

This is a review of the manuscript entitled “A 12-Year Climate Record of Wintertime Wave-Affected Marginal Ice Zones in the Atlantic Arctic based on CryoSat-2”. The manuscript describes a method to retrieve the wave-affected Marginal Ice Zone (MIZ) using Cryosat-2. After introducing the importance of the MIZ, they describe their algorithm to retrieve the inner and outer limits of the MIZ. Then, they apply this algorithm over 2 case studies and discuss their definition of the MIZ against wave detected by Sentinel-1 to evaluate their method. They pursue this evaluation using this time a comparison of their method with ICESat-2 retrieved wave-affected MIZ for collocated tracks, with a special focus on 1 case. Having gained confidence in their algorithms and assessed sources of uncertainty, they extend their study to CS2 tracks in the Atlantic Arctic for the period 2010-2022. They describe the properties of the MIZ in 3 sub-regions and find no significant trend in the MIZ width in any of these regions. Finally, they discuss other sources of uncertainty.

The manuscript is generally well-written and clear. It synthesizes a large amount of work, with a strong emphasis on the validation of the algorithm using a multiple-sensor approach. The science is sound, well-referenced, and the results are well-discussed. Therefore, I recommend this manuscript for publication after minor revisions.

Minor general comments:

I would suggest restructuring section 6 to start with the discussion and end with the summary. I think that would make more sense and conclude the paper on a more “positive” note. I would also suggest concluding by adding a few sentences to give some context to the results. For instance: the dataset is now available to the public and the research community, what type of application do the authors suggest for it? Could we use it already to evaluate the MIZ extent in wave-ice coupled models? What is the next step with this dataset? For instance, is there any plan to retrieve more quantitative data from CS2 in the MIZ (floe size, wave height in ice...)? What is missing to do that? Is there any plan to extend the method to the Pacific Arctic, or Antarctica? Would it work? This conclusion does not need to answer all these questions or to provide an in-depth plan of future work, but I think giving some direction would really improve the impact of the paper.

I have another general comment that is more like a suggestion. The quantity of information lets me think the manuscript could be divided into two: proof of concept one (section 1→4) and a short result article extending section 5. That would certainly increase the impact of section 5 and benefits the authors. Now, the paper is coherent as it is and reads well despite being long, so the decision should be made by the authors.

Specific comments:

L1: “integral part of the ice cover” → I am not sure what this expression means. Important part of the ice cover?

L20: I am not sure “incurred” is the right verb here.

L22: Wave attenuation is a big topic and there is no real consensus on which processes (not all related to friction) dominate depending on wave and sea ice conditions. I would suggest “a diversity of processes”? On this note, I may be a bit biased, but I would suggest that a direct application of this dataset is to gain a better understanding of the processes dominating the wave attenuation by constraining the MIZ extent in wave-ice coupled model (see what Boutin et al. 2022 did with Horvat et al. 2020 dataset for instance).

L23: “more important roles by inducing positive feedback” → Asplin et al. 2012 only suggest it might be the case, but I don’t think it has been proven. I would add potentially (by potentially inducing...).

L24/25: The sentence is a bit confusing. Also, I’m a bit picky maybe but I feel “Ingvaldsen et al., 2021” is not the best reference to support the statement made here as it discusses physical and ecological changes, not really changes in human activities.

L32: “and the respective uncertainties” → the phrasing is confusing here. “and are highly uncertain in the MIZ”? (I am sure there must be a reference for that)

L66: “Furthermore, besides [...] that contain extra information of the ocean’s surface.” I am a bit confused by that sentence. I would recommend splitting it into shorter simpler sentences.

L71: “However, due to the relative coarse resolution of CS2 with respect to the typical wavelengths in MIZs” → Wavelengths is a bit ambiguous here → (surface gravity) wave wavelengths.

L74: “Wind waves affect the ice cover by wave/swell generation, the propagation into the ice edge, and the ensuing interaction with sea ice, including breaking the sea ice into smaller floes and the wave attenuation”. This sentence is a bit confusing and needs some rephrasing. (For instance, I understand the first part as “Wind waves affect the ice cover because they can generate swells”, which is not correct).

L82 “, waves and swells” → swells are still waves, so maybe “wind waves and swells”?

L83: I feel like these references are not the most appropriate to support the statement made here. The fact that waves get longer as they propagate has been known for a while (I’d suggest Robin, 1963, see below).

L87: on→in ; wave → waves

L91: The authors might want to repeat the reference to Figure 1 at the start of this paragraph, it really helps the reader to look at this figure while reading the description of these quantities.

L107: constitutes

L113: “is utilized” → I think “is used” works better here, and in a lot of places in the rest of the manuscript.

L135: I would recommend referring to a manuscript’s figure that shows such patterns (there should be one in Collard et al., 2022 for instance).

L159→165 I find this paragraph confusing, it could be worth re-ordering the information, maybe starting with the introduction of the physical concept (looking for individual leads as a proxy for pack ice), and then explaining how this is done in practice. I would also recommend adding a comment on this choice of defining pack ice with the presence of leads. Technically, the MIZ can be characterized by the presence of many small leads. While I understand the idea of the authors, I think it can be counter-intuitive to potential readers.

L200→203. I find the description of the method to retrieve “xi” hard to follow. I would suggest rewriting it or adding a little schematic.

L215: University

L218: “CS2 measured marked” → I don’t understand.

L229: shows → show

L230: “large... than” → “larger .. than”

L248: “is on the order of” → “is of the order of”

Figure 5: Which green points are associated with panels d,e,f?

L329: corrected → correctly?

L354: From the text, I don’t understand the reason why the swell penetration is “potentially limited”. My guess is that this is because this advected ice is thicker than locally formed one, but this is not clear in the text. Or do the authors mean that there is simply not a large band of ice (and so mechanically a narrow MIZ)? Please clarify.

L424: The gridded product resolution is much coarser than the mean width of the MIZ in the Atlantic Arctic. Is it not a problem? I would recommend justifying this choice and detailing what limits the choice of finer resolutions (e.g., the sampling of CS2?).

References:

Robin, G. de Q. (1963). Ocean waves and pack ice. *Polar Record*, 11(73), 389–393. <https://doi.org/10.1017/S003224740005350X>

Boutin, G., Williams, T., Horvat, C., & Brodeau, L. (2022). Modelling the Arctic wave-affected marginal ice zone: A comparison with ICESat-2 observations. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*, 380(2235), 20210262. <https://doi.org/10.1098/rsta.2021.0262>

Best regards,

Guillaume Boutin