Review egusphere-2024-2104

The paper estimates wave energy attenuation rates from preprocessed IS2 tracks around Antarctica. It discusses the advantages and caveats of such an estimate and provides the wave attenuation coefficient as a function of non-dimensional MIZ distance. It paper is well-written, has a clear outline, and derives plausible attenuation coefficients of wave energy in the MIZ around Antarctica. It provides evidence that wave attenuation is a function of frequency. However, a few major points must be raised after reviewing the manuscript:

1. The paper uses a preprocessed data set from Brouwer et al. 2022 and Fraser et al. 2024 that is based on the ATL07 product. One ATL07 segment contains the median (or mean) of 150 photon retrievals, which leads to varying segment lengths depending on the photon density of the data. The paper re-samples the segment heights of varying lengths to 8m, which can lead to substantial aliasing of the wave signal, especially in marginal ice zones where the photon retrieval can be below, but wave amplitudes are high. That is not discussed.
   In addition, the most recent versions of ATL07 filter the wave signal on purpose to remove surface wave effects, as they are considered noise for sea ice products. The use of other, lower-level products would be necessary when this analysis is applied at scale. (see Issue 3 in version 6 if the data release: https://nsidc.org/sites/default/files/documents/technical-reference/icesat2_atl07_atl10_known_issues_v006.pdf)
2. Sample uncertainty
   In one same paragraph (L89ff), they describe the section length L as 128 data points and/or a section length of 2048, 8192, or 16384 meters, but an L with 128 data points with 8-meter spacing results in 1024 meters. This needs to be clarified.
   Further, they say the choice of L is arbitrary but then acknowledge that the scatter between estimates is "reasonably large" (L179). A better quantification and accounting for how many samples one would need per wavenumber for a good estimate would strengthen the paper. The impact of ice edges and step-like changes in sea ice height on the FFT is also entirely ignored, which can substantially impact the estimated wave spectra when doing FFT (Hell and Horvat, 2024).
3. angle projection uncertainty
   The authors establish the projection of the true wave number k_a on the observed wave number k through the incident angle \theta but then state that they follow the "common assumption" of waves propagating in the direction of the IS2 tracks. The authors argue that the circumpolar nature of Antarctica favors waves in the north-south direction. I'm afraid I have to disagree with the argument, and I would like to see evidence for that statement. Think it is the most common that the incident angle must be addressed when measuring spectra from IS2 because the main wind/storm direction is east-west. The

median zonal wind direction over the southern ocean is about 5 m/s, while the mean meridional wind direction is near zero. In other words, it would be surprising if the dominant incident wave energy comes from the north in line with IS2 tracks. We would expect a mean wave direction going southeastward, leading to systematic biases in the wavenumber estimates. The wave climate may also substantially vary by region as the southern ocean storm has a clear climatological pattern (Hoskins and Hodge, 2005, for example). The assumption that wave travels in the north-south direction might be common, but there is evidence that using this as a general assumption for analyzing IS2 around AA is wrong.

Section. 3.2:
The study then tries to access the impact of wave direction on the attenuation rate using some test cases using Sentinel-1 SIC as a tracer for the \alpha. What is \alpha_p? This variable is not introduced. Further, while it is plausible to model variation in α as a function SIC, the functional relation between α and SIC is not given.

The authors realize that even slide incident angles will create different wave amplitudes along the transect due to the strong sea ice heterogeneity along the propagation path. While I share their statement about the resulting uncertainty of the wave attenuation rates, I don't follow their argument about negative attenuation rates, i.e., wave energy growth. Without energy input, wave action can only stay constant or decay with distance into the sea ice; however, here, the quantity used is wave energy, which can increase while the action is conserved. This can be done by wave-current or wave-sea ice interaction (Squire, 2018, or similar). The discussion about "negative attenuation rates" is not very physical without adhering to these wave actions and is confusing to the reader.

3. Sample tracks
   section 3.3: Here, the authors try to give a best guess of the overall attenuation rate from all transects derived in Brouwer et al. The Authors hope that the noise (or randomness in direction) cuts down enough that the mean is a reliable estimate of the attenuation rates. They do not discuss or quantify: a) how many transects they use, b) how those are spatially distributed, or c) what criteria are chosen to select these tracks. It is then questionable if this dataset is a representative sample of wave attenuations around AA, even though the text suggests that. This section needs more context on how representative this sample is. The reader would need to use two other papers to get that information —Brouwer et al derived 304 tracks in 4 months of one year. Given the amount of data IS2 provides, this is then a small test set of data samples with substantial uncertainties in the underlying metrics.
4. Unknown error due to unknown other metrics
   The attenuation rates are estimates in the normalized distance x/x_miz, while how x_miz is derived is not described. (likely defined in one of the other papers). This metric is

important because of the misalignment and sampling uncertainties, the attenuation rates will also depend on the robustness of the x_miz measure, as this appears in the denominator of \alpha. Uncertainties in x_miz can have a large impact on the estimated attenuation rate.
From section 3 in Brouwer et al. the x_miz is based on co-aligned daily SIC products of 6.25km depending on the total length of x_miz (not given, but often less than 50 km). Could this lead to additional substantial biases in the attenuation estimate? The authors should be more explicit about what is done here and what the impacts are.

Despite the paper's shortcomings, the paper provide evidence that attenuation rates vary with frequency and with distance in the MIZ. These are new finding for IS2 observation but not in general (Meylan et. al 2014, Thomson et al 2021, MONTIEL et al. 2022, and a few others). In summary, even though it is novel to derive attenuation rates from IS2, this paper has methodological flaws that leave questions about the accuracy and validity of the estimate, as discussed in other publications already (Hell and Horvat, 2024).

If this paper wants to describe a new method for calculating attenuation rates, the stated concern give reason why this method is problematic; if the paper wants to derive actual attenuation rates for later use, it needs to qualify its sampling and give reason why this estimate is robust.

additional remarks

- I would reword the statement in L.6 that this samples "over a wide range of sea ice condition".
- L 161: "completely different attenuation rates" - that is strong wording, I would remove that.
- L 244: How do we see wave-current interaction in figure 4? I don't follow
- Fig.8: the coloring choice is unfortunate.

References:
Hoskins, B. J., and K. I. Hodges, 2005: A New Perspective on Southern Hemisphere Storm Tracks. *J. Climate*, **18**, 4108–4129, https://doi.org/10.1175/JCLI3570.1.
Squire, V. A., 2018: A fresh look at how ocean waves and sea ice interact. *Phil. Trans. R. Soc. A.*, **376**, 20170342, https://doi.org/10.1098/rsta.2017.0342.