Rebuttal letter manuscript “The effect of changing sea ice on wave climate trends along Alaska’s central Beaufort Sea coast”

Dear editor, dear reviewers,

On November 29, 2021, we submitted the following manuscript to The Cryosphere (TC) titled: "The effect of changing sea ice on wave climate trends along Alaska’s central Beaufort Sea coast" (MS No.: tc-2021-343). On January 25, 2022, we were informed that the open discussion was closed. We received positive feedback on the work done but both reviewers had several useful comments and suggestions. Below are point-by-point replies to all specific questions and suggestions. Attached you will also find the revised manuscript with the changes made to address the review comments tracked.

Kind regards,

Kees Nederhoff

RC1: 'Comment on tc-2021-343', Jim Thomson, 17 Dec 2021
General comments
This manuscript presents decadal trends in nearshore waves and sea ice along the Beaufort coast. Waves in deep-water are given by the ERA5 reanalysis product, and then the SWAN wave model is used to evolve those waves closer to the coast. The recent implementation of wave-ice interactions in SWAN is applied, with tuning provided by in situ observations. Strong increases in coastal wave exposure are determined.
We appreciate the time Reviewer #1 has spent going over the manuscript in detail and providing very relevant input. Below you find a point-by-point reply to all specific questions and suggestions

Specific comments
This study is a welcome application of a new modeling capability, with a worthy goal of down-scaling climate reanalysis products to assess changing conditions along the Arctic coast of Alaska. The manuscript mostly suitable for publication, but I have a strong concern about a mis-match between the study design and the interpretation of the results. This is not so much a fundamental flaw as distinction in which conditions the model can be applied. The term "nearshore" in the title and in the text is misleading, since this usually refers to the zone in which waves are shoaling and then depth-limit breaking occurs (< 10 m water depth). Although the calibration effort for friction and ice coefficients is impressive (and properly evaluated with separate data) the data are mostly farther offshore (> 20 m water depth); data that are closer inshore have a limited range of ice conditions from which to extrapolate.

The key issue is the role of landfast ice. Hosekova et al (GRL, 2021, https://doi.org/10.1029/2021GL095103) recently showed that landfast ice is not resolved by ERA5 and this produces large systematic biases in coastal wave exposure. Understanding and incorporating the results of this recent paper will be an essential revision to the present manuscript. The authors are clearly aware of landfast ice (lines 44-48), but their methods have not considered that it may be missing from the ERA5 ice product or that it might dramatically change the ice coefficients for the SWAN model (or if the IC4M2 parameterization can even be applied at all).

More broadly, the literature is clear that wave-ice models are only as accurate as the ice input (which is typically much lower accuracy than the wind or other inputs). Forcing a high-resolution nested SWAN model with low-resolution ice concentrations from ERA5 (at 0.5 deg) is problematic. The skill scores in the present manuscript are more indicative of offshore conditions; the nearshore validation is limited duration and scope. The present study is still valuable, given how little data is available in this region and the novel model application-- but it the extrapolation to nearshore wave climate must be more cautious.
We completely agree with Reviewer #1 that model skill has been assessed mainly in the offshore region due to data constrains. While we were aware of the importance of landfast ice and its potential effect in the nearshore zone, we were not aware of Hosekova et al. (2021) when submitting the first revision. Therefore, we have made several changes to the manuscript to more accurately state what is performed in this study and how a reader should interpret these results (L127-129, L509-L517).

Technical comments

Figure 1: Are the offshore stations (triangles) really buoys, or were these up-looking sonars? Either ways, they are mostly too far offshore to measure the effects of landfast ice. Also, I was not able to find these stations in a search at https://aoos.org/. Please provide more specific links or DOIs for each data product used.
Thank you for catching this: the 'buoys' were indeed upward looking sonars (ADCPs). The deployments labeled '_A, _B, _K, _V were funded and conducted by Shell Oil Company; these data were proprietary until recently. Some of these data can be located at ndbc.noaa.gov with a search for 'Shell Arctic Buoy', and with the remainder being processed for public release. Measurements obtained with the Sofar Spotters (last 4 rows in Table 1) were funded by BOEM in support of this study and will be publicly available on the AOOS site. We have rephrased this throughout the manuscript (L133-144, and Figure 1).

Figure 8: an energy-weighted centroid period might provide a better (and more physically relevant) comparison.
We acknowledge that there are several ways wave periods can be computed. In this study, we are limited to the T_{m01} since that is what was recorded from the Sofar observations. Peak wave periods from the offshore Shell observations were converted to Tm01 with the formulation of Goda (2000). We added this information in the revised manuscript (L220-225).

Figure 10: again, this trend from ERA5 does not represent landfast ice, which may significantly alter the signals right at the coast. The trend presented is valid as a regional product, not as a nearshore product.
We agree with Reviewer #1 and have therefore included in the caption of the figure a clarification on offshore ice concentrations and changed the analysis to only base it on offshore conditions (L347-351).

Line 352: there might not be any alongshore variability because the SWAN model is forced with ERA5 at 0.5 degree resolution.
Correct, we have a statement on the resolution in the Discussion section (L379-380).

Line 368: The canonical range of gamma from Battjes and Janssen (1978) is meant for solitary waves and other simplified cases. Field studies have demonstrated that gamma ~ 0.4 is much more realistic (Raubenheimer et al, JGR, 1996).

We agree with Reviewer #1 and have therefore included a statement on this in the Results section (L394-398).

Line 385: The increase in wave period is most likely related to increase fetches in the larger domain, which allow more wave development. The steady wave steepness is related, but probably it is more a consequence of the wave evolution with increasing fetch.
We agree with Reviewer #1 and have therefore included a statement on this in the Results section (L414-415).

Line 402: Worth noting that the reduction of wave power from offshore to the 10 m isobath is probably not dissipation, but rather refraction on the shelf (which conserves only the shore-normal component of the energy flux as waves propagate to the nearshore).
Yes – thank you. We have clarified this in the Results Section (L414-415).

Figure 14: This might be another place to demonstrate the [unresolved] effects of landfast ice. Most of the decrease in wave power (wave energy flux) in the cross-shore direction is probably just refraction on the shelf. Dissipation by depth-limited breaking would not occur seaward of ~ 5 m depth, and dissipation (attenuation) by ice is likely more uniform b/c of the coarseness of the ERA5 ice concentrations. Anyway, this figure could be modified to show the interplay of these processes. Another process to resolve would be the blocking of wave energy by the barrier islands.
We have performed a preliminary analysis for several sea states to quantify the importance of individual processes (i.e. refraction, dissipation, role barrier islands, etc.). The particular contribution varies by sea state. Figure 14 shows the cumulative wave power for the entire 41-year time period and it is therefore not feasible to redo the simulations to breakdown the relative importance of each process. However, we have included a statement on this analysis in the revised manuscript (L433-435 and L517-519).

Line 476: I disagree with this statement on multiple grounds. The quality and resolution of ice products is essential for representing a process that is dependent on ice. Not only do Hosekova et al (2021) show that landfast ice can persist for weeks (and even months) without appearing in ERA5, but also the type and thickness of ice can cause dramatic changes in the ice coefficients appropriate for accurate wave attenuation.
We agree with Reviewer #1 and have therefore altered the discussion section. In particular, we added the reference to Hošeková et al. (2021) stating that landfast ice is not resolved. Moreover, we discuss the model calibration/validation and that the scope of nearshore validation is limited (and absent during the ice season). Lastly, we included the probable overestimation of these results due the missing landfast ice and potentially paths in how to improve this (L508-515).

RC2: 'Comment on tc-2021-343', Anonymous Referee #2, 22 Dec 2021
This study investigates the wave climate change in Alaska's central Beaufort Sea coast based on high-resolution wave simulations. With ice decline, some wave climate change trends are identified. Before I can recommend the paper to be published, some unclear points should be addressed.
We appreciate the time Reviewer #2 has spent going over the manuscript in detail and providing very relevant input. Below you find point-by-point replies to all specific questions and suggestions

General comments:

I am not sure that I fully follow how model calibration was done.The authors compared the different friction formulations and coefficients. Even though, the authors have given references to those formulations. Instead of going to read the references, I strongly suggest that the authors give a general introduction about those formulations (like they did for the wave decay by ice). What is the main difference between those formulations?

We agree with Reviewer #2 of the value of adding more information on the bottom friction and have therefore added this in the revised version of the manuscript (L156-163).

L194-197: Why do the authors give the calibration information in the caption of Figure 4? Is there any logical connection with Figure 4?

We apologies for this formatting mistake. We have corrected this in the revised version of the manuscript.

L142-148: I suggest that the information about calibration here should be moved and merged with L230-235.

We agree that this was not the most logical location. However, we have combined this part of the text in the 'Methods'. Moreover, we rephrased several sentences to make it clearer what was done (L205-2018).

20% of the data were used for the ice season calibration, which is stated in L234. However, in L259, it said that "for all 13 observations". I am confused that how much data are used for the calibration.

We understand the confusion of Reviewer #2 and have therefore rephrased this sentence. 20% of the offshore data between 2007 - 2013 combined (1,439 time points) were used. These are however based on 13 observations in the offshore region.

The impact of air-sea temperature difference on wind growth is used in the model set-up. Which SST data did you use? ERA5? I would suggest that the authors give some discussion about the limitation of the SST in the marginal ice zone since it is an important data source for your simulations.

Reviewer #2 is correct with the assumption that ERA5 was used. We have clarified this more in the revised version of the manuscript (L192-195) and included a discussion on the resolution of ERA5 in the discussion (L500-515).

The finest model resolution is about 500m in the simulations. The wind forcing data from ERA5 is about 30km. Many small-scale wind variations cannot be captured by ERA5 in the marginal ice zone. I am wondering why did you use so coarse resolution wind data for so high-resolution wave simulations. At least some discussions about this issue should be added to the manuscript.

We agree with Reviewer #2. A similar point was brought up by Reviewer #1. We have therefore extended the discussion section to include this point (L500-515).

From my understanding, the wind sea is largely decayed by ice in the marginal ice zone. In the relative small domain areas, the waves are mainly dominated by swell during the ice season, is it true? If it is the case, I would assume that the accuracy of the simulations during the ice season is largely affected by the wave boundary conditions, right?

In this relatively small area, accuracy of the model computations is indeed strongly driven by offshore boundary conditions. Despite the 0.25° resolution ERA5, it is chosen here as a convenient tool widely used to study multi-decadal evolution of wind, wave, and sea ice conditions in the Arctic. It provides needed information on, besides offshore wave conditions, of wind speed and ice concentration. See for example, Figure 6 in which we varied the coefficient of the IC4M2 ice model. This shows how important the ice concentration is in decaying offshore wave energy.

Detail comments:

Figure 1: it will be easier for readers to get the water depth distribution if you add the topographer information in Figure 1 or Figure 4.
We agree with Reviewer #2 and have therefore added depth contours to Figure 4.

Figure 2: Give the information that the location of the ERA5 is shown in Figure 1. L110: “110^oN” to “110^o”; “75^oN” to “110^o”
We have changed the caption of Figure 2 in order to correctly describe the coordinates of the ERA5 wind rose.

Figure 3: Are the data shown in the figure the area average?
This data is based on to offshore point and is averaged per day. The spread in the data shows the variability from year to year. See the caption of Figure 3 for more information.

Section 3.1.1: the authors use the ERA5 information in the previous section and figures. The introduction of ERA5 data is in the later section.
That is correct. Due the lack of observational data, we have used ERA5 for the site description. This is not ideal since, as observed by Reviewer #2, however we felt this was warranted due to the limited other sources available to base this on.

L120-121: the resolution of the wind field is not 0.5 degrees or did you use 0.5degree resolution wind for the wave simulations?
We used the highest resolution available from ERA5. That is 0.5 degrees for the wave conditions and 0.25 degrees for the atmosphere. We have clarified that in the revised version of the manuscript (L123-124).

L125: Did you use mean wave direction or peak wave direction?
We used the $T_{m0,1}$ throughout this study and have clarified the wave parameters and their computation in the revised version of the manuscript (L220-225).

L129: Foggy island Bay -> FIB
We have changed this and used the acronym instead.

L138: What are Tp and Dp? Peak wave period and peak wave direction?
We have changed this and written the different parameters out.

L145-149: Which domain is used for the calibration?
All model domains were utilized for the calibration and validation. We have clarified this in the revised manuscript (L210).

L175-178: Give more information about the \Omega. How does Omega is used in the wind input term?
We added an example in the Materials and Methods section (L181-182).

L213: n->N

We are not sure where Reviewer #2 is referring to and have therefore not been able to address this comment.

L215: “wave height”->” significant wave height”; “wave period”->”mean wave period”, etc
We agree with Reviewer #2 and have therefore used the symbol and introduced the parameters in the new ‘Wave parameters’ section.

Figure 5B: What is T_{m0}? You use T_m as the mean wave period in the above text.
We added more information in the Methods section (L220-225).

L254: Table 2->Table 3
Changed the number in the revised manuscript.

L259: Table 2->Table 3.
Changed the number in the revised manuscript.

L260: In Eq 2, there are six coefficients. Here, why only two coefficients for the calibration?
We understand the misunderstanding and therefore clarified in the Materials and Methods section that previous work has only used c2 and c4 and excluded the other coefficients (i.e. others coefficients are zero)

L254: data in 2007 or 2019 (caption in Figure 6)?
It should be 2007. In the revised manuscript we have revised the caption.

L265: What is 5% PI in figure 6C?
That is 5% exceedance probability. We have included this in the caption of Figure 6 in the revised manuscript.

L284: Figure 7B is wave period or wave height? The ylabel shows H_s, I think it should be Tm.
The caption of the figure is incorrect. We are presenting two timeseries of wave height. In the revised version of the manuscript we have adjusted the caption (L312-314).

L285: I am confused about the data. In the title of the figure, they are 2020A and 2020B+C. In the caption, they are #0519-1 and 0519-2. If you check the Table1, 2020A is #0518.
We understand the confusion of Reviewer #2 and have therefore changed the caption in the revised version of the manuscript (L312-314). The title of the subplots is the name of the observation. Upper panel (A) is 2020A (Spotter #0518) and bottom panel (B) is 2020B and 2020C (#0519-1 and #0519-2). See Table 1 for an overview of field measurements.

L334: Table 3->Table 4
Changed the number in the revised manuscript.

L354: Table 3->Table 4
Changed the number in the revised manuscript.

L370: How did you identify the storms?
An analysis identified per calendar year, the annual maximum wave height of the year and associated storm date. The result is a list 41 annual maximum wave heights and associated storm date per calendar year.

L379 mean wave period or peak wave period?

Mean wave period. We have elaborated on this the Methods section of the paper.

L389: mean wave direction or peak wave direction?
Mean wave period. We have elaborated on this the Methods section of the paper.

L397: Table 3->Table 4
Changed the number in the revised manuscript.

L400: How did you calculate the wave power? Based on wave spectrum or bulk parameters?
We have added the equation for wave power in the revised version of the manuscript (L239).

Figure 14: Please give the location of the transaction in one figure.
We agree with Reviewer #2 and have therefore labeled this transect in Figure 1.

L408: Table 3->Table 4
Changed the number in the revised manuscript.

L470: Liu et al. 2016 -> Liu et al. (2016)
We made this correct in the revised version of the manuscript.

L495: the in this study-> in this study the
We made this correct in the revised version of the manuscript.

Editor initial decision: Start review and discussion, Bin Cheng, 24 Nov 2021

General comments
Dear Nederhoff and co-authors
Thank you for your submission to TC/TCD. As you may know, papers accepted for TCD appear immediately on the web for comment and review. Before publication in TCD, all papers undergo a rapid access review undertaken by the editor and/or reviewer with the aim of providing initial quality control. It is not a full review and the key concerns are fit to the journal remit, basic quality issues and sufficient significance, originality and/or novelty to warrant publication. As a result, even a manuscript ranked highly during access review can receive a low ranking during full peer review later. Evaluation criteria are found at www.the-cryosphere.net/review/ms_evaluation_criteria.html. Grades are from 1 (excellent) to 4 (poor).

ORIGINALITY / NOVELTY (1-4): 2

SCIENTIFIC QUALITY / RIGOR (1-4): 3

SIGNIFICANCE / IMPACT (1-4): 2

PRESENTATION QUALITY (1-4): 1

The manuscript examines the Arctic coastal wave climate trends associated with the sea ice condition. The research subject fulfils the scope of TC. The analyses are robust, and the results are convincing. I believe at this stage the manuscript can be posted in TCD without large changes.

However, I have a few scientific comments on this manuscript, and I hope authors may consider them during the revision:
We appreciate the time the Editor has spent going over the manuscript providing very relevant input. Below you find a point-by-point reply to all specific questions and suggestions.

1) The paper entitled "The effect of changing sea ice,,, wave climate,,," which is a very brilliant starting point for a multidisciplinary study, however, I see the large part of the manuscript dealt with wave climate, the quantitative linkage to the changing sea ice is some weak. I would like to see more discussions on the impact of sea ice on wave climate.
We understand where the editor is coming from. We are a group of scientists with different backgrounds (coastal and atmospheric sciences), however the manuscript focusses more on the simulated climate trends and less on the air-sea interactions and feedback processes. The latter is definitely of interest and importance and therefore we have included this in the discussion section (L518-526).

2) For example, what do we know about "sea ice think" Sice? How it can be linked with sea ice concentration (SIC) or even sea ice thickness (SIT)?
In this work, we have applied, calibrated and validated the equations from Rogers (2019) within SWAN. In the correct implementation, the sea ice sink term is scaled with ice concentration but not with ice thickness. We have included this information in the revised version of the manuscript (L178-179).

3) P6, 125/130 "offshore significant wave height (Hs), mean period (Tm), and direction (Dm)": If I understood correctly, you applied those outputs from ERA5 to the SWAN model, if this is the case, please discuss how significant those parameters affect the wave climate calculated by the SWAN? in particular Hs, since it is already a wave model parameter?
The editor is correct, we have used the outputs of ERA5 to force the SWAN model. In deep water, the trends are relatively similar between ERA5 and our results. This is because the (offshore) boundary conditions dominate the results. However, in the nearshore, a depth-induced saturation was found which deviates from the trends found in deeper water (L395-399). This was one of the main findings of the work.

4) P6, 140 "(e.g., Hs, Tp, Dp, etc.)" : what are the etc. please speak out or remove it.
We have clarified the wave parameters and their computation in the revised version of the manuscript (L220-225).