Reply to Referee #1

Impacts of a lengthening open water season on Alaskan coastal communities

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

We would like to thank Referee #1 for his/her constructive comments, which have helped improve the quality of the paper. We present below our detailed responses to the comments in blue.

1. Overview and major comments

This paper aims to use the Historical Sea Ice Atlas (HSIA) to assess potential direct and indirect impacts from sea ice change for 3 (or 4) Alaska communities. Unfortunately, the analysis described does not sufficiently support the paper's conclusions, due to a lack of focus and a clear connection to the primary analysis. The paper simply tries to cover too much ground. Further development, extension, and quality-control of the HSIA analysis is recommended, with additional explanation of the physical context of the sea ice environment at study locations, and more explicit linkages to community interaction and feedback regarding key impacts.

In order to provide more explicit linkages to community interaction and feedback regarding key impacts, we have introduced new indices that are based on community knowledge and experience, as reported in other studies. We have also drawn upon an additional data product (WRF-downscaled ERA-Interim winds) in the calculations of the new community-relevant indices. Specifically, the augmented set of indices includes: the number of 'false freeze-ups' (number of times ice concentration oscillated above and below the threshold value before true freeze-up was finally achieved), 'false break-ups', timing of freeze-up and break-up, the length of the open water duration, number of days where the winds are too strong to hunt via boat (wind speed thresholds from Ashjian (2010), and number of wind events capable of performing geomorphological work or damage to infrastructure from creation of waves and storm surge (using the definition of these wind events from Atkinson (2005) and Solomon (1994). By building our analysis around these metrics, we demonstrate how local and indigenous knowledge can inform use of a large-scale dataset to form locally-relevant indices describing change in sea ice conditions, which also leads to changes in the duration and strength of winds over open water.

Review metrics:

1. Does the paper address relevant scientific questions within the scope of TC?
Yes, but it is encumbered by side issues.
We feel that we have managed the side issues by refining the scope of the paper, but without more details about these side issues we cannot address this more specifically.

2. Does the paper present novel concepts, ideas, tools, or data?
Partly, but not fully realized.
We are not aware of any other study which has based a study on these indices for the communities examined in this study. We also feel that the newly added indices further demonstrate the value of a more generalized method to apply large-scale datasets to examine locally-relevant impacts of environmental change.

3. Are substantial conclusions reached?
No.
We have provided new conclusions about the recent decades (1979-2014) of the following indices for three communities: Number of false freeze-ups, number of false break-ups, number of open water days deemed too windy for offshore subsistence hunting, number of wind events capable of performing geomorphological work (erosion) or damage to infrastructure or habitats. From 1953-2013, we have also presented changes in the timing of freeze-up and break-up for the communities of Kotzebue and Shishmaref. In Utqiagvik, there has been an approximate tripling of the number of wind events capable of significant coastline erosion from 1979-2014. We believe that our documentation of changes in the various indices have led to new conclusions about environmental changes of dual relevance to Arctic coastal communities.

4. Are the scientific methods and assumptions valid and clearly outlined?
No – though if better explained and more thoroughly executed they may be.
We appreciate the reviewers comments and have expanded in the methods section. Specifically, 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 – the discussion section relies too little on the results of the analysis.
The discussion has now been organized into subsections, each of which builds upon results from the previous subsections, leading to conclusions about marine access and coastal vulnerability.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
Only in part. The method of recalculating the Barnett Severity Index (BSI) was not given.
Based on this and other review comments about the necessity of the BSI, we have removed the BSI (including its use and discussion) from the manuscript. Previously, we had cited Drobot (2003) for the methods of calculating the BSI.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
Mostly.
Without further specification of which related work this pertains to, we cannot address this comment in detail, but we have made an effort to clearly outline (in Section 2) how our indices were developed by using appropriate references that have used indigenous knowledge, especially in the revised methods section.

8. Does the title clearly reflect the contents of the paper?
The title is nominally accurate but the paper does not succeed due to overreach.
Our title in our original manuscript is “Impacts of a lengthening open water season on Alaskan coastal communities.” In our revision to the reviewers comments, we state how each of our indices relates directly to impacts on Alaskan coastal communities. Moreover, trends found in most of the indices are also directly related to the lengthening open water season. We have decided to add a subtitle such that now our title reads “Impacts of a lengthening open water season on Alaskan coastal communities: deriving locally-relevant indices from large-scale datasets and community knowledge”

9. Does the abstract provide a concise and complete summary?
Improvement is needed.
Since we have changed the focus of the paper from analyzing direct and indirect impacts to the development of the locally-relevant indices, and have changed the abstract accordingly.

10. Is the overall presentation well-structured and clear?
No.
We appreciate this comment, and have made a major effort to re-organize the manuscript such that each index has a corresponding appropriate subsection. The subsections are linked throughout the Methods, Results, and Discussion main sections.

11. Is the language fluent and precise?
Improvement is needed, especially toward the use of plain language instead of jargon.
We have reviewed the wording for jargon, but would appreciate more detail in order to address this comment. For example, we have added a description of what 'rotten ice' is in the Introduction section (partially melted and weak). We have also made an effort to be descriptive of the added dataset (WRF-downscaled ERA-Interim) for readers who are unfamiliar with this dataset.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
The organization of the paper is poor, it attempts to cover too much ground on the basis of too little new analysis, and it would be improved by substantially editing the discussion section down to those questions clearly related to the HSIA analysis. The quality of the figures is low.
We have removed the BSI in response to the "attempts to cover too much ground" comment, and we have sharpened the focus on the user-relevant indices. As mentioned in our response to comment #10 above, we have reorganized the Methods, Results, and Discussion into their own subsections such that each index we have developed can be clearly followed from how it was calculated from the large-scale datasets of HSIA and newly-added WRF-downscaled ERA-Interim dataset. The quality of the figures has been addressed by replacing some of them.

14. Are the number and quality of references appropriate?
References for recent sea ice research papers are lacking; references to similar studies on indigenous knowledge of sea ice for the St. Lawrence Isl. communities, for example, are incomplete.
We have now restructured the paper such that the references citing community-based knowledge that we have used to develop thresholds are a central component of the paper and clearly described in the Introduction and Methods. References pertaining to walrus harvest and how open water impacts the sea ice conditions (accelerates the break-up of shore-fast ice), have been added to the restructured Discussion section concerning the transition period between ice and open water. For example, the reference Krupnik (2002) has been added there.

15. Is the amount and quality of supplementary material appropriate?
N/A

Abstract:
The abstract could be more concise with the removal of some unnecessary phrases like ‘It is often remarked’ (Pg1 L1) which only weakens the statement that ‘Alaska coastal communities are on the frontlines...’ This statement has been removed. Also the phrase ‘navigational regime shift’ (Pg1 L11) is not the best choice given the freighted meaning of the term ‘regime shift’ in the physical sense, and further obscured by reference to ‘navigational’. One could simply state that the impact of ice conditions on vessel operations near Utqiavik (Barrow) have eased since … [because]. The term ‘regime shift’ here and elsewhere is an example of jargon that doesn’t add materially to the paper and should be avoided as it is not essential to the work at hand. The reviewers comment here refers to the shift seen in our extension of the BSI timeseries. As noted earlier, we have followed recommendations of the other reviewers and removed the BSI in the new version of our manuscript.

Introduction:
It would be helpful to mention the role of the Barnett Severity Index (BSI) since it is highlighted in the abstract. It is also reasonable to provide the specific date range of the study, and a general sense of the application of “community feedback and interaction” with the research team, or, if indirect, the sources of indigenous knowledge incorporated in the study (i.e. collected interviews of elders, joint review of archived interviews, previous published work, etc.). It may also be appropriate to mention work by I. Krupnik, for example, that is of a similar nature. The introduction would also be a good place to mention the specific communities discussed in the paper (note that the community of Wales is introduced in results section 3.2 for the first time). For this paper an organization statement in the last paragraph of the introduction would be useful. We appreciate the comments here and have changed the introduction substantially. As mentioned previously, the BSI has been removed from the new manuscript and so is no longer in the introduction. We have mentioned the work done by I. Krupnik (2003) which is highly relevant. The introduction is also where we have now clearly outlined the work of Ashjian (2010), Atkinson (2005), and Solomon (1994) which have each identified thresholds using indigenous knowledge necessary for developing several of the indices now presented in this paper which did not exist previously (such as number of days ‘too windy’ for subsistence hunting via boat, and wind events capable of doing geomorphological work or damage to infrastructure). We have also added a statement near the end of the introduction, outlining the paper’s contents and organization.

Data and Methods:
The structure and completeness of this section could be improved by separating and briefly addressing each element identified previously, for example: a) HISA and its underlying data and related issues/benefits for the specific period of the study (e.g. blend of surface and satellite obs, use of microwave data near the coast, impact of quarter-monthly resolution); b) sources of indigenous knowledge used in the study and how they were used; c) selection of study areas and reference HISA grid cell locations in light of (b) or otherwise; d) selection of metrics, as in 30% ice concentration threshold (Serreze et al. 2016 doi:10.1002/2016JC011977 provide a good rationale); e) BSI, which is absent in this section and the introduction but mentioned in the abstract – this is especially important as the BSI index uses sea-ice inputs only for the area north of Barrow and along the coast to Prudhoe Bay, hence its extension to distant study areas should be justified, and particularly given Barnett and Drobot’s cautions regarding use of historical data prior to ∼1978 and the impacts of potential ice loss on the utility of the BSI; losses that have now taken place. We have restructured the Data and Methods section as suggested by the reviewers comment here. It is now organized into subsections, which describe the datasets used (HSIA and WRF-downscaled ERA-interim products), the methods for selecting grid cells representative of each community and the derivation of locally-informed indices The Serreze et al (2016) reference has been added as per the Reviewer’s suggestion.

f) some brief general description of how direct and indirect impacts will be evaluated in light of results (e.g. erosion, travel for hunting, prey availability, transition season, as well as indirect ones) would be useful here. There are elements more appropriate for the introduction and/or discussion section included here that should be removed (e.g. line 13 and following). Since we have removed the BSI, lines 13 and following have also been removed. Based on this and the other reviewer comments on the need for a refined scope of this manuscript, we have shifted the focus of the paper from the differences between direct and indirect impacts, to the development of locally-relevant indices utilizing two large-scale datasets of HSIA and WRF-downscaled ERA-Interim. As such, a 'brief general description of how direct and indirect impacts' are evaluated is no longer relevant in this case.

Results:

The issue with Barrow in the first paragraph requiring an alternate calculation for break-up date should be explained in the methods section, and especially justifying the linear interpolation. It appears that in some years the ice never moved back sufficiently from the coast, and thus the interpolation leads to the inference of a condition that may have never physically existed. Inspection of the pertinent variables given by Barnett (1976) show 11 years (of 23 examples) 1953-1975 where the sea ice did not retreat at all (as measured in nautical miles), or not far enough to be detected in the reference grid box (esp. 1975). We have removed the linear calculation for Barrow because some years indexed did not have a true 'freeze-up and break-up event'. The number of false freeze-ups and false break-ups have been given for Barrow from the ERA-Interim dataset, as explained in the new version of the manuscript, and we feel this is a better representation of the change being seen there. Interpretation of these results depends on the physical context that helps explain why differences exist between the various study locations. For example, ice cover near Wales is influenced by the dynamics of the Bering Strait inflow, and local/advected solar heating in this and especially the Alaska Coastal Current (ACC). The ACC is not well observed by the Bering Strait mooring array (Woodgate 2012 and related papers) and should probably be considered by reference to additional sources. The Kotzebue area is dominated by fast ice, influenced by local solar heating, river discharge plumes and related freshwater stratification, and factors related to the ACC at the outer edges, and is probably less sensitive to dynamics (i.e. ocean currents, wind-driven flaw leads, etc.) until later in the break-up process, compared to other locations. The amount of open water in Barrow, until recent decades, was defined primarily by the width of the lead between the coast/fast ice edge and the heavy polar ice pack (e.g. Hunt & Naske 1977), which is in turn influenced by (among other factors) large-scale atmospheric patterns such as identified by Barnett (1976) and others since. An important factor at Barrow then may be the major loss of multiyear ice in the recent period, and accompanying changes in the mobility and strength of the ice pack. This is perhaps the most important distinction between Barrow and other locations where first-year ice has been the norm over the entire study period. The organization of the results section could be improved, perhaps by treating each phenomena as a sub-section, and by adding some analysis of the physical context required to understand why the observed differences may exist. This may well throw light on how communities' experiences and responses vary in the face of change to be discussed in following sections. Most sections may benefit from an opening remark explaining what follows and its organization (this is often done in the last paragraph of the introduction as well).We have responded to this comment by adding a detailed description in what we feel fits best into the Discussion subsection 'Increases in the number of windy days over open water and open water duration'. This description more clearly discusses the trends in the results section of the change in open water days, and includes the suggested comments about freshwater input from Kotzebue Sound and the loss of multiyear ice from Barrow. We have also added the reference (Section 4.2) of Ahlnas and Garrison (1984) which discusses how the warmest water during the summer in the Chukchi Sea occurs in Kotzebue Sound, and also the extent of the Alaska Coastal Current. We have added as the reviewer has rightly suggested organizational sentences to the start of each subsection, as well at the end of the Introduction.

Discussion:

The connections between the results of the analysis of HSIA data and the generally vague and unfocused discussion sections are unclear. Finding that there have been various changes in the timing of break-up, freeze-up, and open water duration is not novel in itself, though the impact on communities may be, if better explained by the specifics of the HSIA analysis here and its linkage to indigenous knowledge. We understand this could be interpreted as unclear, and feel that the addition of the locally-relevant indices in their own subsections (4.1, 4.2, and 4.3) makes for both a more connected and clearer analysis/discussion of the results. At points in the discussion the authors undermine their purpose with statements like: "Comments like this highlight the complexity of the transitions between open water and ice-covered seasons. It therefore might be problematic to simply use a sea ice concentration threshold to define the shoulders of the break-up and freeze-up transition seasons. We examined variability in the duration of the 0-30% period, but found no significant trends" (Pg 8 L1). This is rather discouraging to the reader. This statement has been removed in the discussion. We believe that the number of 'false freeze-ups' (number of times ice concentration oscillated above and below the threshold value before freeze-up was finally achieved) and 'false break-ups' compared between the communities examined provides a more robust representation of the changes happening in each community during the transition season. The BSI is introduced in discussion section 5, where the authors describe extending the index from 2000 to 2013, but do not explain specifically how or why (which should have been done in data and methods if this was an important component of the paper). Then they report that the original index and the extended index (HSIA BSI) don't agree very well and this may be due to basic incompatibility of the input data. The BSI is not mentioned again until one sentence in the conclusion, which begs the question of why it was in the paper at all. It would be interesting to know if the BSI is still used for long-range sea ice forecasting (its original purpose) or for other reasons, but its inclusion in this paper is not warranted. We agree with the reviewer, and the inclusion of the BSI is not necessary for the main points of this paper. We are able to demonstrate, without the BSI material, the use of local information to guide analysis of temporal variability based on large-scale datasets. The trend of the BSI also matches the number of open water days in Utqiagvik well, and since the latter has remained in the paper, we feel this is another reason the BSI should be removed for clarity and conciseness.

Conclusions:

Relationships between the specific HSIA-based sea ice analysis reported here and impacts of a lengthening open water season on Alaskan coastal communities are not clearly shown, especially with respect to community interaction and feedback.

Based on the comments of this reviewer and the other reviewers, we have shifted the focus of the paper to the use of metrics informed by community knowledge to assess local impacts of sea ice change using larger-scale datasets, as outlined in our response to "Overview and Main Comments" above. When viewed in the context of the temporal variation documented here, this can give meaning to how a changing climate is impacting Alaska coastal communities. We have changed the Conclusion section to reflect this, where we summarize our findings on the changes in these indices for each community examined.