Answer to Reviewer 1, 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

This manuscript presents unique observations of ocean wave attenuation in sea ice, in which the attenuation is modulated on a 12-hour cycle. The increased attenuation coincides with sea convergence driven by tidal and/or inertial currents. The observations are clearly documented, including contextual information on sea ice and oceanographic conditions. The manuscript carefully presents and evaluates different hypotheses for the underlying mechanisms, before concluding [cautiously] that the increased attenuation arises from floe-floe interactions during ice convergence.

I find the analysis herein to be rigorous, and I am fully convinced that the observed changes in the waves are related to changes in the sea ice. That said, I have recommendations for major reorganization of the work and reframing of the results. I think the attenuation estimates should be the very first part of the results (and thereby more central to the paper). I think the wave-current analysis and “extra” hypotheses should be evaluated after the sea ice mechanism is evaluated, possibly as appendices or discussion material. It is important to retain this material and show that currents cannot explain the observed modulation, but in the present form the ‘reader-fatigue’ from this material undermines the impact of the attenuation results.

We want to thank the reviewer for their kind words, and we note that the reviewer is generally positive about our data, results, and manuscript, though the reviewer and us may have different opinions about what is best suited as a methodology / goal for the manuscript, and we may disagree on which focus we want to give to our work.

We agree that the paper is currently quite long and goes into many details. We are fine moving significant parts of the technical discussions into appendixes, and reordering some of the scientific content of the paper. This will be a quite major re-organization of the paper and a significant amount of work, though this does not change the scientific content per se.

For reasons discussed later in this answer, the main point of this manuscript is to demonstrate that the modulation effect likely comes from the effect of the sea ice, and that this is likely the effect of sea ice convergence and divergence, i.e. ice becomes more or less closed. While the reviewer seems to assume given their comments that it is obvious that the modulation we observe comes from the effect of the ice, we believe that this is not as clear a priori as the reviewer seems to assume: complex currents and bathymetry effects may cause strong, time-dependent, non-local modulations to the wave height. Therefore, our focus is that the main point of this manuscript is to demonstrate that subtle changes in sea ice conditions are the most likely explanation for the modulation observed. We want to keep this focus as it is and not expand it overly, as we believe that it is necessary to carefully validate this affirmation. In particular, we are not willing to extend the manuscript by doing ad-hoc tuning and comparison of waves in ice attenuation models, which would need to be done in a very careful way and with a larger dataset split into some training and validation subsets to avoid HARKing and allow us to offer actual predictive power and scientific value. This can be the focus of future work, which ideally should extend their dataset basis to more cases than just the one we present. Said otherwise, we believe that this research field is already suffering from many studies over-fitting parameterizations with free parameters to individual small-scale field deployments, and we do not want to add to this confusion. Therefore, we want to establish that a physical parameter is important in determining wave in ice attenuation rate, but we believe that to actually develop a parameterization, a much wider body of input data should be considered, which is outside of our scope.

Therefore, we are willing to address most of the small comments made by the reviewer. However, we unfortunately disagree regarding i) the point about the tuning of existing attenuation models (though we are not really sure of what the reviewer had in mind, and if it was a request for adding more work, or just a side comment, see the discussion below), and ii) the suggestion to compute attenuation rates directly from computing ratio of spectra between pairs of buoys. Going into more details, the reasons for this are as follows:

Regarding i), we believe that a relatively “simple” ad hoc and a posteriori tuning of free parameters with time-dependent values (since, as we already show with simple models (WIC runs) that have generally similar characteristics to the complex models suggested, time-independent parameters in wave attenuation models are not able to reproduce the observations) in existing models to fit and reproduce our observations, would be a methodological mistake akin to HARKing. While we agree that we could reproduce our (or, really, any) observation by performing ad-hoc a-posteriori time-varying parameter tuning in existing models, we believe that this would create an “artificial” agreement and actually imply nothing about the true validity of the models and the physics they contain.

Regarding ii), the array of buoys is, by a wide margin, not aligned with the direction of wave propagation. As a consequence, the differences in attenuation are arising from a combination of factors, ranging from depth in the sea ice, to the sea ice area through which each wave ray has propagated. This is a major reason why we put so much emphasis on comparing our buoy observations to models, rather than “simply” computing ratios between buoys. This will be made clearer in the next iteration of the manuscript. We note that this is well aligned with some recent findings and discussions, see for example an enlightening recent study on this topic: https://doi.org/10.3389/fmars.2024.1413116 .

As a result, we want to keep the focus of the manuscript as it is now: showing, by eliminating alternative explanations, and by leveraging detailed model runs with different sets of physics enabled or not, that the modulation observed most likely comes from the effect of the sea ice, and is most likely an indirect consequence of sea ice convergence and divergence.

Given the buoys configuration, proving such a fact is in our opinion not trivial, and this is why this takes most of our efforts in this manuscript. Since the manuscript is already quite long and all our data are openly available, we believe that this focus makes sense in the present context. Naturally, further studies either by us or by other groups can further investigate these data, building on the understanding we generate here.

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).

Once the attenuation estimates are more central in the paper, the results can be reframed to acknowledge that small changes in attenuation rate make big differences in wave observations over long propagation distances. Thus, the observed factor of 10 modulation in significant wave height (SWH) from 0.03 to 0.33 m arises from a mere factor of 2 modulation in the attenuation rate. In the context of prior waves-in-ice studies, this a very modest change in the attenuation rate. For example, Rogers et al 2016 (DOI:10.1002/2016JC012251) find similar changes that occur simply from differences in the shape and maturity of pancake ice floes. Other studies find that a factor of 2 change in the attenuation rate can occur between the compact edge of the marginal ice zone and the more diffuse interior (Hosekova et al 2020, DOI: 10.1029/2020JC016746). With this in mind, I disagree with the interpretation that the convergence of the sea ice "switches on" a new mechanism related to floe-floe interactions (collisions, etc). Rather, I think that convergence of sea ice causes subtle changes in sea ice concentration and/or thickness (through volume conservation), and that causes an increase in the attenuation rate. The increases might be reasonably well-described by existing parameterizations (Meylan et al, 2018, DOI: 10.1002/2018JC013776; Rogers et al 2018, NRL/MR/7320--18-9786). Those existing parameterizations have tuning parameters that are not tested here, so we cannot say whether new formulations are required.

We agree that we can change the order of the manuscript and make this point about the "relatively moderate" (factor 2) change in the bulk attenuation coefficient early in the manuscript. We are fine adding the references that the reviewer mentions, and discussing that these point to a wide range of bulk damping parameters.

We are a bit unsure about what the reviewers means / wants regarding the tuning parameters that are not being tested. On our side, we do not want to perform ad hoc tuning of existing parameterizations and make this part of the manuscript. We also believe that the nature of the variability we observe here is quite different from the variability observed in the studies that the reviewer lists. There are several reasons for this, as presently highlighted in the manuscript:

- The sea ice conditions in some of the studies that the reviewer points to were very different from the ones we had: our buoys were deployed in close drift ice, and these conditions were definitely not conditions dominated by grease ice and pancakes as in several of the studies cited by the reviewer. Moreover, we observe periodic changes in the SWH and corresponding attenuation rate over 12 hours. As discussed in the manuscript, this cannot reasonably be expected to come from periodic changes in the ice conditions: the sea ice will not periodically change from grease to pancake or other state back and forth within a period of 12 hours.

- We do not have observations at a level of granularity that is fine enough to validate if the attenuation rates periodically doubles / halves over the "global" domain considered everywhere, or if the attenuation rate remains the same in large areas, and does much more than doubling / halving locally. This was our initial motivation for keeping the bulk attenuation rate plot for the discussion part of the manuscript, and not giving it too much focus, though this can be made clearer and will be now discussed in the manuscript explicitly. We will make this clear, by explaining that the damping rate we provide in the current Fig. 14 are bulk rates over (relatively) large distances, but that we do not know the details of short-scale spatial variations: this may correspond to either a general doubling / halving, or to much larger changes on smaller areas. This is hinted at in recent studies, e.g. https://doi.org/10.3389/fmars.2024.1413116 that illustrates how difficult and potentially tricky it can be to relate local and "large-distance-averaged" damping rates. A significantly larger number of buoys, deployed in a denser network, would be necessary to answer these questions with good confidence - we will make this clear in the next version of the manuscript too.
- Regarding the general direction of tuning / testing the tuning of free parameters. This may be a bit of a different discussion / side thread, but we find that it is potentially problematic, from a fundamental methodological and scientific viewpoint, to tune models in an ad hoc, a posteriori way to fit observations, which is why we do not perform such a task here. We agree that, since most waves in ice models have free parameters[1] that can be tuned, and observations are relatively few and scarce, we can tune any of the models presented in the literature to represent any field observations from a few buoys (especially so if we allow dynamic time-dependent tuning of the parameters as a function of some ice high level properties). However, we believe that this i) does not offer any insight into the physics, as any model independently of the physics it implements can be tuned in such a way, ii) holds no predictive power for future studies and operational models, as the tuning has to be done for every set of observations individually, iii) can easily turn out to be a subtle form of overfitting / "HARKing": effectively, by doing so we would hypothesize (tune in an ad hoc way) what the free parameters in the models should be after observations are taken, so that a good result is guaranteed to be obtained independently of the correctness of the input model and its physics. This, in our opinion, gives a false feeling to the reader about the performance of these models and the fact that these focus on the right physics. This is the core reason why we use models here not to reproduce our observations through ad-hoc tuning, but only to highlight that interesting physics are happening and cannot be explained by simple bathymetry or current effects included in such models, nor by standard non-tuned wave in ice attenuation models, so that the features observed deserve attention.
- Moreover, we are a bit skeptical that the physics present in these models can realistically be expected to lead to the periodic variations that are observed. This is already discussed at length in the manuscript (pages 28-29). In particular, we are not

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[1] usually at least 2 or more parameters are available to perform tuning of such models, to the best of our understanding up to 6 for some of the most complex and least-constrained models; this is more than enough to "draw an elephant and make his trunk wiggle": https://www.nature.com/articles/427297a , https://en.wikipedia.org/wiki/Von_Neumann%27s_elephant

convinced that volume conservation leads to thicker ice when the ice is more closed, which can reverse to thinner ice when the ice is more open. We believe that this may be true for a layer of grease ice, but not really for solid ice floes: ridging may happen, but this is not a “reversible” process: the sea ice does not “unridge” when it opens again. This is the main point of the discussion on pages 28-29, that considers a wide range of possible sea ice characteristics changes that are taken into account in the physics of these models, and that we do not believe can be expected to change periodically and be reversible. If we believe that we cannot expect the physics present in these models to be “reversible”, we do not believe that applying these models with parameter tuning to match our observations makes sense, following the point discussed above.

So, while we are willing to add a short description about the points above, in particular highlighting that ad-hoc tuning in time of the free parameters in wave in ice damping models could reproduce any attenuation rate observed, including the one we present, we do not believe that this could help advance the scientific discussion and understanding in the present case. This is why we do not perform such an analysis, and this will be made clearer in the next version of the manuscript.

Naturally, we remain open to the idea that a scientist may find a way to integrate more physics and causality into existing models, and come up with an a-priori theory and model that could quantitatively explain, without HARKing, our observations and other observations without the need for ad-hoc tuning. If so, we believe that this would be a significant advance, and we would be delighted to read about such work. Our data are fully open source, so that any member of the community can leverage our data to do such work. But this is beyond the scope of the present work, and would probably require considering a much larger dataset to be convincing: here, we simply focus on showing that complex features are observed, that cannot be a priori explained (without ad-hoc a-posteriori tuning) by existing models.

Specific comments:

The introduction could be a bit more careful not to overstate the ongoing buoy revolution. Certainly, more and more buoys are being developed and deployed (and this is great). Of the nine OMBs deployed for this study, only a few ended up in the analysis. We should humbly remember that works like Doble et 2006 (DOI: 10.3189/172756406781811303) deployed almost as many buoys 20 years ago, with similar capabilities.

We are fine to discuss older deployments and tone done on the low-cost buoy developments, and highlight to some higher degree that this is a continuation of a trend. However, the fact that we have reached 10x cost reductions and fully open source designs means a lot for the ability to scale up measurement volumes in our opinion. For example, the present study would not have been possible before this 10x cost reduction. In particular, as the reviewer points out, we had to deploy 17 buoys in total over the corresponding cruise to “be lucky” and have a particularly interesting signal on 4 buoys - we believe that this is an argument for, and not against, the need for more and larger deployments.

Moreover, as pointed above, we would ideally like to have even significantly more buoys (we are now actually routinely deploying 30-50 buoys per cruise, instead of 17 here). This is made possible by these cost cuts.

Fig 2 shows clear modulation of the wave spectra within the sea ice. The next logical step to compare with prior studies is to calculate the spectral attenuation rates (and then explore how this is modulated on the 12-hr cycle). In present form, that does not occur until page 31, and even then it is only a bulk attenuation rate. A spectral calculation might reveal more physics, including exploring the power laws described by Meylan et al 2018. If the goal of the paper is to show the cyclic convergence of sea ice changes wave attenuation, then please calculate the attenuation!

While we agree in principle with the comments by the reviewer, we do not believe that this is applicable to the present case. As visible in Figs. 6 and 8, the waves are coming from the South-West direction, propagating towards the North-East. The buoys we have in the area are, by and far, not aligned with this direction of propagation. This is one of the key reasons why we put so much effort and emphasis into running full wave models that include bathymetry and current effects both with and without ice, and we spend so much time comparing to the open water conditions. In particular, it would be deceiving to compute an "attenuation rate" between buoys 200913 and e.g. 19648, since the attenuation difference does not really come from the propagation of the waves "from 200913 to 19648", but rather from the differences along propagation of the waves through different areas and depth of sea ice through the MIZ on their way to each buoy individually. This will be made clearer in the text.

As a general note, we believe that this is a common, and often overlooked, weakness of waves-in-ice propagation studies: as buoys are generally either imperfectly or not at all aligned with the wave propagation direction, corrections and projections are usually applied to obtain attenuation rates. This can be complex and a source of errors depending on the case considered and the complexity of the local bathymetry and currents, which can result in complex shape for the wave rays (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). Similarly, the buoy-to-buoy comparison without a full spectral model being run can lead to many subtle artefacts. A particularly good illustration of this fact was recently presented in https://doi.org/10.3389/fmars.2024.1413116 , and this will be highlighted in the next version of the manuscript.

We believe that, as a consequence, attenuation studies should give more importance to running actual full-blown spectral wave models and comparing these to observations, rather than computing ratios of the signal at buoys that may not be aligned with the wave direction of propagation, which can be deceiving. This is in particular true in complex bathymetry and current situations, as the ones that we have at present: see fig. 9 and 11, that show that a significant (5-30% in the most extreme case, though this is significantly less than the 90% modulation observed by our buoys) modulation effect is present due to current effects. Therefore, directly comparing the spectra between buoys at different locations and deriving a fine-grained attenuation rate from this comparison would possibly include large sources of uncertainties and errors and be misleading.

These are the key reasons why we want to limit ourselves to computing and plotting the bulk attenuation rate analysis between open water and one single buoy as we do it now to get a feeling for orders of magnitudes, but we believe that going further in a point-to-point attenuation ratio analysis between two buoys should be avoided.

Similarly to the other comments, if the reviewer disagrees with our view, the data are openly available and more analysis can be performed independently of our work - however we would just not feel comfortable doing so ourselves, and we believe that this would be much more involved and uncertain than just computing an attenuation rate based on comparing the spectra for two of the buoys.

The work to show that currents and other non-ice mechanisms are insufficient to explain observations is very thorough, but it is almost a distraction. It's pretty clear from buoy 200913 that there is no modulation near the ice edge. So it's not the incident wave field that is changing. In particular, the historic/statical open-water wave analysis would be better placed in an appendix.

Our opinion is that the analysis of currents and other non-ice mechanisms is more necessary than what the reviewer believes / highlights in their comment. In particular, it could be possible in theory to have no modulation on the periphery of the domain, but to still have very strong and localized focusing effects due to bathymetry and / or currents closer to the coast, leading to modulation closer to the coast where the inner buoys are located. This is made even more complex by the fact that our buoys are not well aligned with the wave propagation direction. This is why we put so much emphasis on testing the hypothesis that the modulation comes mostly from the effect of the ice, and not from alternative effects. We can make this even clearer, and state explicitly that very strong modulation can be obtained locally due to currents and bathymetry (see, e.g., https://doi.org/10.1016/j.ocemod.2022.102071 , https://doi.org/10.1175/JPO-D-20-0290.1 , https://doi.org/10.1175/JPO-D-23-0051.1), so that we need to make sure that this is not what we are observing.

Despite these considerations, we are fine to reduce the discussion in the main body of the text, and put the more technical and lengthy parts in new appendixes.

Bottom of p 27: the statement that the CICE model used as input to the wave-current-ice ("WCI") model results reproduces the "time dynamics of the sea ice cover" is not supported in a quantitative way. Does CICE reproduce the convergence and divergence calculated from the buoys (Figure 3)? More broadly, the tone of this section is “well, the WCI model does not show the modulation, so a new mechanism must be needed”. My alternate interpretation is that the WCI model has ice damping parameters that could be tuned for convergence of sea ice (increasing concentration, thickness, or both). Figure 5 shows that sea ice concentration is very high at buoy 19648, but it is not 100%. Convergence could cause it to increase.

We believe that we already show that the CICE model reproduces the ice dynamics. As pointed by the reviewer, this is the point of Fig. 3: the rightmost part of the figure shows both the divergence computed from the buoys of interest following the “triangle element method” (solid red line), and the corresponding divergence computed from the CICE model (dotted red line). As visible there, the correlation is clear “with the naked eye”. Naturally, this is a complex case due to bathymetry and currents, so that minor time delays and peaks happen, but overall the general patterns are quite well recognized with the eye. To make this even clearer and more formal, we have computed the Pearson correlation coefficient between the BOI divergence and the model divergence, and the value obtained during the active phase of the modulation event is 0.71. This is convincing quantitative evidence that the CICE model is able to reproduce the convergence and divergence effect. This will be added to the manuscript.

We agree that it is likely not the sea ice convergence and divergence per se, but its effect on, among others, the sea ice concentration and “closedness” of the ice, and how this influences the physics governing wave attenuation, that may impact the attenuation rate observed. However, the sea ice concentration is not directly measured by the buoys, and, therefore, it is not possible to compare the sea ice concentration and its “closedness” from the CICE model to in-situ data. However, since these are directly constrained by the local level of convergence and divergence, which match well between observations and models, we have a proxy evidence that the CICE model is doing well on these aspects. This will be made clearer in the manuscript.

Regarding the impact of the sea ice concentration on the model: sea ice concentration is already taken into account through the linear weighting of the open water vs. ice terms in the spectral model, see the description of the model setup on page 11. While we agree that this is based on a relatively simple weighting method, this is to the best of our knowledge the standard in the field. We do not think that ad-hoc tuning of other free parameters inside existing wave damping parameterization, without a solid explanation to back it, would be a good practice with as little data as we have here: as discussed above, we could reproduce any behavior with tuning. Moreover, we believe, as pointed above, that developing new models is outside of our scope of the present work and would likely require considering significantly more and more diverse data, as discussed earlier in the answer.

Bottom of p29: the literature is pretty clear that ice floes do not follow the waves in "synchronization" but rather they slide down-slope on the face of the waves and have 'added mass' that introduces phase changes. Thus, they definitely collide. See Shen et al 1987 (DOI: 10.1029/JC092iC07p07085) and also Herman et al (JGR, 2018). Also the Smith and Thomson 2020 stereo work (already cited in the intro).

Thank you for pointing to these references, we agree that they are relevant, we will add a few sentences and a short paragraph discussing these.

Technical corrections:

The lack of line numbers in the PDF is frustrating

We agree with the reviewer, actually we had a discussion on this point with the editor at the submission of the manuscript, see the letter to the Editor. Unfortunately, given how the submission system works, there is nothing more we can do when using the “ArXiv submission method”, as far as we know.

The usage of a hyphenated ‘waves-in-ice’ or simply ‘waves in ice’ is not very consistent. I suggest the convention of hyphenation when the phrase is used as an adjective (e.g., “waves-in-ice physics are estimated”) and no hyphen when used as a noun (e.g., “waves in ice are measured…”)

We will go through the manuscript and make this consistent to the best of our abilities. The journal may have its own policies regarding hyphenation, and this will also be handled by the journal “redactors” at a later point when the proofs get generated, if this is considered necessary. We also believe that, while it is true that our command of the English language is not perfect and most of us are not native English speakers, this does not impact the ability to convey scientific ideas.

Top of p3: it is the *gradients* in wave radiation stress that transfers momentum to the ice and water. Without gradients, the radiation stress is simply an ongoing flux of momentum (but no transfer).

Thank you for your comment and in-depth reading of the manuscript, this is indeed not clearly explained at the moment, we will improve on the text.

Top p3: another recent fetch study is Brenner and Horvat (2024): https://doi.org/10.1029/2024JC021629

We are fine to add a couple of sentences discussing this work.

P 11: The inclusion of possible temperature modulation (and associated changes in sea ice rheology) is a good point, but evaluating with ERA5 seems like a poor match to the task. ERA5 would probably only show temperature changes if it also had modulation sea ice, which it does not. Surely the CICE model employed herein has temperatures?

The reason for looking at ERA5 data is that, given the 12 hours period observed in the buoy data for the modulation, only a drastic and fast change in the atmospheric temperature and associated heat fluxes with the sea ice would have any chance to exert a forcing that is strong enough to modify the ice conditions in a way that such a signal could be observed. ERA5 is a well-known and robust dataset that is suited for checking this, and, since this is not happening in ERA5 2m temperature data, there is no need to further consider complex sea ice temperature profile models to rule out this possibility: the forcing that could trigger such drastic changes is not present to start with.

Regarding the exact model runs we performed, these were run with CICE (metroms system) using Arome-Arctic as forcing. There too, there is no sign of large, periodic variations in 2-m temperature in the forcing of the ice model.