

The work examines satellite observations of MIZ extent, finding total MIZ extent has changed little since 1979, although/because the MIZ has moved poleward and widened. They then examine their satellite measurements against an FSD-resolving model. This is skillfull and useful analysis and the scientific content is solid.

I *am* concerned with the editorial content. I think this can be addressed without too much effort and this study would then warrant publication. My words here likely will not require near as many in response.

I was assigned as an initial reviewer, but did not upload in time. Given the three other reviewers, I will just give my main comment as I do not want to add undue burden to the authors. I hope it is useful, and I am available if the authors have questions or would like additional context.

Main comment: As presented, this work is motivated by arguing others: **(1)** fail to grasp the difference between a linear measure (MIZ width) and an areal measure (MIZ extent), and **(2)** use sloppy terminology like "rapidly changing".

Regarding **(1)**, you list studies that "assume MIZ extent is increasing". I wrote one! Dismayed I made such a claim without evidence, I went and checked. Here are the relevant quotes:

... the Arctic marginal ice zone ... has been widening during the summer season (*Strong and Rigor, 2013*).

and

... dramatic intra-annual variability in sea-ice cover is found in the MIZ and in seasonal ice zones ... as summer sea-ice cover becomes thinner and more fractured, these regions will become larger ...

The MIZ is widening, and areas with seasonal ice have enlarged greatly regardless of whether the MIZ has (see *Kinnard et al. (2008)*). Having it on my desk, I do not see that *Boutin et al. (2020)* made this claim, either.

Regarding **(2)**, the MIZ *is* changing by many metrics explored here. In my reading these are often well-explained. This MS contains plenty of examples of its own loose phraseology: e.g., "significant" regional changes, correlations, trends, uncertainties, and declines, without accompanying measures of statistical significance. I don't think you need to change these, just remember that there are many hairs to split in life. I would argue that the use of "rapid" is a thin one.

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*)!

Smaller comments: I found that the referencing needed a careful re-examination. This was most obvious to me in papers that I am very familiar with. Therefore I would

suggest going through and making sure the referencing is accurate throughout. Examples:

- P2L33: The size-dependent melt rate problem was formulated by *Steele* (1992), not *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.
- 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).
- P11L319: *Meylan and Bennetts* (2018) does not deal with sea ice fracture but wave scattering.

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).

References

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