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The Cryosphere
Discussions

Interactive comment on "Impacts of a lengthening open water season on Alaskan coastal communities" *by* Rebecca J. Rolph et al.

Anonymous Referee #1

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Review of: Impacts of a lengthening open water season on Alaskan coastal communities by Rebecca J. Rolph, Andrew R. Mahoney, John Walsh, and Philip A. Loring

Summary:

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.

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Discussion paper

Review metrics:

1. Does the paper address relevant scientific questions within the scope of TC?

Yes, but it is encumbered by side issues.

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

Partly, but not fully realized.

3. Are substantial conclusions reached?

No.

4. Are the scientific methods and assumptions valid and clearly outlined?

No – though if better explained and more thoroughly executed they may be.

5. Are the results sufficient to support the interpretations and conclusions?

No – the discussion section relies too little on the results of the analysis.

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.

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

Mostly.

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.

9. Does the abstract provide a concise and complete summary?

Improvement is needed.

10. Is the overall presentation well-structured and clear?

No.

11. Is the language fluent and precise?

Improvement is needed, especially toward the use of plain language instead of jargon.

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.

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.

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. . .' 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.

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.

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 $\sim$1978 and the impacts of potential ice loss on the utility of the BSI; losses that have now taken place.

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).

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).

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).

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.

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.

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.

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.

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