

Based on the reviewers' comments, minor changes have been made to manuscript before uploading the final version, and those minor changes are found below. Authors changes are shown in blue, or as a tracked changes version of the submitted manuscript.

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Changes of the Arctic marginal ice zone during the satellite era

Rebecca J. Rolph, Daniel L. Feltham, and David Schroeder

Reviewer 1: Court Strong

I appreciate the authors' detailed attention to my comments. The revised manuscript reads very well and is an excellent contribution.

My only remaining suggestions relate to clarifying the terminology associated with the quantitative analysis leading into equation (1). If I understand the calculation here, the term "radius of the MIZ" (line 190) might be more clearly described as the radius of the parallel on which the MIZ is centered (measured perpendicular to the Earth's axis of rotation). The use of the term 'MIZ perimeter' (line 191) here could be confusing if the reader envisions an annulus and considers the perimeter as the sum of the inner and outer circumferences. Perhaps replace by 'MIZ outer perimeter' or, more precisely, the circumference of the parallel on which the MIZ is centered.

Thank you, these are good suggestions, we have added two statements around lines 190:

190 RMS values. The radius of the MIZ was approximated by $R_{MIZ} = R_{Earth} * \cos(\Theta_{MIZ})$ where Θ_{MIZ} is the monthly-averaged MIZ latitude and R_{Earth} is the radius of the earth. The radius of the MIZ as described here refers to the radius of the parallel on which the MIZ is centered (measured perpendicular to the Earth's axis of rotation). The MIZ outer perimeter (P_{MIZ}), or the circumference of the parallel on which the MIZ is centered, was then approximated from the average latitude of all MIZ grid cells while assuming a spherical earth and no land. This was done by substituting R_{MIZ} for the radius in the perimeter equation

Reviewer 2: Chris Horvat

(The main points are selected below from .pdf file he has uploaded, which is also available to the readers.)

Overall, I don't think you need these motivating arguments. I would remove them from the paper, leaving the focus on the presentation of MIZ, the model results, and the observation that MIZ extent is not increasing. That is a great paper to me - thanks to it, we now present MIZ extent in a recent paper (Horvat et al., 2020)!

We have changed the wording so that the statement now reads: '... tend to assume that the MIZ is expanding...' and have also italicized the word 'extent' in the following statement: 'The purpose of this paper is to show whether the *extent* of the MIZ, defined in this study according to the operational characterization, is actually changing.'

P2L33: The size-dependent melt rate problem was formulated by Steele (1992), not Tsamados et al. (2015).

This is a good point, and we have now added Steele (1992) before Tsamados et al. (2015).

P5L137: Horvat and Tziperman (2015) developed the FSTD model, not an ice thickness

distribution. Roach et al. (2018) then formulated this (with modifications) in CICE.

Good suggestions, we have changed the statement to the following:

170 floe size of 300 m, but in CICE-CPOM-2019 a joint floe-size thickness distribution (FSTD) is used which has been ~~developed~~
~~implemented and developed~~ by Roach et al. (2018) following ~~the ice thickness distribution of~~ Horvat and Tziperman, (2015).

P5L150: Both referenced papers do have wave spectra, but they employ different techniques. Which do you use? The exponential attenuation with floe number was first investigated by Dumont et al. (2011).

We have removed the reference to Bennetts et al. (2017) in this line, and have left the reference (Horvat and Tziperman, 2015). This is also found in the description of attenuation in Roach et al. (2018).

P11L319: Meylan and Bennetts (2018) does not deal with sea ice fracture but wave scattering.

We have replaced Meylan and Bennetts (2018) with Kohout et al. (2014) and Montiel and Squire (2017).

I would also point out that while this may be the first analysis of Arctic "MIZ extent", the first analysis of "MIZ extent" was probably by Stroeve et al. (2016).

We have added a statement in the introduction to reflect this, and it reads:

lacking, such as quantification of the MIZ extent relative to the total sea ice extent. Thus, we need to remain cautious and provide a specific and clear definition of the property of the MIZ when stating the Arctic MIZ is 'rapidly changing'. It also
50 follows that we need to be aware of the extent to which our observations are able to constrain any model of the MIZ. Note that Stroeve et al. (2016) have examined MIZ extent in the Southern Ocean.

Reviewer 3: Anonymous

General comments

The revised manuscript "Changes of the Arctic marginal ice zone during the satellite era" by R. Rolph, D. Feltham, and D. Schröder provides a comprehensive analysis of evolution in Arctic marginal ice zone (MIZ) extent relative to total sea ice extent (SIE) in a changing climate, and clearly addresses issues noted during the review process. Evaluation of MIZ width and latitude provide additional evidence for the absence of trends in MIZ extent, while investigation of MIZ area provides further characterization for changes in the MIZ based on the operational definition of the 15% - 80% sea ice concentration threshold. Thank you for a rigorous and quantitative analysis in response to questions raised. Please find below some additional comments and questions for consideration.

1. Thank you for the figure showing changes in MIZ area for the satellite record, and for including analysis and a discussion of MIZ area. Also of interest is the change in ice concentration distributions and heterogeneity within the MIZ; this could be evaluated by examining the time series for the ratio of MIZ extent to area. In particular, does the area analysis include an evaluation of the trends in the ratio, or a comparison of trends in MIZ

extent and MIZ area separately?

Although the authors note in lines 337 – 343 of the revised manuscript “Since there is no trend in sea ice area within the MIZ and no trend in the MIZ extent, there is no significant change of sea ice concentration within the MIZ based on observations (where sea ice concentration in the MIZ is given as the ratio of the area of sea ice in the MIZ and the extent of the MIZ). Similarly, there would not be any trend of sea ice area within the MIZ relative to the MIZ 340 extent. Since there is also no observed change in MIZ extent, it follows that the pan-Arctic averaged sea ice concentration is not declining in concert with its declining extent. This suggests that changes to the extent of the MIZ depend strongly on the sea ice thickness distribution.”, it might also be helpful to present the figure for the time series of the MIZ extent to area ratio.

We appreciate the reviewer’s suggestion to add the analysis about sea ice area within the MIZ to the paper, and think this figure fits into the frame of the paper well. As we have mentioned in the last response to Reviewer #3, the time series of MIZ extent to area ratio would also not show a trend, since both the MIZ area and MIZ extent each do not show a trend. This information is given in the manuscript, so we do not think it is an essential contribution to add the suggested figure of the ratios.

2. Although trends are considered for the 1979 – 2017 timeframe, what behaviour is observed for the MIZ property (extent, area, ratio of extent to area, latitude and width) anomalies relative to the 1981-2010 climatology? Anomalies, in addition to trends, might further illustrate changes in MIZ properties in recent years.

An anomaly analysis would be interesting to include in a future study, but not essential at this point to better convey the main messages already in the manuscript.

3. Line 31. Perhaps include the phrase “defined in this study according to the operational characterization” following MIZ.

Yes, we agree and have now changed the statement to: The purpose of this paper is to show whether the extent of MIZ, defined in this study according to the operational characterization, is actually changing.

4. Figure 2. Perhaps include names of months in titles, for consistency with other figures.

Yes, thank you, the names of the months have now been added to Figure 2.

Thanks again for your responses and the opportunity to review the manuscript.

We have also added acknowledgements to the manuscript so they now read:

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