This is the review of the revised version of the manuscript „Finely-resolved along-track wave attenuation…" by Joey Voermans and coauthors.

I'll start with a technical issue:
Unfortunately, the version of the manuscript with tracked changes has only the major additions marked. Small corrections and fragments that have been removed are unmarked, which makes the assessment of changes that have been made to the text quite difficult. An advice to the Authors for the future: please use the "trackchanges" or a similar LaTeX package, so that all changes made to the text are clearly visible.

In general, I find that the changes the Authors have made to the manuscript have significantly improved its quality. Most comments and suggestions of the reviewers have been convincingly addressed, and missing details regarding the data analysis added.
However, a few issues remain that, in my opinion, should be solved before the manuscript is suitable for publication in TC. They are related to my previous major comment #1 (notably, the most important one):
The Authors seem to misunderstand the core of that comment – or they just pretend they do in order to avoid the necessity to modify/expand their analysis…
Of course, the sea ice properties do vary at the spatial scales < 8 km (the resolution of the analyzed data). Obviously, they also vary at larger spatial scales, resolved by the analyzed dataset – and, as the results presented in the manuscript clearly show, the net result of that variability are non-exponential profiles of wave energy.
Do the Authors seriously claim that the apparent attenuation – that is, the observed attenuation resulting from the sum of all factors influencing it – miraculously changes character from exponential to non-exponential one when it is observed at spatial scales below and above 8 km?
This is exactly what the Authors assume.
Very importantly: our discussion here is not about attenuation related to any given physical process. In both cases, at the scale of 8 km and at the larger scales resolved by the data, it is the apparent attenuation that is analyzed.
Why should it change character? Why at 8 km? If the resolution of the data equaled, say, 20 or 50 km, would the Authors compute the attenuation in the same way and use the same arguments to justify it?
It's all simply inconsistent and therefore very unconvincing.
In short, I don't find the answer of the Authors to my comment #1 satisfactory.