

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-307

Anonymous Referee #2

Referee comment on "An indicator of sea ice variability for the Antarctic marginal ice zone" by Marcello Vichi, The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2021-307-RC2>, 2022

This paper presents a new method to map the MIZ that was originally mapped by using the 15-85% of sea ice concentration (SIC) from passive microwave remote sensing data. Different algorithms in deriving the SIC would give very different MIZ (extent). But the new method that using the standard deviation of daily SIC anomalies (on monthly basis) gives consistent MIZ (extent) based on the SIC derived from different algorithms/datasets (Figure 5). Therefore, the paper concludes that this new method is a better method as compared with the 15-85% method, although without thorough evaluation to see if this is indeed the best MIZ (extent). I would think this is a new method and deserves further investigation and I encourage the author to do so. I would think the very first addition to confirm the potential effectiveness of the method is to apply this method to the Arctic sea ice. If the same conclusion is achieved, I would think it might be effective. Another way to evaluate the method is to compare the MIZ derived from high resolution imagery, especially for those areas and periods (for example, later spring/summer) with the highest disparity among the new method and existing methods.

Second, as indicated in the introduction, that SIC based MIZ identification is more reliable in the wintertime in southern oceans, I would agree your method seems achieve similar results (make sure this is correct), but for summer time, especially Nov, Dec, your results show too much high extent (Figure 5), similar or even larger, as compared with these from the 15-85% method that already said they are not accurate. Since overall, the Nov and Dec ice extents are smaller than the Sep/Oct, I would say the MIZ (extent) should be smaller than the Sep/Oct MIZ (extent). I know your statistic-based MIZ include those of the polynyas, not sure if these should be excluded? MIZ-like statistics can also found in the interior of the pack ice, should these zones also included as MIZ? In figure 6, your MIZ (yellow) for the December seems way to bigger and this makes me doubt your method for the later spring (Nov/Dec). maybe you need to use a larger threshold value for this period? Instead of 0.1, maybe 0.15 for this case? In Figure 7, the MIZ (extent) is larger than the SIE in five months, needing good explanation. To me the MIZ (extent) from the

NOAA ORD data seems more reasonable (all smaller than the SIE) (Figure 7). In line 227-228, you mentioned "climatological MIZ extent shown in Fig 5 is an underestimation of sea ice area", but then in line 232, you said that "MIZ extent presented in this work exceeds the total SIE". Some confusions here needing explanation.

third, in the figure 5, I believe this is the 30-40 year averages, right? can you show a at least a sub-set of the those in each year? say 2008, 2009, 2010, 2011...; so make sure those differences also seen in yearly curves, not just an effect of average of 30 years or 40 years...

fourth, your taking of 0.1 for the σ value seems random, why not 0.12, 0.15, 0.17, or 0.2...? should this number the same for the Arctic sea ice?