The Cryosphere Discussions

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Reply on RC2

Marcello Vichi

Author 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-AC2, 2022

Note to the reviewers

I would like first to apologise for the delay in providing an initial response to the comments. This is attributed to unfortunate timing, since the reviews came back at the very start of the academic year in the Southern Hemisphere. I had to wait for the end of the first term to find the proper time for focusing and adequately addressing the critical comments and helpful suggestions.
In addition, I have been working on an update of the manuscript using the newly released version 4 of NOAA/NSIDC Climate Data Record of Passive Microwave Sea Ice Concentration (https://doi.org/10.7265/efmz-2t65) . There has been a major change in this version, which is now released with the default application of temporal and spatial filters. Users would then not be able to reproduce the same results from the pre-print, and the version 3 dataset is not available any more.
The difference between the NSIDC CDR and OSI-450 that I highlighted in the pre-print is now much reduced, because both datasets are released with the application of various types of filters and interpolations. As indicated by reviewer 1 and further addressed in the specific answers, I am going to include in the revised version an analysis of the impact of the various filters on the diagnosis of SIC variability and the detection of MIZ characteristics.
The delay in this response was then additionally caused by my correspondence with the NSIDC data producers to make sure that it is possible to reproduce the version 3 results from version 4 (in addition to some small errors that I found in this new version that will likely be corrected in revision 4.1 of the dataset). This is still unresolved, but I am confident that it will be finalised by the time of submission of the revised version of this manuscript.

Answers to Reviewer 2

I thank the Reviewer for the comments and suggestions. Together with Reviewer 1, they have given clear indications on how to improve the analysis and strengthen the use of this methodology. The comments from the reviewer are indicated in italics and my answers are in normal text.

RC2*: 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.

Answer: The literature on the MIZ definition in the Arctic using the operational threshold is much more extended in the Antarctic. This work addresses the limitations of the method when applied to Antarctic sea ice, but I acknowledge that I have not provided enough supporting literature to indicate the need for a new definition. This has been pointed out by Reviewer 1, and will be addressed in the revised version of the manuscript. The extent of the MIZ is more limited in Arctic sea ice, although recent literature is indicating that there are shortcomings in the simulation of the MIZ fraction in climate models (Horvat, 2021). I would argue that a complete analysis of this indicator in the Arctic would be beyond the scope of this work, but I agree that some indications would be useful. In the revised version I will add a discussion on the application of this method to the Arctic, showing a figure of the \sigma distribution that will help to better constrain the choice of the threshold (as requested in the last comment below).
I will dedicate a specific section on the comparison with existing observations and case studies, as also requested by the other reviewer. I agree that descriptive MIZ features can be obtained from literature, although these datasets are unlikely to be comprehensive enough in a spatial and temporal sense. I have selected a few examples that include direct floe size measurements, satellite imageries and reported sea-ice visual conditions and I will use them to assess the value of the indicator and the difference with respect to the operational MIZ classification. The assessment will be done considering the climatological intent of determining regions of higher variability that is at the basis of this approach. A comparison with instantaneous observations (e.g. SAR images), long-term drifters (Womack et al., 2022), and short-term cruises will need to be put into context, and this will be done accordingly in the revision. Furthermore, this indicator could be used to detect persistent artefacts in SIC retrieval (e.g. Lam et al., 2018, for an example of an erroneous persistent polynia in a region where such a feature is absent). Regions of known permanent sea-ice cover (as for instance regions of M-Y ice) may show unrealistically high variability which can be identified by using the proposed anomaly method.

RC2*: 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?*

Answer: The reviewer is correct that Fig.5 may suggest some misleading conclusions. I acknowledge some ambiguity in the use of this method to compare with the traditional SIE. This method is originally designed to diagnose a more appropriate monthly climatology that would improve the current operational definition. Specifically it is meant to address whether a region of the seasonally covered ocean is characterised by relatively high temporal variations in SIC, a metric that cannot be obtained by the 15-80% threshold. I do not mean to say that the threshold-based estimates are not accurate, but that there are regions of the ice-covered ocean that present physical characteristics similar to the MIZ even when the concentration is above 80%. This includes areas within the pack ice and areas of polynyas. My analysis is therefore more oriented towards the estimation of variability due to heterogeneous ice conditions, independently of where they are located. These regions are mostly along the margin with the open ocean, but not necessarily, especially in autumn and spring. I will also consider a change of title in the

revised manuscript to indicate that the work first addresses the variability and how this can affect our definition of the MIZ.

***RC2**: 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.*

Answer: I apologise on the confusion. This apparent contradiction will be resolved in the revised manuscript, firstly by providing a more adequate explanation about the different ways of estimating the MIZ extent. SIE is a specific diagnostic, which is complementary to the estimation provided in this work. The most important pointi s how sensitive these diagnostics are to the underlying approximations. I have done further research on this, because another reviewer suggested possible biases of this indicator due to the degrees of post-processing of the raw brightness temperature data. The summer extent of the region characterised by high monthly variability is indeed modified by the inclusion of spatial and temporal interpolations. Using raw data, the December extent is further reduced (using both the SIC threshold and the \sigma indicators), which is important information. In the revised manuscript I will add a section that explores the impact of the various filters and I will better characterise the uncertainties using the total error of the algorithms. This analysis of the uncertainties was not included in the current manuscript and will be used to better constrain the seasonal cycle of the Antarctic MIZ.

***RC2**: 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...*

Answer: yes, this is the climatology. In the revised manuscript I will present specific years as well as different regions, but the caveat indicated in the manuscript at lines 220-230 is still valid. There is a fundamental computational difference between the climatological averaging of the monthly extents shown in Fig. 5b, in which a monthly mask is multiplied by the pixel area then integrated and averaged, and the mask based on the climatological monthly standard deviation of the daily anomalies . This is because the average of the standard deviations computed from sub-samples of a population is different from the standard deviation of the whole population.

***RC2**: 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?*

Answer: this number is obtained from the analysis of the median distribution shown in Fig. 2b. The results are not sensitive to 20% variations around this value and this will be indicated in the revised manuscript. However the threshold depends on the filtering level applied to the raw data, as for instance shown in the current manuscript when comparing Fig 2b (from the unfiltered NSIDC CDR) with supplementary figure S2b (from the filtered OSI-450). Now that the NSIDC CDR Version 4 is released in the filtered mode by default, this difference is not visible anymore unless the user removes the temporal and spatial interpolations. A dedicated analysis will be added in the revised manuscript. I will also add the distribution for the Arctic and will discuss the implications.