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 $\bar{\alpha}(x)$ 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 $\bar{\alpha}$ 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 $\exp(-bx^2)$, 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.

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 x_{MIZ} 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.
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 $\alpha(x/x_{MIZ})$ 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.
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>0.7x_{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?
5. In spectral wave models, $c_g dE/dx \sim AS_{ice} + S_{ee}$ (c_g - 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}.)