Interactive comment on **"The effect of changing sea ice on the vulnerability of Arctic coasts"** by **K. R. Barnhart et al.**

Anonymous Reviewer #2

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The authors thank the Anonymous reviewer for their thoughtful and constructive review. We have made changes to the text in response to their comments. Please find our comments in this document in italic. In addition, we have included a copy of the manuscript with the changes made during revision marked as one of the Author Comments.

General comments:
The paper presents useful pan-arctic data on the growing open-water period in the Arctic Coastal Zone and on the growing fetch at a particular site (Drew Point, Alaska, USA) on the Beaufort Sea. It argues that vulnerability to coastal erosion is most directly related to open water period. The data presented on ice condition and bluff height was also nice. There is undoubtedly a lot of good information in the manuscript.

Specific comments:

Section 5.3.1.
Work done by Chapman et al. (2004, US Army Corps of Engineers) suggests that, when calculating storm surge height, the drag coefficient increases due to the presence of sea ice. This would appear to be inconsistent with some of the work cited in the manuscript.

We could not find Chapman et al. (2004, US Army Corps of Engineers) but we could find a later report by Chapman et al. (2009, US Army Corps of Engineers) – this and other citations not in the original manuscript are at the end of this document.

Chapman et al. (2009) focuses on making storm predictions and itself does not report findings about sea ice drag coefficients, but does cite a number of papers in the section titled "Ice Concentration Correction" (page 20) that do report on sea ice drag coefficients (Birnbaum and Lupkes, 2002; Garbrecht et al. 2002). It is an oversimplification to state that the drag coefficient increases with the presence of sea ice – the research summarized by Chapman et al. (2009) indicates that the sea ice drag coefficient reaches a maximum at 50% ice coverage and decreases with both increasing and decreasing ice coverage. We have added text to the section "Sea ice control on waves and storm surge" that summarizes this research.

However, in this submission, we do not model surge or wave generation over sea ice. Instead we set the boundary of the surge and wave generation at the sea ice edge. Thus we have not made any changes to our modeling based on this comment.

Section 5.3.2.
The comparison of Drew Point wave measurements and calculations presented in the manuscript (Fig 10) would suggest that the fetch-limited equation for wave height (developed by the Coastal Engineering Research Center, CERC) is actually not very accurate in this situation.

I am led to wonder which wave processes, neglected by the CERC formula, might be responsible for the disagreement: wave refraction, wave breaking, wave setup, wave shoaling, duration of wind condition, etc. Alternatively, could it be that incorrect assumptions are responsible for the disagreement such as: the assumption of quasi-static conditions, the use of the Drew Point wind data to represent the ocean wind condition, the use of the 0.15 ice concentration contour to designate the sea ice edge, etc. Some discussion of the limitation of the equation for wave height should be presented.

We think that the wave modeling we have presented is reasonable. We have looked through a number of published papers that employ coupled storm surge and wave models (e.g. the well established ADCIRC + SWAN) and find that they report similar data-model agreement as "good" (Bunya et al., 2010; Akpinar et al., 2012; Hope et al., 2013). This word, however, is subjective and related to the intended application of the model.

For transparency, we included the scatter plot, which we think looks much worse than the time series of wave heights. As part of this revision we have added two standard statistics for data-model comparison (R^2 and RMSE). In addition we have added text in which we discuss where the model does poorly.

In the second part of this comment, you bring up a number of interesting points regarding wave processes that are neglected by the CERC formula and assumptions that we have made. Any number of these issues could lead to data-model disagreement. We have added text to the "Storm surge and wave model" section to include a more complete list of these simplifications and assumptions.

Also, it would be useful to include the equation in the manuscript.

We have elected not to include the wave field and storm surge model equations in this manuscript as they are fully spelled out in the supplementary material in Barnhart et al. (2014) and represent previously published material by Dean and Dalrymple (1991) and Dean and Dalrymple (2004) as well as the fetch-limited wave model of Coastal Engineering Research Center (1984). We have added reference to the section numbers and equation numbers to assist the reader in locating these equations. In the supplementary material of Barnhart et al. (2014) this material amounts to 27 equations, which we think is more than we should add to this submission. While we could include only the most critical equations for storm surge and wave modeling, these number roughly 8, a number we think is also too large to merit reproduction here. However, if the editor's would prefer that we add this to the text, we will happily oblige.

Section 5.3.3.
It is certain that coupling the surge and wave height calculation would in principle lead to improved calculations since that is what happens in nature. Hence, the discussion around this point could be made more concise. Given the weakness in the wave modeling with the CERC equation, the effort to determine whether wave height is "saturated" or not does not seem to be appropriate (if this wave equation is used).

We have shortened the portion this section that considers the benefit of coupling surge and wave models in response to these recommendations. However, we think that the question investigated in this section "is there a length that is less than a synoptic scale storm at which set up or wave heights no longer continue to increase?" is worth discussing. This allows us to better consider how changing sea ice impacts the nearshore environment. While we agree that coupling storm surge and wave height leads to improved calculations, we think that there is benefit in showing the results of the simpler case in which the wave height model is not coupled to storm surge. We also disagree that there is substantial weakness in the CERC equation. While there certainly is not perfect model-data agreement, considering the simplicity of the model, we think it preforms well.

Section 5.3.5.
The first two sentences in paragraph 2 (lines 23-25, p. 2298) are confusing.
"Over the 1979–2012 period we find 799 storms, only 28 of which set water levels up. Over this same time, we find 306 positive set up events."
The first sentence implies/states that there were 28 water level setup events. The second sentence implies/states that there were positive setup 306 events. It is not clear from the text how you get two different numbers for the same phenomena.

We apologize for the confusion. Here we are discussing two different phenomena. A "storm" is defined as a continuous time period with wind speeds in excess of 10 m s^{-1}. A positive set up event is defined as a time period in which the water level is set up – a definition that does not include any information about the wind speed or direction. We have added a sentence that clarifies this distinction.

Figure 13.
The plot of the distribution of directional fetch and modeled positive set up is compelling.

Thank you for the positive feedback.

Section 6.1.
It is uncertain how you determined that the increase in the open water season is the main driver for increasing coastal erosion rates. You note in various locations that there are multiple additional phenomena contributing to increased erosion rates including: increase in size of large setup events, increase in the frequency of positive set

up events, and increased water temperatures. Is it the case that the increase in the open water season is relatively large (i.e., the open water period has basically doubled in length whereas the other phenomena have not changes quite so much)?

We attribute the changes to coastal erosion to a variety of factors including the increase in the length of the open-water season, the increase in the size of large set up events, and changes in water temperature.

At the beginning of Section 6.1 we state:
"Changes in coastal exposure over the period 1979–2012 are primarily caused by changes in the duration of the sea-ice-free season (Fig. 12a) and the increasing positive extremes of set up magnitude (Fig. 13c)."

At the end of this section we state:
"Overeem et al. (2011) found that the increase in erosion rate over the satellite record tracks the average duration of the open water season. Thus the increase in open water explains most of the change in the coastal erosion rate. However, the increase in size of large set up events (Fig. 13c) and the increase in frequency of positive set up events (Fig. 14a) likely also contribute to the increased erosion rates observed at Drew Point. The water temperature is another factor for controlling erosion rates in ice-rich permafrost coasts, and is influenced by increasing the duration and the area of open water adjacent to the coast (e.g. Barnhart et al., 2014)."

p. 2287, line 20. Beaufort Sea is repeated twice.
Second instance of "Beaufort" removed.

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