Answer to Reviewer 2, Review round 1

“Buoy measurements of strong waves in ice amplitude modulation: a signature of complex physics governing waves in ice attenuation”

The Cryosphere, egusphere-2024-2619

The manuscript describes a detailed analysis of an observed wave event in the Arctic MIZ via wave buoy measurements that exhibits a large amplitude modulation over a period of 3 days in the Spring of 2021. The 12-hour modulation period strongly points towards an effect of currents/tides. A wide range of datasets are then used to test this hypothesis. The main finding is that currents and tides alone cannot fully explain the magnitude of the observed modulation and that processes related wave-ice interactions are likely the main cause of this effect. In particular, a periodic switch between ice drift convergent and divergent regimes, which leads to stronger vs weaker ice-induced wave attenuation, respectively, is most likely what explains the modulation. A discussion of the physical mechanisms that can cause wave attenuation in ice-covered seas leads the authors to conclude that an on-off switch of floe-floe interactions, e.g. inelastic collisions and hydrodynamic pumping, could explain these alternating wave attenuation regimes.

Overall, the manuscript is reasonably well written and most conclusions are well supported by evidence. Discussions of limitations and uncertainty are also well incorporated. My main criticism relates to the style of writing, which is (i) quite informal in places and (ii) not efficient, with a lot of repetitions and general lack of conciseness. This makes the paper unnecessarily long in my opinion, which in turn deteriorates the reading experience. Therefore, I strongly suggest that the many authors of this paper have a critical look at the writing and attempt to be more concise in presenting their arguments. This is the main reason why I recommend major revisions. More details and suggestions are provided in my comments below.

We want to thank the reviewer for their detailed comments and helpful suggestions about our work and manuscript. We observe that the reviewer is positive about the observations presented, and that most scientific points raised are relatively minor clarifications that we can easily implement.

Regarding the format and manuscript organization aspects raised by the reviewer, we agree that the reorganization of the manuscript suggested is a good suggestion, and, while this will be quite a bit of work on our end to implement, we are willing to do so. As this is purely a reorganization task, this should in theory not present any fundamental challenge.

We agree that the reviewer suggests good changes that will make the manuscript easier to read and, therefore, we are willing to:

- move some of the more technical / “nitty gritty details” to appendixes
- re-order some of the parts as suggested by the other reviewers, taking some of the points that are for now discussed later in the manuscript earlier on
- cut on redundant parts and / or move some in-depth discussions that can feel redundant to appendixes, as suggested by this reviewer

Naturally, this will be a significant amount of work that will take a bit of time, especially as many contributors have participated in this manuscript.

In the following, we follow The Cryosphere’s revision process that, at this stage, only answers to the reviewer are provided (and we do not provide an updated manuscript yet).

Main comments:

- p2: I find the list of references given for the different sources of wave attenuation by sea ice and sea ice breakup to be somewhat biased and missing key papers, especially from key contributors like Squire, Meylan, Bennetts, Montiel, etc. Some suggestions: Mosig et al (2015) for viscoelasticity; Kohout and Meylan (2008), Montiel et al (2016), Pitt and Bennetts (2024) for scattering; Montiel and Squire (2017), Mokus and Montiel (2022) for breakup. In addition, I fail to see the distinction between diffraction and scattering in this context. The paper by Zhao and Shen develops a diffusion approximation from a scattering model in a specific regime and is not really representative of the research on wave scattering in the MIZ.

Thank you for pointing to this. We will take an iteration on the introduction and include the references you suggest, as well as the key papers linked to these. We are fine toning down the distinction between diffraction and scattering.

- I think section 3 is too long and redundant. I understand the authors want to cover their bases, but I think the analysis done in section 3.2 is sufficient to demonstrate that wave-current interactions alone is not enough to explain the observed modulation. Sections 3.1 and 3.3 add very little to the paper in my opinion. My advice would be to focus on the results of section 3.2 and briefly mention that other lines of evidence though ray tracing analysis and altimeter data in open water support the conclusions. Maybe 3.1 and 3.3 could be included as a supplement or appendix if the authors think they are important. In its current form, I don't see the added value of having them in the main text.

We wanted to make sure, as the reviewer points out, to “cover our bases” and analyze this case from several different perspectives to make it as sure as possible that our observations cannot be explained by a “non ice related” mechanism. In particular, our discussions with wave experts, several of whom are part of this paper, made it clear that we had to carefully consider bathymetry and current effects, and whether these could explain the observations (see e.g. discussions about these aspects in https://doi.org/10.5194/egusphere-2024-2104 , https://doi.org/10.1017/jog.2022.99 , https://doi.org/10.3390/jmse12112036). To make absolutely sure that, to the best of our knowledge, this cannot be the case, we considered several approaches, resulting as pointed out by the reviewers to the three relatively heavy sections 3.1, 3.2, 3.3.

However, it seems that both this and the other reviewer are actually convinced enough by the discussion in section 3.2 alone, and both reviewers say that we are maybe overly cautious (and heavy to read) by effectively “triple checking” this result in sections 3.1 and 3.2. Therefore, we are willing to move most of sections 3.1 and 3.3 to Appendixes, and focus this section on a shortened version of section 3.2, moving our extra evidence into Appendixes. This will be an easy change, though it does require a bit of work to rework the flow of the manuscript.

- In section 4, I think the discussion of all physical processes that could explain the observed modulation is not that convincing. Sea ice convergence/divergence will change ice concentration locally, but many processes are likely to damp waves more in tightly pack ice compared to loose ice, including scattering (due to array effects), turbulence and yes also floe-floe interactions. I think this section does not need to be that long, as what it mostly says is that waves are attenuated more in tighter ice packs.

We are willing to slightly cut down on this section, tone down some aspects of the discussion, make it even clearer that we do not have specific evidence for one mechanism versus another, and mention scattering due to array effects (though we are not sure of how well established this is). We agree that we can highlight that the general conclusion is that “we show from field data that damping depends on the level of packing of the ice”, and reduce some of the discussions about collisions. However, we still believe that discussing what mechanisms are likely vs. unlikely to produce such an effect is useful, and we want to keep at least part of the discussion about the possible importance of collisions, though we can mitigate it by suggesting that other mechanisms, such as array effects in the context of scattering, could also play a role.

- I think the discussion of the paper is missing an analysis of what is causing convergence/divergence regime shift in the ice drift. I imagine currents and tides, but I don't think the point has been made sufficiently clear. This means that currents and tides are responsible indirectly, i.e. through their effect on the ice, on the observed modulation. If that's indeed the case, why is this not a more common feature observed in other datasets? Have the authors looked at other students showing SWH time series to see if a similar modulation is seen?

Indeed, we believe that currents and tides are the mechanism responsible for the convergence / divergence observed. Experiments with and without tides in the metroms model show a strong influence on sea ice divergence by the tides. Without tides, only undulations of much smaller magnitude, and varying frequency and phase, are present. Clearly, the convergence / divergence is present in both the observations and the models (see Fig. 3). While the model does not offer a causality explanation to the divergence and convergence it produces, we see two possible sources for the convergence and divergence especially in model data: i) tides and currents, or ii) the effect of the wind, either iia) due to shifting wind conditions that apply a stress on the ice and open or close the outer MIZ, or iib) due to the wind triggering inertial oscillations in the water which, when encountering gradients in the sea ice concentration. However, looking at the 2m winds from ERA5 in the area (see Fig. 1 here), seems to discard this hypothesis. Therefore, we believe that it is most likely that the tide and currents are responsible.

We are not sure why this effect has not been reported in the literature before. One possible explanation is that while it may happen regularly, this effect is seldom so dominant and so clearly visible as it is in our specific case. Given the uncertainties in waves in ice attenuation models, buoy motion, buoy signal noise, etc, it is well possible that less pronounced oscillations in this kind would have been easily overlooked. Similarly, the present event was very easy to spot since it is pseudo periodic over a few days, but a monotonic increase or decrease would be harder to flag and associate to this mechanism. If this hypothesis is correct, then the value of our case is that this effect is so pronounced that it is easy to see and hard to overlook: a “hidden variation” of 20 or 30% of the SWH could have been hidden in other sources of noise and uncertainties, and overlooked in other data. Hopefully, researchers will be on the lookout for such signals in the future.

We actually believe, now that we are aware of this phenomenon, that we see it quite regularly in our data (though maybe not always as pronounced). We can take a few examples from other data that we have released previously, see for example Fig. 2 and Fig. 3. We have been aware of this for a few months now, and we believe that this is quite exciting and deserves further analysis. However, this will require quite a lot of work and time to investigate in details, and we unfortunately do not have this time resource available at the moment. We can mention this and add a few points of discussion in the manuscript, but doing a systematic study across many dataset, while very interesting and a logical next step, goes beyond the scope of our manuscript and would be a large endeavour to be done systematically and robustly.

As a side / anecdotical note, the main author of this manuscript was initially very worried seeing this pattern, as he assumed that this was coming from instrument malfunction - though this worry was quickly dissipated by observing that a clear pattern was visible across instruments of different types, which means this is clearly a real signal. But the only reason why we ended up looking at this event was that it was so obviously visible, even in log scale, in Fig. 2 of the manuscript (which is routinely produced by automated scripts processing OMB data) - otherwise, we would have easily overlooked it. While this is anecdotal, this can explain why this was not reported before, and this can be mentioned in the manuscript.

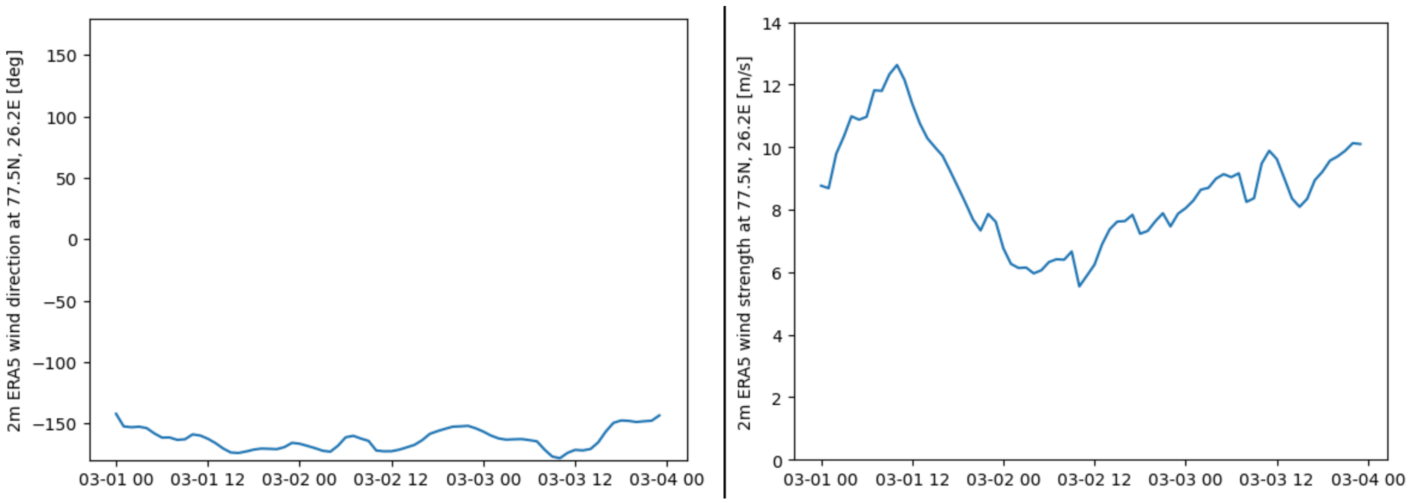


Fig. 1: ERA5 wind direction and strength at the general location of the BOIs further in the MIZ. We deliberately use a full scale to illustrate the (relative absence of) large scale changes in the wind strength and direction with a 12h period. As visible there, the wind does not have drastic 12h quasi-periodic changes over the time considered that would match the convergence and divergence observed.

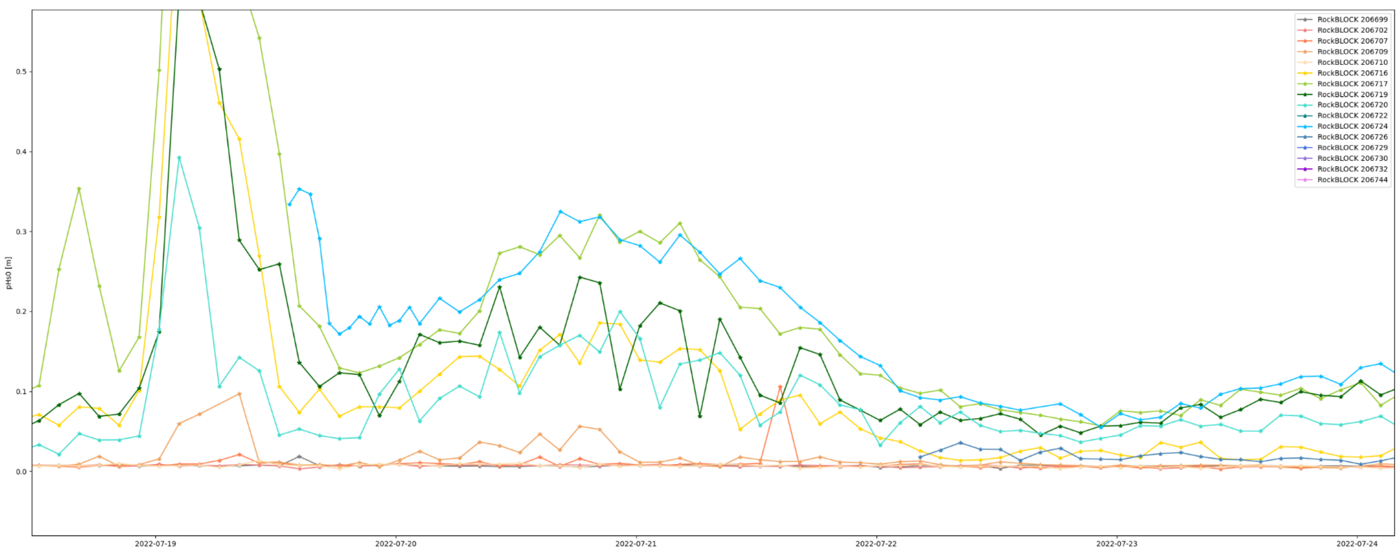
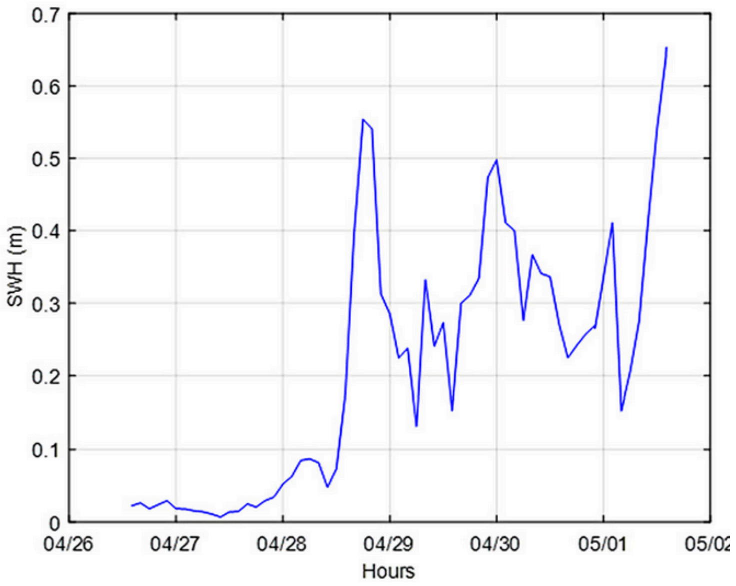


Fig 2: 2 more examples of strong periodic SWH modulation. These are only some of the most obvious such modulations involving at least 2 buoys we have found looking at other data after the present work was done, we believe that more events with less pronounced modulation are visible in several dataset. These data are selected from the files available at the following url: https://github.com/jerabaul29/2024_OpenMetBuoy_data_release_MarginalIceZone_SeaIce_OpenOcean/tree/main/Data/2022_AWI_UTOKYO , which come from a deployment in the sea ice North-West of Svalbard in 2022, so a different location and year. The data are openly available on github, and they are described in https://www.nature.com/articles/s41597-024-04281-1 . More "less obvious" events may be present in many other dataset - these are only examples of patterns that are so strong and clear that they are easily caught by the naked eye.



(c)

Fig. 3: SWH plot taken from https://doi.org/10.1016/j.coldregions.2021.103463 , Fig. 16 (c). We believe that this may be another example of such "modulated SWH event happening together with sea ice opening and closing". This is the data from a buoy that was deployed in April 2017 in a field of broken ice floes, around 200km South of the location of the buoys used in our present manuscript. Similarly to our manuscript, strong modulation in the SWH is observed over a period of a few days. There is also strong sea ice opening and closing happening in the area, as revealed by "buoy triangle analysis", see the manuscript for more details. This modulation was not particularly noted back then.

Other comments:

- p3, last sentence: This sentence is hard to read and could be worded better.

We are ready to reformulate this sentence.

- Fig 1: It is not clear which of the buoys on the left panel are selected for the SWH data shown on the right panel. Do the colours of the tracks and curves match? If so, why is there no black dot on the orange track?

Yes, the colors do match. However, we had also included trajectories that were not used as no SWH data was provided by some of the prototype buoys, and the trajectories overlap and hide each others. We will remove these additional trajectories to make the plot easier to read in the next version of the paper.

- p7: "As visible there ..." -> that is not obvious to me. How can we make out the MIZ in this image and where are the BOIs at the time of the image?

The buoy positions are indicated by the markers at the time of the image. We believe that, while it is always challenging to interpret SAR images, the reader should be able to distinguish the general shape of the MIZ limit.

However, we agree that the figure is not so easy to read. Therefore, we are willing to:

- re-generate the figure so that only the BOIs are included, which makes the figure much easier to read (similar to the next point)
- draw the general MIZ limit on top of the figure, to indicate the MIZ limits.

The data are much easier to interact with on the portal, so the user curious for more details should use the link provided. Unfortunately, the portal is not designed for generating and exporting “publication quality figures”, so we just have to do the best we can with it. Using another solution would be too much time and effort compared to be implemented.

- Fig 4: It is not clear which tracks correspond to the BOIs. Also I count more than 9 tracks, even though it was mentioned earlier that 9 buoys were deployed.

Indeed, it is not very easy to see which buoys are the BOIs, and there are more trajectories because we also had trackers that only did GPS tracking, not wave measurements. In addition, we had actually deployed more trackers, but there are some buoys that are outside of the area of interest at the moment of the event and, therefore, are not BOIs. The clutter on this figure was due to using the “raw” portal image, and the fact that the portal cannot be tuned to enable or disable individual trajectories in a dataset.

We have worked towards improving this and will now use a tweaked custom version of the portal rendering, see the screenshot below (Fig 3). Unfortunately, the portal is not so flexible for doing the plotting, so this is the best we can do - the users curious of more details can follow the link provided and interactively look at the data if they want.

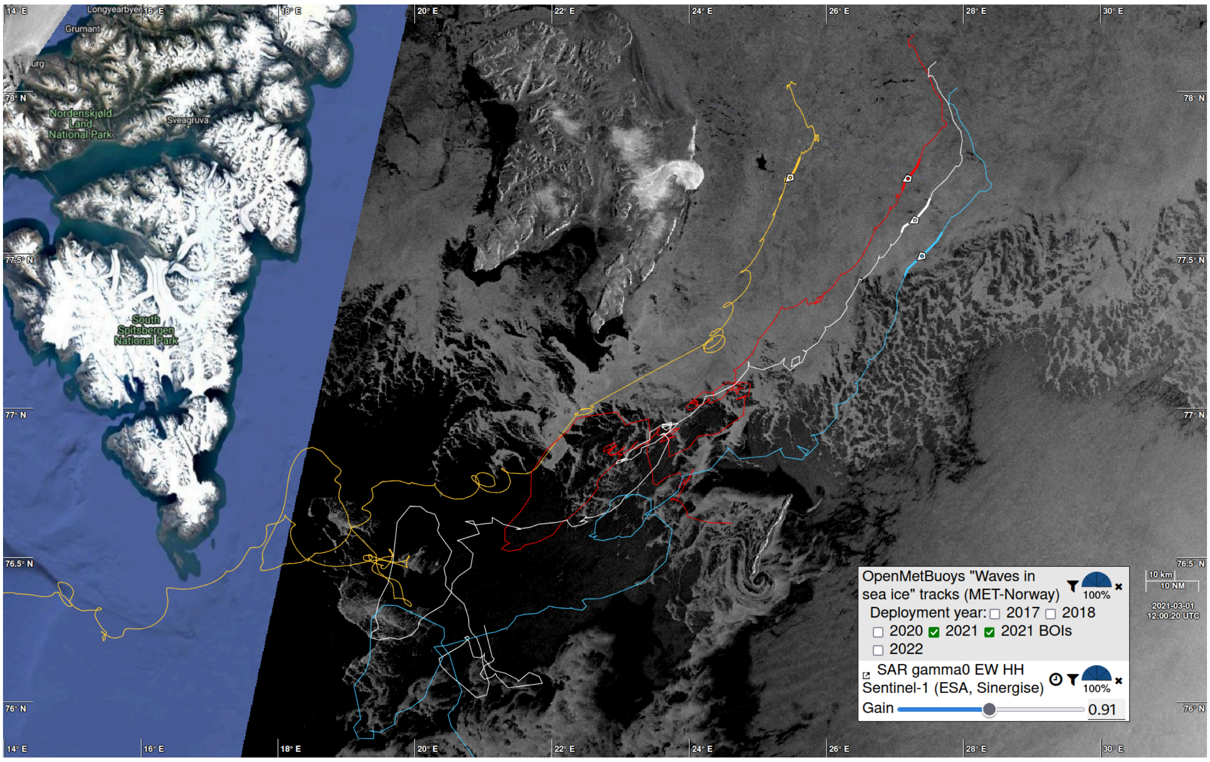


Fig 4: re-generated portal image, with only the trajectories of interest.

- p8: The discussion of Fig 5 should probably guide the reader towards the conclusion made. It is not clear at all to me that ice floes around the BOIs have similar size to the wavelength. Are dark patches open water? If so, in panel (d) it seems to be the other way around.

We agree that it is often not easy to interpret SAR images, and we believe that it is hard to make more out of these images than just getting an understanding of the general texture and length scales of the floes present in the images. Moreover, it is such, to the best of our understanding, that the distinction between water and ice is not as simple as dark vs. light color, due to variations in the kind of mode used when the images were acquired, polarization, incident angles, etc. What we want to point the reader to is that i) in subfigure (a), the wave crests are clearly visible and correspond well to the wave propagation direction predicted by the model; in subfigures (b) and (c) , it is visible that the SAR return is very inhomogeneous, with typical length scales of O(1km); this likely indicates complex fields of broken floes, and confirms that the area is neither close ice, nor open water, but something “in between” that can support the convergence and divergence obtained from the buoys trajectories and the model, confirming that this is realistically happening. Finally, the image (d) shows that this MIZ where the buoys are located is the outer area at the periphery of a field of larger broken floes. This will be made clearer in the next version of the manuscript.

- Fig 5: annotations on each panel are too small.

It is difficult to really scale up the annotations that are part of each panel, because this is exported directly from the data visualization portal, which provides only limited ability to tune annotations. We will do our best to re-generate these figures and maximize the annotations, but in the end, the reader curious of more details will have to follow the links provided to look on the portal directly, and we can only do “as good as we can” when exporting figures from the portal into a static manuscript.

- p11: The wave attenuation model used by Yu et al (2022) is not a common choice. Could this choice be better explained? The empirical model was obtained by fitting data obtained in the Southern Ocean Autumn, likely with a lot of pancake ice, so probably very different ice conditions compared to the Arctic Spring.

The Yu et al parameterization is used here as we have observed experimentally in several studies over the last year, both at the Met institute and at MeteoFrance, that this parameterization when used in operational wave models is the one that allows, without ad hoc tuning, to produce wave predictions in the MIZ that statistically best agree with observations. This was reported succinctly in a report (https://documentation.marine.copernicus.eu/QUID/CMEMS-ARC-QUID-002-014.pdf), and was also discussed recently at the EGU (https://meetingorganizer.copernicus.org/EGU24/EGU24-18430.html). This observation that the parameterization of Yu et. al. provides significantly better results than other models available was confirmed independently with both WAM and WW3 runs at MetNo and MeteoFrance, which were run by co-authors on this manuscript.

We have been in contact with J. Yu and E. Rogers over the last months and they were also positively surprised by this result, and at this point we have fully switched to using their parameterization at MetNo in operational models. We believe that the robustness of their parameterization shows empirically that simple models based on tuning to large datasets can be relatively robust independently of the exact location and season, which is an interesting fact. We agree that this may be a bit surprising, and we can make the reasons for this choice clear in an added discussion to the next version of the manuscript.

- Eqs (7), (8), (9): mathematical notations are quite poor in all these equations. Usually successive letters in italic denotes the product of the quantities denoted by the corresponding letters, so for instance SWH actually means S*W*H. Grouping letters together to denote a single quantities is usually done by using roman font type.

Thank you for pointing this out, will we fix this typography and style aspect in the next iteration of the manuscript.

- p27, penultimate sentence: This statement assumes that the ice-induced wave attenuation model used captures properly the dependence on thickness and concentration. I don't think this has been verified.

The waves in ice attenuation parameterization used (Yu et al) has a dependency on sea ice thickness. In addition, the wave model as a whole has a dependency on the sea ice concentration as the terms for the open water vs. fully ice covered contributions in the spectral model are weighted by the sea ice concentration. Therefore, it is, strictly speaking, correct to say that the model takes into account both sea ice thickness and concentration.

However, we agree with the reviewer that this has not been carefully verified yet in the peer reviewed literature (as a side note, we would argue that this is not better or worse than the situation with any other model or parameterization, as we believe that no model has really been well and robustly verified in general, as visible from the plethora of parameterizations developed and tuned over the years, and the difficulty to make these work robustly in operational models). Therefore, we are willing to tone down this statement, and i) remind the reader of how both effects are taken into account and write that, while ii) making it clear that this is a best attempt at taking these parameters into account, but the approach may still have imperfections.

- p28, second paragraph: I don't understand why refraction, reflection and diffusion are used instead of the more general term "scattering". Also, overwash should be mentioned as a dissipative process, noting that it doesn't fit into the categories listed. Further, scattering does not just depend on floe geometry. There are also array effects (multiple scattering), so the response (including attenuation) will change if floes are loosely or tightly packed, as could be expected in a divergent or convergent ice drift regime, respectively. Therefore, I'm not sure scattering should be dismissed so easily.

We are fine changing our text to use the term scattering, which may indeed be a better technical term in this context, as well as discussing scattering in more detail including the points raised by the reviewer. We are also fine to add a mention about overwash - though it is our feeling, after having been on the sea ice and deploying the instruments, that the freeboard was enough to make overwash relatively unlikely.

- p28, 3rd paragraph: Viscoelasticity is interesting as it has been used to explain attenuation in homogeneous ice cover as well as highly inhomogeneous ice covers. In the latter case, viscoelasticity is of course not the process that causes attenuation, but a convenient effective model for wave damping by sea ice. In the present case, where the ice field seems to be broken up into floes, i.e. non homogeneous, I agree viscoelasticity is likely to not be the dominant physics explaining wave attenuation. I don't think changes in Young's modulus or temperature is the main argument against viscoelasticity.

Thank you for pointing this out. We believe things have been a bit unclear in the literature on this aspect. We are happy that the reviewers agrees that “in the latter case, viscoelasticity is of course not the process that causes attenuation, but a convenient effective model”: this is also our view, but we believe that not all papers make it clear, and we are not sure either that everybody in the community would agree (though this is speculations on our end). In particular, it is in our experience quite common to be asked by reviewers to tune or consider tuning “existing models” (that often rely on one form or another of viscoleasticity) to fit observations, even in cases where viscoelasticity is clearly not a realistic mechanism.

We agree with the reviewer’s comment, and we will point this out better in the next version of the manuscript: i.e., i) there is no good reason to expect viscoelasticity to play an important role in the present case, and ii) even if it would play some role, there are no temperature fluctuations that could make this vary. We would like to keep the note about the temperature variations, as this also affects / partially rules out the possibility of strong melting / freezing happening back and forth over the period considered.

- p28/29: I think the discussion on collision based on reviews from a different paper is out of place. I'm sorry to hear the authors had a difficult reviewing experience in the past, but I don't think the present manuscript is the place to settle scores. Collision studies have been conducted, at least in the lab, so why not just refer to those, e.g. Bennetts and Williams (2015).

We thank the reviewers for pointing out that this could be read differently than what we intended. We believe that this comment refers to:

“ The question of the existence of collisions in the MIZ has been slightly polemic in the last few years (while no article explicitly mentions this as being polemic, as far as the authors are aware, this assessment is based on personal communications and discussions received by some of the authors, and reviews received during the publication process of Løken et al. (2022)). In particular, there are arguments that floes follow the waves in synchronization, so that there are no collisions actually happening between the floes even if all floes move“,

and some of the discussions following this.

Our aim here was not to “settle scores” but rather to explain to the readers that there have been some debate / disagreements in the community, probably to a larger extent than what is possible to grasp from reading the literature (not that disagreement is not bad per se in our opinion; it is our view that constructive disagreement, as long as it is not obstruction of evidences, is much welcome), and that this may have biased the literature a bit - this means there are strong opinions coming from some people who likely regularly act as reviewers, which may effectively filter out the manuscripts that readers go through and modify the relative balance between different phenomena discussed in large parts of the literature.

But we agree that this can be either removed or quite a bit toned down, and we will add the reference suggested and take one more iteration through the literature. Still, we note that the literature discussing these effects is (surprisingly?) scarce, despite the fact that collisions are a reasonable attenuation mechanism that has been measured both in the laboratory and in the field in the past.