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.

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.

Are substantial conclusions reached?

No

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.

Are the results sufficient to support the interpretations and conclusions?

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

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

Not at all.

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.

Does the abstract provide a concise and complete summary?	
Yes, although there is potential for improvement.	
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.	
Is the language fluent and precise?	
Yes	
Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?	
Mostly	
Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?	
Yes, to all of the abovementioned sections.	
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.	
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).
- Maybe the section that explains the HSIA is not necessary (defining it here as a historical atlas of sea ice cover would be enough).

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.
- 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.
- P.1, L.21: Why not cite 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.

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".
- 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.

- 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).
- 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?
- 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.
- 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?
- 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?

Results

- Generally, this section relies heavily on the linear trend, although the linear trend is not mentioned in the methods nor is its quality assessed.
- 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).
- 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?
    - In Figure 4 suddenly "Wales" appears, without introducing it before.
    - 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.
- 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!
- In section 3.2, lines 21-24 are already interpretation and should be moved to the discussion.
- 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?
- 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.

Discussion

- The quality of the produced data is never questioned nor is it assessed. How robust are the results?
- 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.
- 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?
- P5.L34: Is there any evidence for that? Any data? any references?
- P7L21: If this has been reported, add the reference.

- P8.L3: This statement should probably come much earlier.
- 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.
- 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.

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.
- 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.
- 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.
- Most of the conclusion is actually a discussion and not a conclusion from the analyses presented in this study.

Minor points
- Some sentences are hard to read, e.g. P2L3-4, L20-21.
- P.2, L.29: Sea ice concentration = sea ice cover. Introduce this definition already in the introduction.
- 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.
- The legends in figure 2, figure 3 are too small and do not contain the necessary information.
- 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.
- Figure 8: add the 1:1 line
- 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
- 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
- P.4, L.21: replace "has" with "could have" because we cannot be sure about this.
- P.6, L.25: "polynas" → actually called "polynyas"
- 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.
- P.8, L.24: also give the 5 nautical miles in kilometers (in general only use one distance measure)
- P.9, L.14: "will" or "can"?