outlines our study communities, how we have developed each community-relevant index, which datasets were used, as well as the main conclusions from the changes seen in the results of the calculated indices timeseries.

10. Is the overall presentation well structured and clear?
Somewhat
Please see our responses to Reviewer #1 and #2, Question 10: We have taken much care to re-organize the manuscript such that each index has is organized into appropriate subsections. The subsections are then consistent across the methods, results, and discussion.

11. Is the language fluent and precise?
Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
Not relevant.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
The methodology section should be expanded to provide greater details on how the BSI was replicated using the HSIA data.
We have expanded the methodology section such that the methods are organized into subsections about how we calculated each index. We have removed the BSI as we feel the conclusions drawn from results from the changes seen in the timeseries we extended can also be drawn from the newly-added indices in the updated manuscript. Please see also our responses for Reviewer #1, Question 13, and the supplementary section titled 'BSI interpretation' of Reviewer #2.

14. Are the number and quality of references appropriate?
Yes
We are glad to hear this, we have also added several references to the new manuscript as necessary based on the changes made.

15. Is the amount and quality of supplementary material appropriate?
Not applicable.

The paper relies on a limited and simplistic analysis to draw weak and poorly supported conclusions. The authors appear to acknowledge the limitations of their analysis, but precede to venture into topics far removed from their analysis. For example, the discussion of sea ice change on migratory marine mammals was quite disconnected from the analysis of local ice conditions near communities. Also, in terms of analyzing the "days left for whaling", the authors conclude that the spring ice-based whaling season has been cut in half (from 160-180 days to approx 80 days), when in reality the spring whaling season has never been much more than late-April through early June (<60days). (I comment more on this in my more detailed attached comments.) Little effort was made to address the simplification of their assumptions, although they point it out themselves in several cases (for example, by noting that their analysis is not sufficient to track the presence of landfast ice). As further example, the authors pointed 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 on complex impacts to communities.
We have changed the manuscript substantially from the submitted version based on the reviewers' comments. Please see our responses to similar concerns expressed in the main comments of Reviewers #1 and #2. These are at the top of the response to Reviewer #1 titled 'Overview and major comments', and in the start of the supplementary comments for Reviewer #2. We also appreciate this reviewer's comment about the whaling figure. We address this also in the detailed comments below. What was meant in the original manuscript by the whaling figure is that, due to the earlier break-up timing offshore Utqiagvik, the spring whalers now have less time for hunting from the sea ice. We have decided to remove this figure from the manuscript due to this reviewer's comments. In its place, we now focus on the increase in the number of windy days seen offshore Utqiagvik which we speculate will impact the fall whale hunt (Figure of this index is given in response to Reviewer #2, Question 3, and is also present in the new manuscript). To determine what is deemed 'too windy' to hunt via boat, we have used the wind speed threshold from Ashjian et al (2010), who used interviews from a number of whalers from Utqiagvik. We have used the newly added dataset (WRF-downscaled ERA Interim (Bieniek et al, 2016) in order to obtain the wind data offshore of these communities. We calculated the number of days where the wind speed threshold from Ashjian et al (2010) was exceeded during the open water period and presented a timeseries of this as a fraction of 'usable' vs 'too windy' open water

days for each community. Pertaining to the Reviewer's comment about the transition seasons, we have shifted the focus from the duration of the the transition season to the number of times the ice froze-up and broke out before the 'true' freeze-up and break-up. We have provided an interpretation of our results for this index in its own subsection in the Discussion section of the new manuscript.

We appreciate the reviewer's detailed comments provided below. Our responses are in orange.

Supplementary comments:

Abstract: "reduced access to subsistence hunting species" should be reworded. Its not the species that are doing the hunting.
This statement has been removed based on the substantial changes we have made to the abstract in light of the newly developed indices described above and in the responses to the other reviewers.

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.
The abstract now clearly links some main results to the impacts. For example, in Utqiagvik, there has been an approximate tripling of the number of wind events capable of significant coastline erosion from 1979-2014, and also an increase in the number of days too windy to be hunting via boat.

Pg 1, Line 20: Suggest changing to "while changes in the seasonality and extent influence the migration of..."
This statement has been removed because it does not really fit with the new changes in the manuscript. However, we have added a similar statement in the newly added subsection "Characterization of the communities examined" where Shishmaref is described, stating that Shishmaref "is at the center of animal migration routes and also a center of a complex food-distribution system based in subsistence hunting practices Marino (2012)."

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?
Key challenges of the place-based nature of climate change are that the impacts of climate are not felt equally across the globe, and that climate itself varies geographically around the Earth. For example, some places are getting drier while others are seeing more precipitation, and the associated impacts of these changes are what we meant by 'challenges of the place-based nature of climate change.' We agree that the second sentence can be seen as off-topic. In the new manuscript, both statements have actually been removed and the focus of that paragraph remains to explain the necessity of speaking with community members to understand what metrics are most important in the attempt at evaluating the impacts of climate change.

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.

We agree with the reviewers comment here, and have added an explanation to the added subsection in the Introduction titled “Identification of metrics useful for describing climate change-related impacts on Arctic coastal communities”. We have cited Johnson and Eicken (2016) “Estimating Arctic sea-ice freeze-up and break-up from the satellite record: A comparison of different approaches in the Chukchi and Beaufort Seas”. The explanation we added that, while freeze-up and break-up timing can vary based on data source, it is at the same time important to evaluate the timing of each in such a way that can be applied across communities. Using a sea ice concentration threshold is one way of doing this with available data.

Pg 2, line 15: What is meant by rotten ice?
Rotten ice in this context means weak and partially melted ice. This explanation has been added to the manuscript.

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?
By “this type of metric” we were referring to the way the ice breaks up, as discussed in the preceding sentence. In terms of how we have made the argument that “incorporating indigenous knowledge allows the use of large-scale datasets to examine local impacts”, we have significantly extended this practice in the new manuscript by using thresholds identified by indigenous knowledge and applied that to our analysis of the large-scale datasets. This is described in more detail in our responses to Reviewer #1 titled ‘Overview and major comments’, and in our responses at the start of the supplementary comments for Reviewer #2.

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.
We agree with the reviewer comment that this sentence is unclear. This statement now exists in the subsection added to the methods section, which has been added based on the comments by the other reviewers. We have reworded it to “with varying levels of dependence on subsistence activities, ~~such as~~ susceptibility to coastal erosion, and interaction with the offshore oil and gas industry.”

Figure 1: Does the grid cell in the Bering Strait overlap with the Diomede 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.
The grid cell in the Bering Strait is intended merely to show the resolution of the Historical Sea Ice Atlas in the offshore region. We included on the map the communities on which the present paper focuses.

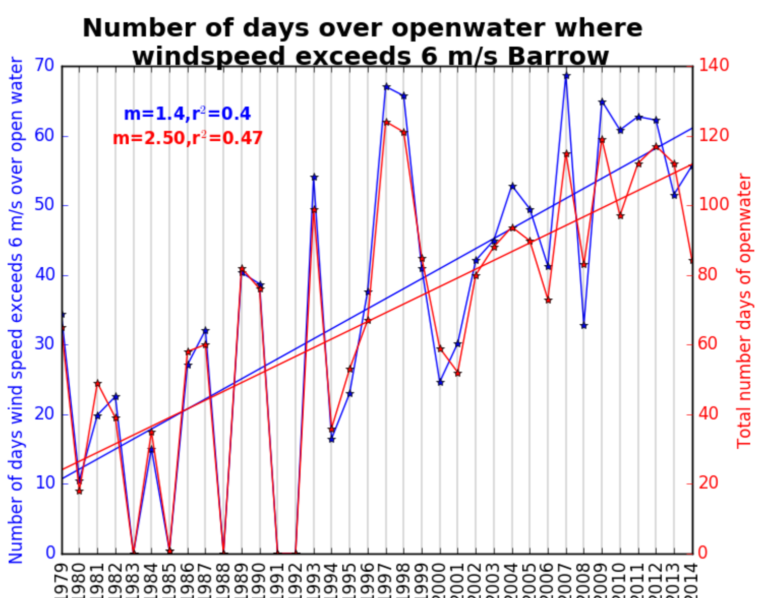
Pg 3, Line 4: Please reference the passive microwave satellite record if that is in fact what you are referring to.
We have changed the phrase “satellite-derived data” to “passive microwave satellite record”. This statement now exists in the reorganized subsection of the Methods labelled “The Historical Sea Ice Atlas”.

Pg 3, line 7: correct to “a different number of”
Corrected to as suggested.

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?
To answer the reviewer's comment we have added to this section: "The maximum concentration was extracted an not the average because if one grid cell in this area was higher than the others, it could serve somewhat as a 'choke point' or hazard while the other grid cells do not. In order to capture this, we decided to take the maximum value of these several grid cells, although the neighboring grid cells do not typically vary significantly in concentration. The entire sea ice cycle was examined."

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").
We appreciate the reviewer's perspective here, but feel that freeze-up does not necessarily imply phase change in this context. We have added the following statement to the manuscript in the newly-added subsection of the Methods titled "Indices related to freeze-up, break-up, and duration of open water period" in order make this aspect more clear: "The timing at which freeze-up and break-up concentration thresholds are crossed does not necessarily imply a phase change, but also can include advection of ice in terms of shifts in winds or currents."

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 where 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%.
Based on this comment and the other reviewer comments (see also Reviewer #1 comment about the Results section), we have decided to remove the timeseries of break-up and freeze-up timing for Utqiagvik. The open water duration is still covered for Utqiagvik (see Figure in response to Reviewer #2 comment in Question #3 of Interactive comments). Also, due to the significant changes in most of the indices evaluated in the new draft, we now have incorporated a separate timeseries figure for Utqiagvik given below. This shows the increase in the number of open water days, along with the number of days deemed too windy for a successful whale hunt (wind speed thresholds taken from Ashjian et al (2010).

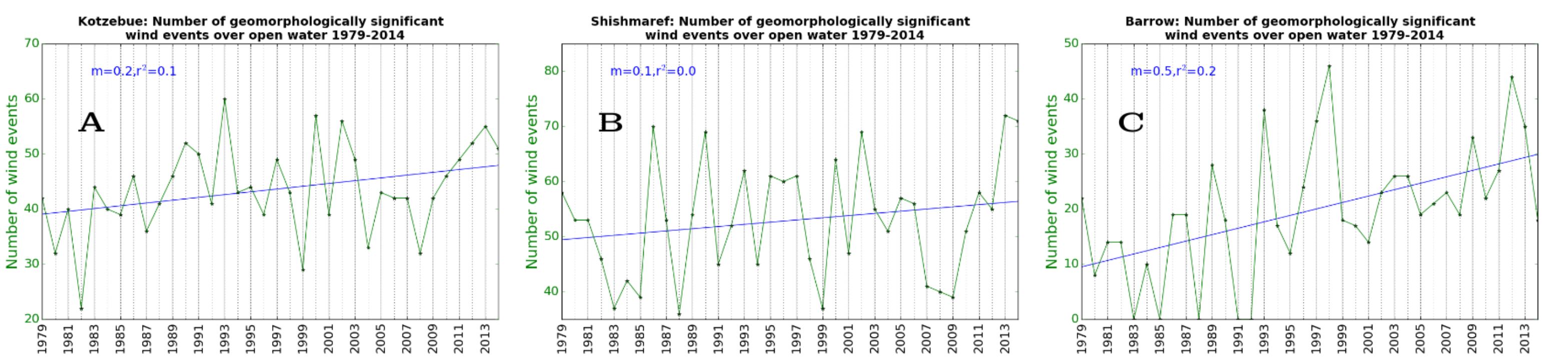


Pg 4, line 2: This paragraph is about freeze-up yet it references "break-up dates".
We have rearranged the organization of the paper based on the comments of the reviewers, and have combined explanations of the results pertaining to the freeze-up and break-up timing with the new results found in the change in the number of false freeze-ups and false break-ups.

Pg 4, line 7: The linear trend at Barrow for break-up was not calculated also. This should be stated.
As mentioned in response to previous reviewer comments (for example our response to Reviewer #3's comment above about Pg 4, line 1-3), we have removed the freeze-up and break-up timing trends for Barrow.

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.
We appreciate the reviewers comment and have reworded to: "Kotzebue shows more variability in the timing of freeze-up than Shishmaref, with 132% of the variance of freeze-up timing for Shishmaref, and 108% the variance of break-up timing."

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?
We have removed this figure because the impacts on the community are more thoroughly covered by the new indices present in the revised manuscript. The open water period was defined using a 15% threshold. These figures pertain to the number of days found to be too windy for hunting safely via boat, and also the changes seen in the number of wind events considered to generate significant enough waves to do geomorphological work (erosion), or damage to structure and habitats. The wind speed thresholds used for these were applied for open water, and come from the references of Ashjian et al (2010), Atksion (2005), and Solomon et al (1994). The change in days "too windy" for subsistence hunting via boat is given in the Figure in our response to Reviewer #2, Question 3. The change in the number of geomorphologically-significant wind events are given in the figure below, which is also included in the revised manuscript.



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.
We have removed this figure from the manuscript based on the reviewers comments. As mentioned previously, what was meant in the original manuscript by the whaling figure is that due to the earlier break-up timing offshore Utqiagvik, the spring whalers now have less time for hunting from the sea ice. In its place, we have focused on the increase in the number of windy days seen offshore Utqiagvik which we speculate will impact the fall whale hunt (Figure of this index is given in response to Reviewer #2, Question 3, and is also present in the new manuscript).

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.
We at present cannot find a referenceable source that demonstrates more hunting in fall, although we know anecdotally this to be the case. We welcome any suggestions of a citable source by the reviewer for this to be added in the paper. As explained in Ashjian et al (2010), and referenced in the new manuscript, fall hunting seems to be changing as the climate changes due to possible changes in the Pacific Water inflow into the Chukchi Sea (and along with it the euphasesiids the whales consume), as well as hunters are reporting they need to travel further from shore to harvest the whales because the whales are being deflected by increasing vessel traffic. This causes problems because the meat can spoil on the long tow back to shore. This as well has to do with the open water period extending into the fall storm season, and the northward shifting storm track, increasing in recent years the number of days reported to be to windy for hunting (see previous Figure). This discussion has been added to the manuscript in the new subsection titled “Increases in the number of windy days over open water and open water duration.”

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.
The statement referencing Clarke’s paper was unclear. We did not mean for it to reference changes in hunting, but changes in the pattern of the Bowhead whale migration. We agree that the statement is speculative and it was meant to be worded that way with the words “could be”. However we can see how this might be unclear. Based also on the reviewers comment below that too much focus is on changes in marine mammals, and also from the limited migration studies we have found (we are open to suggestion by the reviewer), we have decided to remove these statements.

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.
We have greatly shortened this section and the remaining content is now interspersed in the appropriate subsections. However, we still feel it is important to discuss the impacts of sea ice change on marine mammals due to their value for subsistence hunt. For example, in the new subsection of the revised manuscript discussing the increased number of false freeze-ups and false break-ups, we mention this could result in less time or a reduced snow cover on the ice. The latter is important because seals use snow cover for protection from predators. In addition, an earlier break-up (shown in the Figure referenced in the Results section of our revised manuscript pertaining to freeze-up/break-up timing), could lead to problems for the bearded seal, which require stable ice cover in late spring for raising pups and moulting (Kovacs et al, 2011).

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.
We have address this with the development of the new index: the number of false freeze-ups and false break-ups during the seasonal transitions between ice-covered and open water states, instead of the duration of each transition period. We have also added the reference of Serreze et al (2016) to justify our 30% sea ice concentration threshold. Please refer also to our response to Reviewer #1, Data and Methods section, and Reviewer #2, Supplementary comment #2.

Section 5.1: A great explanation of the methodology to recreate the BSI from the HSIA data should be included in the methodology section.
In view of the the comments of the reviewers, we have decided to remove the BSI from the revised version. Please also see Reviewer #1, Discussion comment.

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?
The BSI has been removed. The two figures have different scales because Figure 8 was meant to be a comparison between our HSIA dataset and the BSI calculated from the other sources. Since we had extended the dataset further than the other sources, we did not include those years in comparison. So yes, in essence, Figure 8 does correspond to a subset of earlier years.

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

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?
This statement has been changed so that it is more speculative. However, we did mention in this section that there has been an increase in maritime transit and an associated new recommended shipping route released by the US Coast Guard. We have modified the text to say that an increase in shipping of goods delivery could impact reliance on subsistence foods, especially in young people. While this speculation is only anecdotally supported, we believe it is still worth mentioning here.

We have also added the reference of the Arctic Marine Shipping Assessment (2006) which suggests increased shipping could enhance trade and reduce costs for Arctic communities, and increased development of resources can provide employment and income for Arctic residents.

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.
We appreciate the reviewers comments here, and have removed (as mentioned in the previous comment about the whaling Discussion section) the comments about any concrete number in the reduction of days left for whaling. We meant (and agree this was not adequately clear) that the trends seen of an earlier break-up leave less time in spring to hunt from the ice. We have now focused on the fall whaling season, where the open water season is expanding further into the fall storm season. This has consequences for the ability for whalers to safely travel by boat and impacts the number of whale landings (Ashjian et al, 2010).

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.
As mentioned in a previous comment, the focus on whale migration has been removed from the manuscript.

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.
The source cited for this statement about hunters is Ford et al (2006), which discusses changes in exposure-sensitivity under a changing climate. However, because the revised manuscript has changed, this statement does not flow well with the discussion of the added indices, so it has been removed.

We appreciate Reviewer #3's comments, which have contributed substantially to a more developed and clearer manuscript.