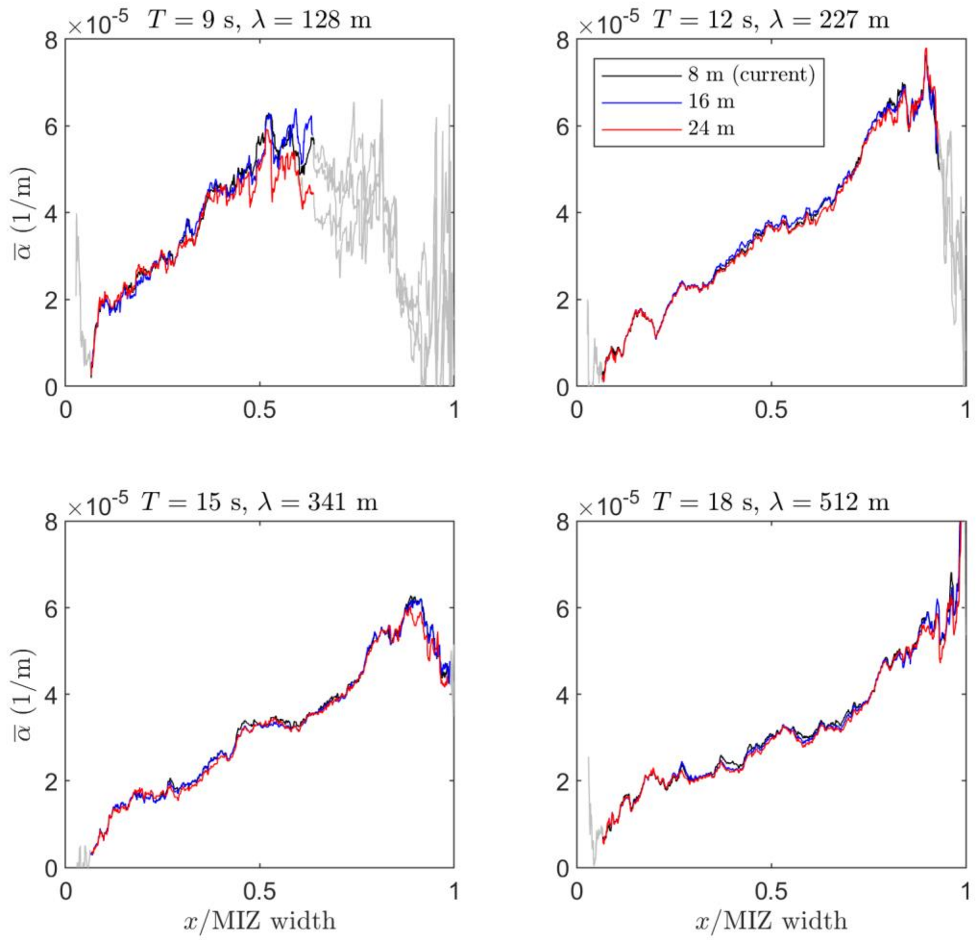
Reviewer 2

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/technicalreference/icesat2_atl07_atl10_known_issues_v006.pdf)

Based on the reviewer's comment, we have sub-sampled the 8 m resolution signal to a resolution of 16 and 24 m to identify the impact of the sample length on the average attenuation rate $\bar{\alpha}$. We refer to the figure below for the comparison for different wavelengths. As the observed differences are very small, we don't think that the issue highlighted has implications for our results nor the interpretation thereof.

We are also aware of the changes to the ATL07 filter now unfortunately excluding the MIZ. We suggest that SlideRule (https://slideruleearth.io/) could be used to produce heights similar to the earlier ATL07 releases, although we have not yet investigated this.



2. Sample uncertainty

In one same paragraph (L89), 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.

We have used 128 sample points (1024 m) as the window, but used longer section lengths L to obtain smoother statistics. We acknowledge that this may not be clear in the manuscript and will rephrase the corresponding sentences.

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.

Indeed, the choice of L is arbitrary, as per the reasons discussed in section 2. The scatter referred to in L179 is not necessarily related to the arbitrariness of L, but more likely to the variability of sea ice conditions between ICESat-2 transects.

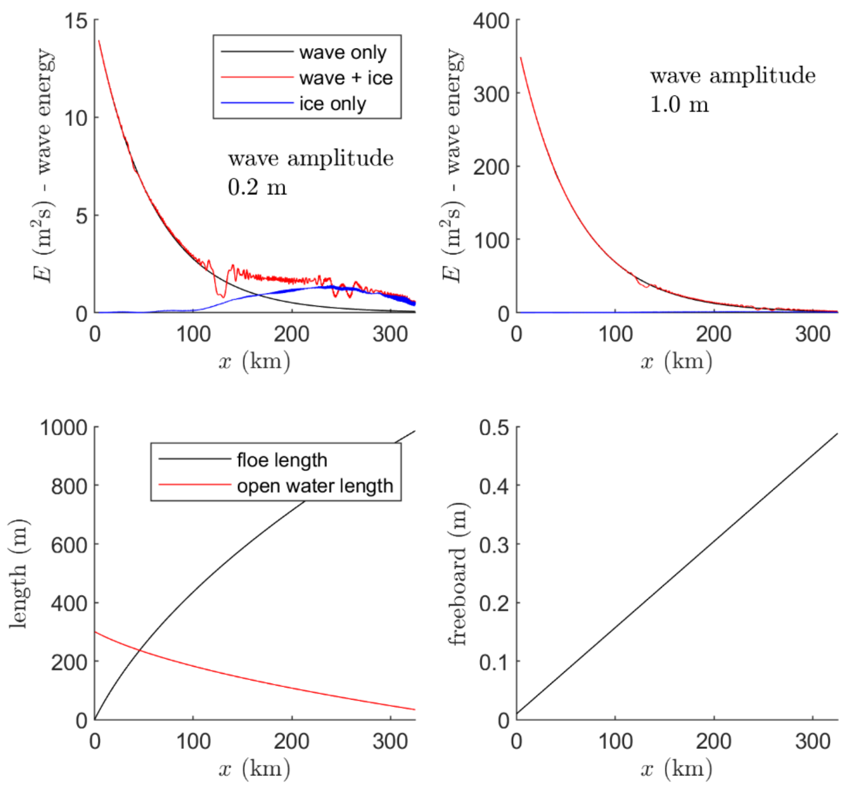
For the impact of L on the results we refer to Fig. 1c in the manuscript, specifically, comparing the red and blue lines. Steep changes in wave energy around x=90 km causes loss in spatial detail due to increases in number of samples per wave number. This is, however, not everywhere the case, and an optimum number of samples is therefore unlikely to be a fixed number. Without ground truth observations of the wave spectra or wave attenuation, it is unlikely that a scientific based optimal number can be provided for the quantification of wave energy.

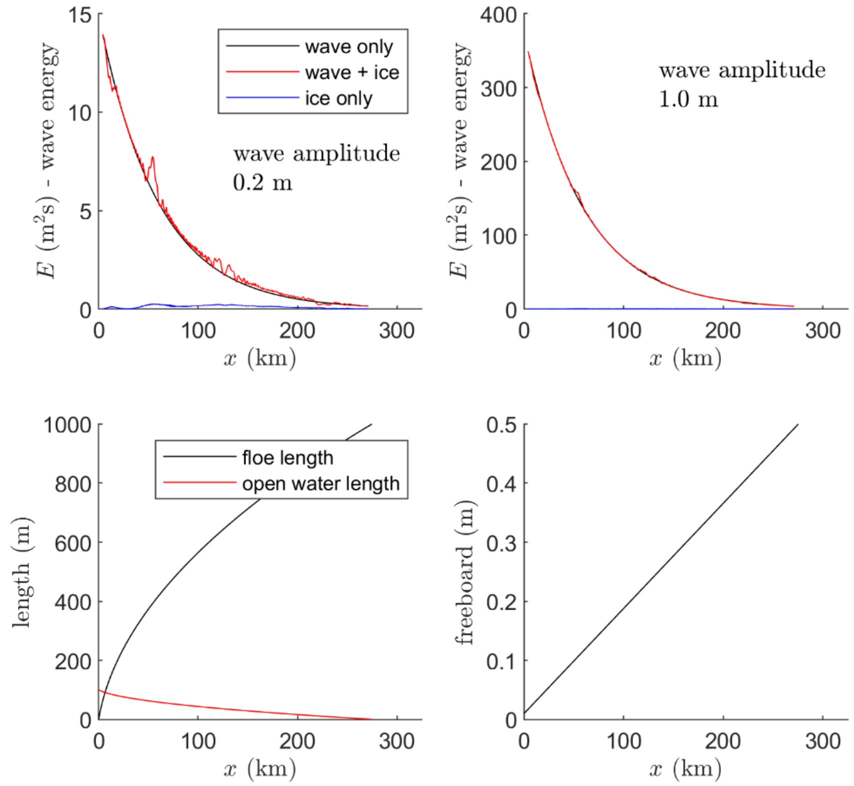
The impact of ice edges and step-like changes in sea ice height on the FFT is also entirely ignored, which can substantially impact the estimated wave spectra when doing FFT (Hell and Horvat, 2024).

In this study we adopted the definition of the MIZ as proposed by Brouwer et al., (2022). In physical terms, this definition may be interpreted as the position where the surface elevation variance transitions from wave dominated to ice dominated. We expect that if the sea ice freeboard (or any other feature of sea ice) introduces substantial errors in the estimates of the wave spectra, this will be outside the MIZ as defined by Brouwer et al.

We did a simplified test to see what happens to the estimated wave energy with distance into the MIZ by taking a simple monochromatic wave and adding artificial sea ice contours on top. We approximated the sea ice by means of 500 ice floes, where the length of the floes increases and the open water between floes decreases with distance into the MIZ. The ice thickness was assumed to increase linearly with distance into the MIZ from 10 cm to 5 m, and the freeboard was assumed to be 10% of the ice thickness.

In the two figures below, we show two different sea ice cases. For the first, the distance between ice floes varies between 300 and 30 m, the second it varies from 100 to 0.1 m. We can see that only for the first, and for very low incoming wave energy, the sea ice has impact on spectrum estimates. In the top left subplot, we may see a strong deviation from the expected curve (wave dominated to sea ice dominated variance). In the other cases, we don't see clear deviations from the wave only case, and as such, we don't expect differences in wave attenuations estimates. For the only case that is impacted, such a transition would have been marked as the end of the MIZ. While this analysis is based on a major simplification of sea ice, we don't expect that sea ice freeboard has significant impact on our results given the currently used definition of the MIZ.





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.

We agree with the reviewer that wave energy travels along the north-south direction may not necessarily be true. Unfortunately, it is not straightforward to obtain such information. As discussed in the same section, we acknowledge that wave hindcast data may provide guidance but comes with uncertainties by itself, where different datasets can provide distinctly different values. Ultimately, only in-situ observations can provide ground truth estimates of the associated uncertainties and biases, as discussed in Section 4.

To acknowledge the potential bias from the missing wave incident angle, we will edit the corresponding paragraph to improve clarity on this matter.

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.

In Section 3.2 we aim to show that the misalignment between the incident wave angle and ICESat-2 transect, and sea ice heterogeneity can lead to apparent negative attenuation rates. These are not physically real (i.e. wave growth), but methodological artefacts. Waves measured at different positions along the transect may have experienced critically different sea ice conditions, which may appear as negative attenuation rates when processing the data, but they are not (necessarily) physical.

In L150 we define α_p as the proxy attenuation rate. The functional relation of α_p is not parametrically provided but its values can be distilled from the colour bar above Figure 4a. The reason why it was not provided is because SAR backscatter images are a poor estimator of the true local attenuation rate and to avoid general adoption in future studies. Particularly, the choice of α_p is merely to demonstrate the methodological artefact of negative attenuation rates that may appear in the attenuation curves derived for individual ICESat-2 transects.

However, to improve readability of this section we will provide the functional relation in the main text and the considerations of the relation in an appendix. Further, to avoid confusion in the interpretation of this section, we will rephrase parts of section 3.2.

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.

We acknowledge the reviewer's sentiment that more data is better. Nevertheless, we believe that 304 tracks provides a large enough dataset to derive trends in wave attenuation (i.e., see uncertainty bounds in Figures 6 and 7). There is, however, a seasonal bias in the observations as we have considerably more transects available from May and September (see section 3.4).

We note that the number of observations are provided throughout the manuscript, and like to refer the reviewer to Fig. 5a, where in red (see right axis) the number of observations are provided. In Figures 6 and 7 they are provided in the subplots (b).

To provide an overview of the spatial distribution of the observations, we will include a map of the Antarctic continent and the distribution of the corresponding transects as a figure in the manuscript.

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.

The MIZ is defined by means of the depth of wave penetration into the sea ice and has a significantly finer resolution than that of SIC products. While this is briefly mentioned in Section 2, we acknowledge that the manuscript can improve with further clarification on this definition.

Additionally, the majority of the transects considered here are from May and September, where the MIZ is about 200 km wide. An error of 6.25 km in the ice edge position is unlikely to cause considerable bias.

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).

The authors respectfully disagree with the reviewer's view that the findings are not new in general. Neither Meylan et al. 2014 nor Thomson et al. 2021 identifies nor discusses explicitly wave attenuation rates as a function of distance into the MIZ, or any parameters that may be considered to be strongly correlated to x/x_{MIZ}. While Montiel et al. 2022 discusses changing attenuation rates in terms of sea ice concentration, this is not necessarily reflective of x/x_{MIZ}. Additionally, in Montiel et al., no straightforward variability of α and sea ice concentration is found.

Whereas it is well known that wave attenuation tends to increase with sea ice thickness, sea ice concentration, floe size etc, and each of these are likely correlated with the distance from the ice edge or latitude, to the best of the authors knowledge, there are no experimental studies available that show this quantitatively.

In addition, to the best of the authors knowledge, this is the first study to use the ICESat-2 observations to obtain frequency-dependent attenuation rates, to obtain attenuation rates across large spatial scales and across the entire Antarctic, and to observe seasonality of the estimates attenuation rates.

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".

We respectfully disagree with the reviewer. As this study looks into more than 300 cloud-free tracks, and capturing sea ice conditions across the MIZ, we are confident that the dataset captures a wide range of sea ice conditions.

L 161: "completely different attenuation rates" - that is strong wording, I would remove that.

On the suggestion of the reviewer we will remove 'completely' from this sentence.

L 244: How do we see wave-current interaction in figure 4? I don't follow

We will rephrase the corresponding sentence to clarify.

Fig.8: the coloring choice is unfortunate.

Based on the reviewer's suggestion, the colour of lines will be changed.