The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-343-RC1, 2021

Comment on tc-2021-343

Jim Thomson (Referee)

Referee comment on "The effect of changing sea ice on wave climate trends along Alaska's central Beaufort Sea coast" by Kees Nederhoff et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-343-RC1, 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.

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.

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.

Figure 8: an energy-weighted centroid period might provide a better (and more physically relevant) comparison.

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.

Line 352: there might not be any alongshore variability because the SWAN model is forced with ERA5 at 0.5 degree resolution.

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).

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.

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).

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.

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.