

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

Anonymous Referee #3

Received and published: 1 December 2017

1. Does the paper address relevant scientific questions within the scope of TC? Yes 2. Does the paper present novel concepts, ideas, tools, or data? The ideas are important and novel, but are not well developed. 3. Are substantial conclusions reached? No. The paper draws conclusions (many of which may be inaccurate) from a weak analysis. 4. Are the scientific methods and assumptions valid and clearly outlined? No 5. Are the results sufficient to support the interpretations and conclusions? No 6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes 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

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the full scope of impacts that they attempt to address. 9. Does the abstract provide a concise and complete summary? Somewhat 10. Is the overall presentation well structured and clear? Somewhat 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. 14. Are the number and quality of references appropriate? Yes 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.

See my detailed comments in the attached PDF.

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Please also note the supplement to this comment:
<https://www.the-cryosphere-discuss.net/tc-2017-211/tc-2017-211-RC3-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-211>, 2017.