

The Cryosphere Discuss., author comment AC1
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Reply to RC1

Jill Brouwer et al.

Author comment on "Altimetric observation of wave attenuation through the Antarctic marginal ice zone using ICESat-2" by Jill Brouwer et al., The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2021-367-AC1>, 2022

Our responses in **bold**.

Reviewer 1: Dr Fabien Montiel

The manuscript describes a new method to estimate the attenuation of ocean waves in the ice-covered Southern Ocean using altimetric data from ICESat-2. The main application is to estimate the width of the marginal ice zone (MIZ), as the spatial extent over which wave attenuation is observed. Given the computationally heavy process involved, only four months of data is analysed. The authors strongly emphasize the advantage of their method compared to another widely-used definition in terms of sea ice concentration.

In my opinion, this is a very good paper that deserves to be published in Cryosphere. It is well written, the work is novel and very rigorously presented, and the results/discussion are interesting. Despite my lack of expertise in remote sensing technology and data analysis, I managed follow most of the methods section. Although my recommendation is for publication with minor revisions, I would like the authors to address the following comments.

We thank Dr Montiel for recognising the importance of the study, and for his thorough review of the manuscript.

Main comments

- My first and basically only concern relates to the need to consider "MIZ width" as a precise quantity in the first place. I understand there has been some previous work on trying to somehow measure precisely the MIZ and find its "boundary" with pack ice. I am skeptical about this as the MIZ was never well defined. The authors quote the "definition" of the MIZ from Wadhams in the introduction (lines 24-25), which is clearly qualitative at best. Any attempt to quantify it will therefore be up the authors to come up with a metric, be it concentration-based or wave attenuation-based. I don't see any reasons why we should expect these would match as they measure different things. In my opinion, the danger in this exercise is to characterise the sea ice cover in a binary

manner, i.e. MIZ or not MIZ. I feel like what we are really after is more nuanced, again especially referring to the non-quantitative definition of the MIZ. I fully agree with the authors that the concentration-based definition is lacking as it does not consider "open-ocean processes". At the same time, a wave attenuation-based definition also has some issues. For instance, if there are temporarily no waves, does the MIZ stop existing during that time? I want to be clear that I am not criticising the work of the authors in trying to quantify the spatial extent over which wave attenuation is observed. This is very interesting and the method they use clearly has a lot of potential for other applications. My concern is more trying to qualify this metric as the definition for the MIZ width. So when the authors refer to "the true MIZ extent" (line 58), the "physical definition of the MIZ" (lines 388-389) or "its true physical definition" (lines 479-480), I am arguing that this is an ill-defined concept and that there is no such thing as a true definition of the MIZ. If there was one, it surely would depend on ice properties as well as wave characteristics. My suggestion for the authors is therefore to rephrase some parts of the manuscript so as to incorporate the fact that MIZ and MIZ width are qualitative concepts as opposed to well defined quantities, unless of course they have a counter argument which I would be very interesting in reading.

We completely agree that there are a number of subtleties related to the MIZ definition, and that there are ways that we could improve how this is communicated in our manuscript. Indeed, what we have done is demonstrated that ICESat-2 can retrieve the limit of wave penetration at a snapshot in time. We agree that this may or may not truly represent some binary measure of the MIZ, for a number of reasons, including a) heavily attenuated wave passage may not alter the physical properties of the sea ice, even if it's still observable with ICESat-2; b) even with perfect observation of the penetration of waves, the wave penetration "yesterday" (or in the recent past) may have been higher, so that ice may still be "modified by interaction with the ocean" (i.e., MIZ). We do concede that what we have measured here may not be the true MIZ extent for these reasons, and that a binary view of MIZ may be unattainable (and may even differ for different purposes). We thank Dr Montiel for bringing up this important point and plan to both a) soften language around "retrieving the MIZ" throughout the manuscript, and b) address this issue more proactively in the introduction.

- The authors seem not to have considered the modelling work of Tim Williams, Danny Dumont and Luke Bennetts on MIZ width as measured by the extent of the ice cover over which wave-induced breakup can occur (see, e.g., Dumont et al, 2011, JGR; Williams et al, 2013a,b, Ocean Model.; Bennetts et al., 2014, Ann. Glaciol., Williams et al., 2017, Cryosphere). I feel this work needs to be discussed as they used another wave-based criterion to measure the extent of the MIZ and is therefore more in line with the proposed definition than the SIC-based one. Of course my previous comment still applies to this other definition of MIZ width.

Thank you for pointing out this deficiency in the literature covered - we fully recognise the potential for scientific advance by combining techniques such as ours with modeling wave attenuation studies (and indeed, have plans to contribute to work being planned by Luke Bennetts in this area), so we will cover these important references in a revised manuscript.

Other comments/typo

- line 21: r missing in "anthropogenic".

Will be fixed

- line 33: the authors might want to consider including Montiel et al. (2022), which has analysed the largest dataset to date of in situ wave buoy measurements in the SO, as another reference. The paper has just been accepted in JPO and can be accessed on arXiv at <https://arxiv.org/abs/2111.04819>.

Congratulations on the accepted paper - we look forward to reading it and plan to incorporate it into the literature review in the revised manuscript. Such a dataset will be of considerable importance to wider validation studies.

- line 84: This sentence is circular as it essentially says that estimating MIZ width improves knowledge of MIZ width!

Good point - sentence will be revised.

- Eq. (1) and line 150: I am a bit confused by this metric and why the authors use it to measure attenuation. Could the authors please clarify?

This metric is an attempt to measure the total amount of ice along any particular transect by "compacting" the lower concentration ice to 100% ice, then measuring the width of that transect of 100% ice. It is used here because wave attenuation is very low in regions of open water, so the important metric is the total length of 100% ice equivalent. We will clarify in the revised manuscript - thanks for pointing out that it wasn't clear.

- line 191+: I believe "change-point" is more appropriate than "breakpoint".

The "breakpoint" terminology comes from the "segmented" package in R. We now note, after checking this, that the term "change point" seems to occur interchangeably in their documentation. Given the reviewer's strong Mathematics background, we are very happy to defer to their expertise, and will change it as suggested in the revision.

- line 212: "on" missing.

Will be added

- line 325: I don't think n has been defined previously.

Very good point - will be rectified in the revision.

- line 363: remove "of".

Yes, will be removed.

- line 366-367: I'm not sure I understand the statement "this may indicate ... within ice". Since you defined the MIZ based on waves, is it not necessary that waves are present in the MIZ?

Reading this back, I agree that it's very ambiguous. We tried to argue that the seasonality of the wave climate aligned broadly with ours, so this may suggest that our retrievals are accurate. This sentence will be rectified in the revised manuscript.

- line 474: that is a bold statement. Not sure what it is based on as there have not been any comparisons with other approaches to measure wave attenuation done in this paper. Consider removing or better justifying this statement.

To be clear in this response, I believe you are referring to the sentence "This technique presents improvements over existing ways to determine wave attenuation in sea ice." Reading this back, I can see how it can be interpreted in unintended ways. We believe that this technique is currently the most precise way of currently remotely retrieving wave attenuation in sea ice. As it currently stands, the sentence (unintentionally) encapsulates in situ techniques as well. A revision will clarify our intent along these lines.