

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-367

Fabien Montiel (Referee)

Referee comment on "Altimetric observation of wave attenuation through the Antarctic marginal ice zone using ICESat-2" by Jill Brouwer et al., The Cryosphere Discuss.,
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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.

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.

- 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.

Other comments/typo

- line 21: r missing in "anthropogenic".
- 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>.
- line 84: This sentence is circular as it essentially says that estimating MIZ width improves knowledge of MIZ width!
- 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?
- line 191+: I believe "change-point" is more appropriate than "breakpoint".
- line 212: "on" missing.
- line 325: I don't think n has been defined previously.
- line 363: remove "of".
- 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?
- 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.