Reviewer 1

In their manuscript „Finely-resolved along-track wave attenuation…", Joey Voermans and coauthors perform an analysis of the apparent wave attenuation in the Antarctic MIZ based on satellite (ICESat-2) derived along-track profiles of wave energy $E(x)$ for four selected wave frequency bands (corresponding to wave periods of 9, 12, 15 and 18 s). From the original data, published earlier in Brouwer et al., 2022, the Authors extract 320 high-quality $E(x)$ profiles and, assuming exponential attenuation, for each of them compute the corresponding profile of the attenuation coefficient $\alpha(x)$. Based on that data, several statistics of $\alpha(x)$ are computed, including the average $\overline{\alpha}(x)$ for all profiles and for individual months, as well as relationships between $\alpha(x)$ and ice thickness, concentration and x/x_{MIZ} (i.e., the distance from the ice edge scaled with MIZ width). An important part of the analysis is related to the influence of spatial variability of sea ice and wave conditions, and of non-zero angles between the satellite tracks and the wave propagation directions on the observed shapes of $E(x)$.

1. My major comment on the manuscript is this: I fully agree with the Authors that they have at their disposal "a unique dataset of waves in sea ice obtained across a diverse range of Antarctic sea ice conditions" (lines 222-223). Going from a few (in many cases: only two) data points to high-resolution profiles of wave energy is a big step forward. The new dataset makes it possible to find new patterns in the data, but also to verify assumptions that have been commonly used so far in analyses of wave attenuation in sea ice. However, to fully use the potential offered by the new data, new approaches are necessary – whereas my overall impression when reading the manuscript was that the Authors tried their best to make their study methodologically as close to the previous ones as possible, and to keep all old assumptions untouched. Neither the individual $E(x)$ profiles nor the final, average attenuation $\overline{\alpha}$ are exponential. In particular, none of the $E(x)$ curves presented in the figures resembles an exponential curve; in some cases, like e.g. in Fig.1a, the $E(x)$ profile is concave rather than convex, so that the exponential approximation is particularly inadequate. Still, the Authors decide to "use of the commonly adopted assumption that wave energy decays exponentially" (line 105): their analysis begins with piecewise approximation of $E(x)$ with exponential functions. First, as just said, even a quick look at the data is enough to say that this is a poor choice, and second, given the very high spatial resolution of the profiles, no a priori assumptions regarding their shapes are necessary. To the contrary, the shapes can be found as a result of the analysis. Several interesting questions could be answered this way. For instance, is there a one type of function that provides a satisfactory fit to the majority (or a large subset) of the analyzed profiles? Is the fit generally better in the inner MIZ than close to the ice edge? Which function provides a good fit to the average energy profile? Is it really exp(-bx2), as the analysis based on exponential attenuation suggests? If yes, how does this function perform for individual profiles? In previous studies, assumptions regarding the shape of E(x) were necessary given the large spacing between data points, and the choice of exponential shape seemed natural given what we know from theory about attenuation related to individual physical processes. However, the Authors themselves convincingly show in Section 3.2 and Fig. 4 (and 3 as well) that due to many different factors the apparent attenuation observed along "random" satellite tracks, and attenuation that would be observed along wave trains undergoing a certain physical process, are two very different things. In short, in my opinion the analysis can be made more convincing and more valuable for future applications if the Authors rethink their approach to the data and modify the manuscript accordingly.

The reviewer highlights an important question in this field that is yet to be confirmed, i.e., does wave energy truly decay exponentially with distance into the MIZ?
Despite its high spatial resolution, it is not straightforward to answer this question with the given dataset despite its high spatial resolution. The attenuation rate α is strongly dependent on the sea ice conditions. This implies that when sea ice conditions along the transect change, α changes along

the transect as well. If one were to measure the wave energy along a transect it may therefore not necessarily show any sign of exponential decay even if wave energy would truly decay locally as $\exp(-\alpha x)$.

Considering that the wave energy $E(f, x)$ in our study was estimated based on a section length of about 8 km, and that multiple independent estimate of $E(f, x,)$ along a transect are required to confirm whether wave energy really decays exponentially with x, sea ice conditions need to remain constant over multiples of 8 km before such an assumption can be reliably tested.

While little is known about the sea ice conditions along each transect, it is unlikely that sea ice conditions are constant at scales of say 30-50 km, and thus, in our opinion, it is most logical to assume piecewise local exponential decay of wave energy in our calculations.

2. I have doubts regarding the widths of the MIZ used in the analysis. The Authors say they used *x*MIZ estimates from Brouwer et al (2022), which are "based on the depth of wave penetration into the MIZ" (line 85), but Fig. 2 and, especially, 3 suggest that these estimates might be inadequate for the present purpose. In Fig. 3b, a reasonable estimate of xMIZ seems to be between 0.6 and 0.7 of the actually used one – which makes a huge difference. At first, I thought that maybe this is just an unfortunately selected example, but the drop in the amount of valid data points in the inner MIZ (red line in Fig.5a) suggests that *x*MIZ used in the analysis is systematically overestimated. Wouldn't it be more reasonable and consistent to compute *x*MIZ(*f*) as the largest *x* for which *E* can be reliably estimated (i.e., for non-grey parts of Fig. 2b and 3b), and only then take a median of those values? The proper estimation of *x*MIZ is very important, because the whole analysis is performed in terms of *x*/*x*MIZ. In its present form, considering what is presented in Figs. 2, 3 and 5, it is not very convincing.

The definition of the MIZ width is indeed a topic of ongoing research. The frequency dependence of the wave attenuation rate means that short waves dampen faster than long waves, and a definition of the MIZ on the wave energy decay within different frequency bands is therefore problematic. In this study, we look closely at the wave energy contained in narrow frequency ranges rather than the total wave energy. The cutoff threshold of the wave energy presented in our Figure 3 and 4 (gray lines) should not be interpreted as an estimate of the MIZ width, as it strongly depends on the frequency bandwidth used to estimate the wave energy. In Brouwer et al. the frequency dependence is somewhat eliminated by looking at the total wave energy instead: "*The inner boundary of wave penetration is defined as the location where significant wave height attenuation equals the estimated error in significant wave height.*", see Brouwer et al., 2022 (page 2327).

The choice of the definition of Brouwer et al., 2022 is obviously debatable considering the lack of scientific consensus surrounding a definition. A definition based on the threshold of what a method can reliably measure seems, however, unwanted because the MIZ width would change based on the measurement method used (an instrument with higher accuracy would measure a larger MIZ width). However, the definition of Brouwer et al., 2022, could provide a more robust interpretation/derivation of the MIZ width, as this definition seems to be very close to where the wave dominated surface elevation transitions into an ice-structure dominated surface elevation (see Brouwer et al., 2022). Such a definition relies less on the accuracy of the measurement method involved.

3. One of the main results of this analysis is the average (over all profiles) attenuation in function of x/*x*MIZ: it is found that, if exponential attenuation is assumed, it increases linearly with x/*x*MIZ. This result is interesting, but it is hard to think how it could be used in practice (as the Authors suggest in the discussion). First, the relationship α(*x/x*MIZ) is useful only if *x*MIZ is known – which is possible only if we already know the waves! Notably, replacing the wave-based definition of the MIZ with another one (e.g., ice concentration based one) would make the whole approach inconsistent, so it doesn't provide a solution to this problem (there are studies that show that MIZ width estimates based on different criteria can be very different). Second,

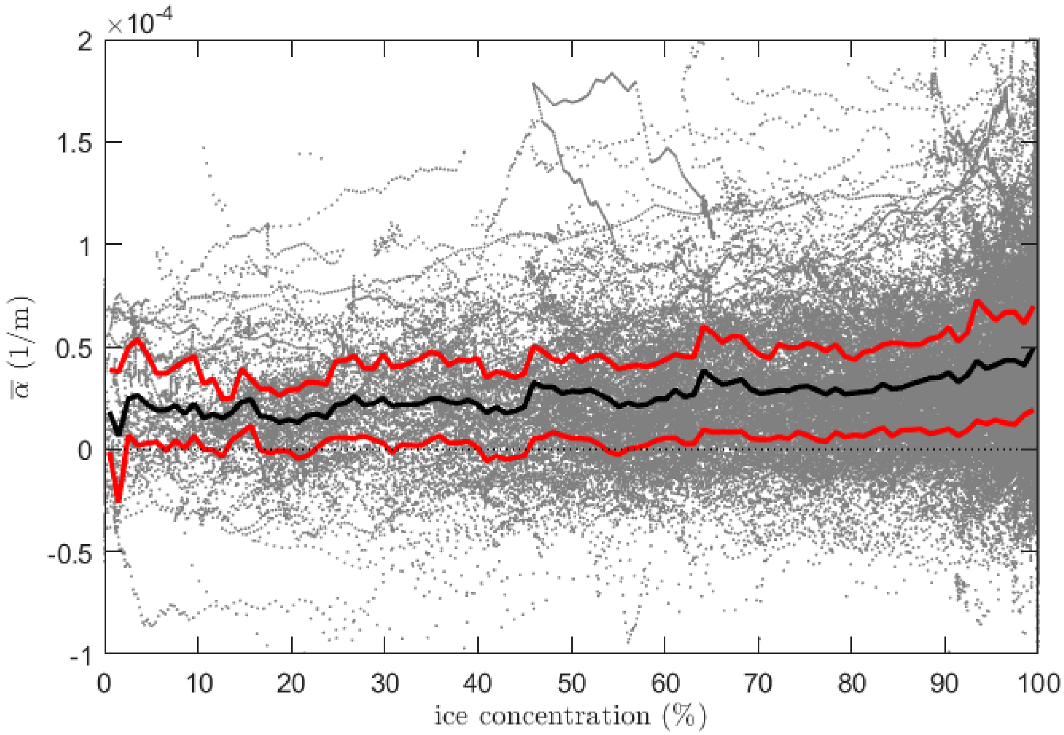
this approach seems reasonably simple only in 1D. In 2D, given that the outer and inner boundaries of the MIZ are irregular curves evolving in time, using the scaled coordinate perpendicular to those boundaries would require solving Laplace's equation (as in Strong et al., JPO, 2017) at each model time step. An approach that is anything but simple when one thinks about the details. I think therefore that the statement "it may provide alternative approaches to model wave attenuation in global models if the MIZ width is a known variable" is very misleading.

We agree with the reviewer that this is more challenging than we anticipated. We will therefore add a note to highlight this problem.

4. I have a question regarding the data in Fig. 5a,b,c. In panels b and c, there are hardly any values of α larger than, say, 5 10-5 m-1. In panel a, values above 5 10-5 m-1 are common in the inner MIZ (*x*>0.7*x*MIZ). Are there no ice thickness and concentration data from those regions, so that they don't appear in b and c? Or what is the reason for the different ranges of α in different plots? The increase of attenuation with ice concentration would be much steeper if all data were included, wouldn't it?

We can confirm that the same data is included in all these figures 5a and 5b. This is not the case for figure 5c as the maximum ice thickness based on SMOS is 50 cm.

The wave attenuation data appears less sorted with sea ice concentration than with the distance into the MIZ because they are more uniformly distributed. For example, see a replotted Fig. 5b below. We note that the gray shaded area in Fig. 5 corresponds to the 25th and 75th percentiles, meaning that 50% of the datapoints are outside this area. In general, one would expect a much stronger correlation with sea ice concentration. This could therefore mean that either this correlation is not as strong in practice, or the data resolution of the ASMR-2 observations is insufficient. We note that based on the study of Montiel et al., 2022, it seems that there is no straightforward scaling of α with sea ice concentration.



5. In spectral wave models, *cgdE/dx* ~ *AS*ice + *S*ee (*cg* - group velocity, *A* – ice concentration; 'ee' – everything else). If we forget 'everything else' for a moment, a measure of *S*ice can be obtained by dividing attenuation with ice concentration. I wonder how those profiles would look like. How

much of the observed increase of *dE/dx* with distance from the ice edge can be attributed to the increase in *A*? Is there a zone of high *S*ice close to the ice edge, as suggested by some studies? (A note related to point 1 above: even if we expect that *S*ice represents a single process leading to exponential attenuation, *dE/dx* should be "corrected" for ice concentration before it can be treated as an estimate of *S*ice.)

This is an interesting suggestion of the reviewer and based on this comment we have plotted α/C against x/x_{MIZ} for $T = 12$ s, see below. We may still observe an increase of α/C, which suggest that there are also other variables are involved in the damping of wave energy with distance into the MIZ, most likely, sea ice thickness and floe size distribution. We note that this is consistent with our reasoning in the manuscript of the increase in α with distance into the MIZ.
We note that interpretation of α/C near the ice edge is unfortunately not possible due to the limited number of observational points present here. This is because estimates of both $E(f, x)$ and α are taken over a finite distance along the transect, and thus no observations of either can be obtained exactly at the ice edge.