Reply to Short Comment #1

Impacts of a lengthening open water season on Alaskan coastal communities

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

We appreciate the input by the two students from the class. We present below our detailed responses to the comments in red.

Summary of the manuscript

The authors have used the Historical Sea Ice Atlas (HSIA) to calculate a date for the break-up and the freeze-up of the sea ice for four coastal Alaska communities (Barrow, Kotzebue, Shishmaref, Nome) as well as for an area in the Bering Strait. The dates were calculated from 1953 to 2013 based on a threshold of 30% ice-cover. Based on this data a linear trend was derived to find a (possibly climate change associated) change in the timing of both freeze-up and break-up. Following this analysis, the paper reviews numerous potential interactions (direct as well as indirect) between the change in sea ice and the impacts on indigenous peoples.

Main Assessments

The study discusses a current topic related to climate change, namely the duration of sea ice cover. However, it remains unclear how these communities were selected and why the data for not all communities that were selected (p. 2, l. 30) are presented (figure 2 and 3) and discussed (results, discussion). Additionally, the methods are lacking in detail and statistical details are not addressed. A significant flaw is the lack of an evaluation of the trend line. As the trend line is the main result of this study, it requires an in-depth evaluation and a discussion that compares these results and this method to other studies on changes in sea ice cover. The discussion subsequently doesn't focus on the derived information from the HSIA (the trend line) but more on potential implications of the found changes for the people in those Alaska communities. These implications are based on a literature review, which makes the manuscript two sided and overcharged in information variety. Further the BSI is introduced too late and only covers a short part of the study which poses the question if it is really needed or useful. In summary, the paper in its current state is unfocused and lacks detail in key sections.
The intended focus of this study is the development of climate-related indices from large-scale datasets using local and indigenous knowledge that directly relate to impacts on Alaska coastal communities. We acknowledge that we may have placed undue prominence on the trend lines of open water duration without the appropriate level of discussion later in the manuscript, as the reviewers suggest. In the revised manuscript, we introduce additional data products (WRF-downscaled ERA-interim reanalysis fields) and develop additional indices that capture aspects of recent sea ice changes relevant to coastal communities. This allows us to expand our discussion of the observed trends and improve the overall balance of the text by more effectively linking our discussion of community impacts with the results of our analysis. Please also see our responses to the main comments of the other Reviewers #1, 2, and 3 in response to the students comments about the BSI index.

General Questions.

Does the paper present novel concepts, ideas, tools, or data?

Yes, using the HSIA a historical record for the break-up and freeze-up of sea ice for four Alaska communities is presented. However, the literature review (mainly in the discussion section) does not present new findings.

We thank the reviewers for this insight. In the revised manuscript, we believe that our discussion is more focused on the results of our analysis, which now comprise an expanded set of locally-informed climate indices.

Are substantial conclusions reached?

No

Please refer to our responses to Reviewers #1-3, labeled as Question 3. 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.

Are the scientific methods and assumptions valid and clearly outlined?

Not fully, since the methods section lacks a statistical evaluation and later sections (results and discussion) further highlight methodological concepts that were not introduced nor discussed.

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. Please refer to our responses to Reviewers 1-3.

Are the results sufficient to support the interpretations and conclusions?

Not fully, since the derived trend line is not statistically evaluated.

We have added some significance values to the subsection 3.1 Please also see our responses to the comments of Reviewers 1-3, labeled as Question 5.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Not at all.

We have now provided further methodological details regarding the datasets used and the indices derived from them. We have also excluded any reference to the BSI, which we acknowledge was not described with appropriate detail.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes

Does the title clearly reflect the contents of the paper?

No, the title mainly focuses on one aspect of the article (literature review in discussion). A better title would include the derived estimate for a change in the date of freeze-up and break-up. For example, it could be: 60 years of historical ice cover data reveal a significant shift towards a longer open-water season.

We thank the reviewers for this insight, but we feel their suggested title does not adequately capture the intended thrust of this work, which is the application of large-scale data to assess local impacts of changing sea ice and climate for Arctic coastal communities. Instead, we have appended a subtitle to our title, which now reads “Impacts of a lengthening open water season on Alaskan coastal communities: deriving locally-relevant indices from large-scale datasets and community knowledge”

Does the abstract provide a concise and complete summary?
Yes, although there is potential for improvement.
Please see our response to Reviewer #3, Question 9. 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.

Is the overall presentation well-structured and clear?
Partly; owing to the fact that the article presents a mix of a data analysis with a literature review concerning potential impacts for the local communities. A comparison of the changes in sea ice with actual impacts for the communities or potential impacts for four different communities would have been interesting but the impacts are all discussed in a very general and theoretica/hypothetical way. The discussion section is unnecessarily long and does not focus on the actual work done by the authors. Furthermore, the figures are of low quality.
We have refined the scope of the paper such that we clearly outline very specific indices, each of which we provide a timeseries for the three communities examined. The structure now includes these in subsections which are connected in the Methodology, Results, and Discussion sections. We feel this adds much better flow to the paper. We have restructured the Discussion section such that each sub-section (4.1, 4.2, and 4.3) correspond with their own index in the results section, which we believe improves the organization of the manuscript.

Is the language fluent and precise?
Yes

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
Mostly
The manuscript contains no mathematical formulae or symbols and the only units used are days and percentage ice concentration (technically a unitless ratio). However, we have now ensured that all abbreviations such as ERA (European Centre for Medium-Range Weather Forecasts Reanalysis) and WRF (Weather Research and Forecasting model) and now explained.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
Yes, to all of the above mentioned sections.
We believe we have addressed all the reviewers comments above.

Are the number and quality of references appropriate?
There are no major references to the methodological part of the paper and almost no references to other studies on sea ice cover. The literature review on the other hand seems to have a good basis of literature.
The Methods section of the paper has been changed substantially. In essence, we have cited a reference for each index we derived a timeseries for.

Is the amount and quality of supplementary material appropriate?
NA

Major review points
Abstract
The term “subsistence hunting” could be introduced once, and then it should be clear that the indigenous people of Alaska rely on the availability of game for their food supply, henceforth it would not be required to always indicate it again (also true for the whole article).
We have added a definition to the Introduction section where the term first appears, to provide a background to those not familiar with communities in the Arctic.

Maybe the section that explains the HSIA is not necessary (defining it here as a historical atlas of sea ice cover would be enough).
We have reduced the description of the HSIA in the abstract, such as the resolution of the dataset. However, we respectfully disagree that paraphrasing the name of the dataset we used in the study is valuable for the abstract in terms of describing the study.

Introduction
P.1, L.14 and 17: If the focus is on food security (rather than the impact on coastal communities in general), why is the first example given related to soil erosion? Soil erosion is likely not the main impact on food security.
The introduction has been reorganized, such that the first statements now read: “When trying to downscale large-scale climate observations, complex inter-connections between communities and the environment can often be overlooked (Huntington et al, 2009). We therefore recognize the importance of incorporating local knowledge in understanding and quantifying the impacts of such direct changes.”

P.1, L.15: the reader is introduced to the terms “direct” and “indirect impacts”. However, for the indirect impacts, the term “globally-induced impacts” is also used. In the conclusion again the term indirect impacts is used. We suggest to use the same term in throughout the manuscript.
We have decided to shift the focus of the paper from the differences between direct and indirect impacts, to the new focus of the development of indices defined by thresholds from local knowledge. The term ‘globally-induced impacts’ does therefore not make an appearance in the new manuscript.

P.1, L.21: Why not cite peer-reviewed literature?
We are not sure what the reviewers are referring to at this line. Between lines 20-22, there are 6 different citations to peer-reviewed literature.

P.2, L.3: It is not quite clear what the term “place-based nature of climate change impacts” refers to. A short description or explanation would help.
Yes, we agree that this statement could have been expanded on to make this more clear. Please see our response to Reviewer #3 about this statement.

Methods
P.2, L.27: the term “best analog representations” was used. What does this mean? For a better understanding of the HSIA it is crucial to know what is meant with “analogs”.
Based on the multiple data sources, analog representations are only used when we need to fill spatial and temporal gaps of the given month. In other words, analog representations assume no large jumps in the sea ice concentration between the first and the last timestep of the HSIA dataset. This does not take place for the majority of the dataset, but for the gaps between 1953-1973. Our analysis extends

from 1953-2013. Further clarification has been added in the subsection 2.1 “The Historical Sea Ice Atlas” in the revised manuscript.

For this study four communities and one offshore area were chosen. The manuscript states that the communities were chosen because of their “wide range of sea ice regimes, with varying levels of dependence on subsistence activities [...]”. For a better understanding of the communities it would be useful to have a short site description for all of them including the reason why a particular location was chosen and further reference to the map in figure 1. Also, it is not clear why this particular location in the Bearing Strait location was chosen.
The Bering Strait is a relatively small area and the sensitivity of our sea ice concentration does not vary much between the grid cells, as there are only about 3 grid cells spanning this with our spatial resolution of the HSIA dataset. We have added a description of each community we chose to focus on in a separate subsection of the Introduction. Please see also our response to Reviewer #2, Comment #1 in the supplementary section.

P.3, L.3: it would be useful to support this claim with a reference to a date for which satellite data would be available (also add a source for the date).
We do give when satellite data is available in the statements in that same paragraph: “One caveat to including multiple data sources is that there are a different the number of observations that have gone into each time segment of data. For example, the frequency of ship-based of observations was not consistent throughout the record and the number of available observations increased dramatically with the advent of the satellite era. However, with regard to the latter, we do not find evidence for anomalous discontinuity in trends around 1978-79, when the passive microwave satellite data are incorporated into the dataset.” However, based on the reviewers comments, we have also added the 1979 date to the statement now where we describe use of the HSIA data roughly doubles the timespan we can evaluate when compared to datasets which only use passive microwave satellite data.

P.3, L.10: first a concern is raised that the data is heterogenous, and then it is only partly explained why and how the HSIA is nevertheless a good source. How did you test if there was an anomalous discontinuity?
We looked for anomalous discontinuities in the dataset by looking for large jumps in the dataset for multiple variables, around the time when the satellite data was introduced. We have also added further description to the analog representation in subsection 3.1 in response the reviewers comments.

P.3, L.16ff: it is not clear how the area was selected, was it done manually? How was the calculation then conducted? Overall this section lacks detail and the results are not reproducible.
The area used to extract the sea ice concentration data was offshore the communities, each covering the same area for the communities which is a 50x75 km box near the coastline. These grid cells are highlighted in Figure 1. We have added a separate subsection 2.3 in the Data/Methods section of the revised manuscript which we feel addresses this comment titled “Selection of grid cells representative of each study area”. We have added also in the methodology section 2.4 and 2.5 clear descriptions of how we calculated each index in our revised manuscript, since we have added several since the last version.

P.3, L.24: the reasoning to why the 30% threshold was chosen is not clear, if 15% does the same as 30%, then why chose 30%? What did other people do to evaluate freeze-up and break-up of ice cover from gridded data?
Please see our response to Reviewer #2, supplementary comment #2.

Overall: No information on statistical analyses that were used is given. Figure 2 / 3 hint at the use for linear regression, but that is the only information the reader gets from the article. The low "% variance explained" suggests that these trendlines may not be significant? Did you do a statistical test to determine if the trendlines are significant? Also, why was a linear trendline chosen? Does a stepwise (two-part) regression fit the data better?
A linear trend line was chosen because it is a common way to show rates of change over climatological timescales, especially for sea ice decline. To be consistent with this and to apply trends across multiple locations, we therefore do not think a stepwise regression is appropriate, even though it might 'fit the data' better. The significance of the trend lines is included in sections 3.2 and 3.3 of the new manuscript.

Results
Generally, this section relies heavily on the linear trend, although the linear trend is not mentioned inthe methods nor is its quality assessed.
The results section of the revised manuscript contains 3 subsections (3.1, 3.2, 3.3). These sections focus on the results of the timeseries of the indices developed, and we have added statistical values to each of these subsections. Also relevant to this comment by the reviewers is our responses to Reviewer #1 Supplementary comments titled "Results." We have removed the freeze-up and break-up trends for Utqiagvik, based on the discontinuity in the number of physical freeze-up and break-up dates starting from the beginning of the dataset toward more recent years.

It would be good to have a table (similar to Table 1) with all important information and statistical measures (not just the % of explained variance).
Since the main focus of the manuscript is demonstrating the use of indigenous knowledge in conjunction with large-scale datasets, we feel a high focus on statistical values is beyond the scope of this paper. We are also not sure what the reviewers mean by "all important information" and would need clarification on that to address this comment further.

The presentation of the Results is incomplete:
- The studied community "Nome" is only mentioned in the Methods. Is there a reason why it is not presented in the Results and Discussion?
Nome is also presented in the Results of the original manuscript (Section 3.2), put we have decided to remove analysis from this community and focus on the three communities of Utqiagvik, Kotzebue, and Shishmaref. Please see also our response to Reviewer #2, supplementary question #1 titled "Community uses of sea ice".

- In Figure 4 suddenly "Wales" appears, without introducing it before.
Please see our response to Reviewer #2, supplementary question #1 titled "Community uses of sea ice".

- Figure 2 and Figure 3 do not contain the results of "Nome" and "Bering Strait". After the introduction of the four communities and one offshore area in the methods, it is necessary to show all results or at least state why something is not shown / presented.
We are consistent in the new manuscript such that each index (with the exception of Utqiagvik for freeze-up/break-up dates) is calculated for each community. Please see our response to Reviewer #2, supplementary question #1 titled "Community uses of sea ice".

- P.4, L.9-15: This focuses on the explained variance by the linear trend, however it is just a qualitative description and raises more questions than it answers no statistical evaluation!
We feel a qualitative description of how much variance is explained by the linear trend is a meaningful value that should be included in the manuscript, but as mentioned above, we feel a detailed statistical analysis of each index developed is beyond the scope of this paper. We have added significance values to sections 3.2 and 3.3.

- In section 3.2, lines 21-24 are already interpretation and should be moved to the discussion.
We have restructured the Discussion section such that each sub-section (4.1, 4.2, and 4.3) correspond with their own index in the results section, which we believe improves the organization of the manuscript. Please also see our response to Reviewer #1, supplementary question titled “Results”.

- Figures 2 and 3: The data for Kotzebue flips back and forth between two values in the 1960s and early 70s. Is this a data limitation issue?
The dates in question are prior to the passive microwave satellite data, and as explained in the Data/Methods section 2.1 “Historical Sea Ice Atlas data”, there is an inconsistency in the frequency of data sources included in this time period. We believe this is the reason for some of the jumps seen in the timeseries prior 1979.

- Figure 4: top 1%, means that for each period a different threshold is chosen. That makes comparisons of the different periods difficult. Also what are the methods used to determine the number of storms? Add a reference.
This figure has been removed from the revised manuscript, as we feel the index timeseries “Number of geomorphological significant wind events” calculated for each community provides more information in this context. Nonetheless, in the previous manuscript, the reference is given in the caption of the Figure, as well as the method to calculate the number of storms, where the top 1% is an accepted way to calculate extreme events such as storms.

Discussion
- The quality of the produced data is never questioned nor is it assessed. How robust are the results?
The data is directly derived from observational datasets. The use of satellite-derived sea ice concentration data is a well documented way to obtain sea ice concentration data.

- P4L26: These are at most potential impacts. Unfortunately, the real impacts are never determined or analysed. So change title to reflect that these are literature based potential impacts.
We do not believe that the well-documented case (e.g. Barnhart et al (2014), Overeem et al (2011)) that the Arctic coastline is more vulnerable to erosion due to sea ice loss should be labeled as a ‘potential impact’, as the reviewers suggest. Coastal erosion is a very real threat to Arctic communities, for example the case of Shishmaref twice voting to relocate their entire community due to their quickly eroding coastline. We expand on the impact of sea ice loss in terms of providing more open water during the fall storm season which allows for more waves to develop. Section 3.3 “Increasing number of wind events with potential for geomorphological change” in the Results and Section 4.3 “Increasing wind events over open water: Number of geomorphologically-significant wind events and consequences for erosion” discuss this in the revised manuscript. An example of one of our results is that the number of wind events over open water that are capable of causing significant erosion or damage to infrastructure and habitats (Atkinson, 2005) has roughly tripled for the case of Utqiagvik from 1979-2014.

- P.5, L.5ff: the manuscript refers to Kotzebue Sound which “shows less of a change in freeze-up and breakup trends”. This could be because it is surrounded by land on three sides. But what about the community of Nome which is on a similar, but not so distinct, location and shows the same trend in figure 4?
Please see our response to Reviewer #1, supplementary comment “Results”.

- P5.L34: Is there any evidence for that? Any data? any references?
There is anecdotal evidence that hunters are increasingly relying on larger boats with outboard engines. However, we have not found any peer-reviewed citations of surveys or something of the sort to cite that statement. This statement also does not appear in our new manuscript.

- P7L21: If this has been reported, add the reference.
Please see our response to Reviewer #3, Question #15.

P8.L3: This statement should probably come much earlier.
Please see our response to Reviewer #2, Supplementary Question #2.

P.8 (section 5.1): the manuscript starts again with an introductory part, then a description of the methods, results and discussion. For a clear structure however, a separation into the correct chapters would be necessary. Within this section the Barnett Severity Index (BSI) is very briefly introduced. Here the reader should get more information about the BSI. What is it? Why is it used? The BSI is only mentioned again in the first paragraph of the conclusions and is thus a minor part within the manuscript. Thus, one should introduce it properly and then also show its importance.
Please see our response to Reviewer #1, supplementary subsection “Discussion”.

The discussion has a major focus on the impacts. However, there are other aspects as for example species extinction that could be more dramatic with more open water days. Further the protection of species in danger from extinction could lead to conflicts with traditional hunting.
The reviewers are correct that sea ice loss poses a significant threat to certain ice-dependent species, some of which are also subsistence species for coastal communities. However, we feel that issues related to co-management of endangered or threatened species is beyond the scope of this manuscript.

Conclusions
- P.10.L7-9: We agree with this but what is the relation between this statement and your actual study/analyses? The four different sites are not discussed in detail and it remains unclear how different these communities are or how different the impact of climate change and changes in sea ice has been for these communities.
We have added a subsection in our Introduction section (1.2 “Characterization of communities examined”) which discusses our study areas, typical sea ice cycles of each, and sea ice use.

- P.10, L.13: the usefulness of the BSI is described. This statement would be more logically placed together with the rest of the BSI, currently in section 5.1.
The BSI has been removed from the revised manuscript. Please see the response to Reviewer #1, labeled “Discussion”

- So far, the direct impacts were always presented before the indirect impacts. On page 10, line 25ff the order was changed which is misleading. Always keep the same order.
We have decided to shift the focus in the revised version from the difference between indirect and direct impacts to the development of locally-relevant indices pertaining to the lengthening open water period, relevant for stakeholders in coastal Arctic communities. Please see the response to Reviewer #1, overall comments. This statement does not exist in the new manuscript.
- Most of the conclusion is actually a discussion and not a conclusion from the analyses presented in this study.
Please see our response to Reviewer #1, Question 10.

Minor points
-Some sentences are hard to read, e.g. P2L3-4, L20-21.
The P2 L 3-4 statement has been reworded. L20-21 is not present in the revised manuscript.
-P.2, L.29: Sea ice concentration = sea ice cover. Introduce this definition already in the introduction.
We have added to the introduction sea ice concentration is the fraction of open water covered by sea ice.
-Figure 1: Create a more useful and visually more attractive map. This map looks like it comes out of a video game of the 80s.
Please see our response to Question # 3, Reviewer #2. The reason we used this map was to highlight the resolution of the HSIA gridded dataset and feel that the map provides more information when presented in this way rather than with a finer resolution we did not use.
-The legends in figure 2, figure 3 are too small and do not contain the necessary information.
The figures with the legends are not present in the revised manuscript.
- Figure 5: don't just copy from Chapman, replot using the same style as the other plots. In general, all plots should have the same style, so also figure 4 needs to be adapted.
This figure is not present in the revised manuscript.
-Figure 8: add the 1:1 line
This figure is not present in the revised manuscript.
-Table 1: maybe it is better to give the trend in days per decade so that it is not a fraction of a day, which could suggest hourly data are needed
We feel that days/year is consistent with the other trends given throughout the manuscript (Results: Section 3), and have decided to keep as is.
-Regarding the figures in general: no titles needed, clearer figure captions needed (a,b,c...).
Also make sure to add informative legends.
P.3, L.8: the word "of" is not needed
We have looked over this line and are unsure which 'of' would be able to be taken out.
P.4, L.21: replace "has" with "could have" because we cannot be sure about this.
We are sure that changes in oceanic heat impact influences when the ocean is able to freeze.
P.6, L.25: "polynas" actually called "polynyas"
This is a typo and this statement does not exist in the new manuscript.
P.8, L.12: the introduction of the BSI is suboptimal. It is here introduced as "severity of ice conditions index" whereas the acronym itself translates to "Barnet Severtity Index". Only after the first introduction of the correct terminology abbreviations should be used.
As mentioned previously, the BSI has been removed from the new manuscript.
P.8, L.24: also give the 5 nautical miles in kilometers (in general only use one distance measure)
The BSI has been removed from the new manuscript and so this statement is no longer present in the revised version.
P.9, L.14: "will" or "can"?
This typo has been fixed in the new manuscript.