

The Cryosphere Discuss., referee comment RC1  
<https://doi.org/10.5194/tc-2021-307-RC1>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## **Comment on tc-2021-307**

Anonymous Referee #1

---

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-RC1>, 2022

---

This is my review of Vichi (2021).

Primarily, I want to apologize for the very long time in returning a review. I hope the author accepts my apology for this, while it is a challenging year to meet professional obligations, this simply was too much time to wait for what was a relatively short and easy-to-read paper.

In this paper, the author seeks an alternative definition of the marginal ice zone (MIZ). They use the distribution of inter and intra-monthly standard deviation of passive microwave SIC values, defining a MIZ metric as those periods where the standard deviation of SIC retrievals within a given month exceed 0.1 (unitless). Their key result is that when applied to the existing PM-SIC data, four main satellite products are in agreement as to a climatological seasonal cycle of overall MIZ extent. This is a new approach. It is clear that the MIZ requires an objective definition, and the author is making an effort to provide one.

Despite these intentions, I find methodological and conceptual flaws in the study that I do not believe permit its publication at this stage, and I recommend significant revisions be

undertaken before reconsidering this MS. Generally, what this article is lacking is supporting evidence. Many of the claims made by the author about this definition \*could indeed be true\*, and it may have immense promise as a definition of the MIZ. But there is no supporting evidence that this definition records something physically relevant to modelers, stakeholders, or observers. With this supporting information, the paper is quite a useful and interesting contribution. But absent it, it is hard to make much of this work.

Here I give a discussion on the merits of this work, focusing on this problem of physical and statistical foundation. I am not including specific small comments because I think any revision of this MS will require substantial changes that may render such comments obsolete. Below I include two overarching suggestions which I believe should be undertaken before this paper is published.

## Discussion

---

The study's motivation is that existing ways to define the MIZ do not capture the physical properties of the sea ice in the Southern Ocean: "I reassess the assumption that absolute values of sea ice concentration contain information on the sea-ice type in the Antarctic...". Throughout the MS, the author makes reference to waves, free drift sea ice, ice types, dynamical processes, "sea-ice textures", etc, which, to be sure, might not co-vary with sea ice concentration sensed via PM and play a key physical role in Antarctic sea ice evolution. Yet the author provides no supporting information that (a) indeed, the 15-80% threshold does not co-vary with these core sea ice physical properties, or that (b) a  $\sigma$  threshold is better, or is related to "ice type" at all.

For example, this statement in the discussion: "the proposed analysis will map relative differences between ice types, even if the specific ice type cannot be classified". But how is this true? But what, other than anomalous variability in reported SIC, is actually being

measured by this metric? Why does this have anything to do with ice type, and what is the author actually referring to here by "ice type"?

The author does not provide a physical basis for *how* the MIZ should be defined, anyways, using different terminology at different points throughout before settling on (L281) "variability". Their variability is by construction the anomalous temporal variability of PM-SIC retrievals. But what the author also emphasizes, as tends to be the case in the literature, is that the MIZ is characterized visually by horizontal variability, i.e. in terms of floe-to-floe heterogeneity, not necessarily temporal variability. Why one should be interchanged with the other is not clear.

The evidence supporting the use of this new definition is in part that all four products agree on a climatological seasonal cycle of MIZ extent. The NOAA/NSIDC CDR product used here is simply the maximum value of the NT/BT algorithms (<https://doi.org/10.7265/efmz-2t65>). Thus the apparent spread in algorithms presented in Fig 5a is in part artificial as NT/BT largely agree, and the CDR product must be smaller than both by definition and should not be compared. As for why the OSI-SAF product produces a more wide distribution of SICs, this has its own substantial literature (e.g. Kern 2019/2020). These algorithms also agree on other metrics too, like SIE. So a global metric with agreement is not altogether all that motivating - there are ways that we know these algorithms all agree, and it may be that the metric you obtain is covariant with one of those. Still, figuring out whether the agreement is "real" requires some further work.

First, it is not necessarily clear they are agreeing for the right reasons: it would be useful to check the marginal ice zone fraction (Horvat, 2021) in concert with the MIZ extent (Rolph et al 2020), as this illustrates whether this agreement is consistent with the same sea ice coverage in general. As the author indicates the use of  $\sigma$  can give rise to broaden extents, is it possible that this is covariant with larger  $\sigma$ -MIZs? Additionally looking at the spatial coherence of the MIZ definition between different products will also indicate if the  $\sigma$  value is the same locationally, or if the definitions agree only when integrated globally.

Further, the author clearly notes that two processes can give rise to high values of  $\sigma$ : broad-scale thermodynamic processes that cause the ice edge to retreat/expand, or pixel-scale variability (perhaps caused by storms, though this is not spelled out in detail). There is no exploration of which actually drives this change, but it is sorely needed: a physical driver over  $\sigma$  values should be foundational to its definition. As mentioned, one very important thing we do know is that all PM-SIC algorithms largely agree on Antarctic sea ice area and extent - so it is possible they also should have similar retreat/extent patterns of the sea ice edge. If this is the leading cause of elevated  $\sigma$  values, then the algorithms would agree -  $\sigma$  values are simply reflecting a synoptic change which could equally well be observed in the SIC values alone. It might be easy to check this, too - if all monthly values are declining or increasing, then the variability being measured is expansion or retreat of the ice edge, and not intra-monthly heterogeneity in the sea ice.

I could, for example, propose a wholly different metric: what if you produced daily maps of the SIC-threshold MIZ (i.e. identified points with 15-80% SIC every day), and averaged this binary indicator over each month instead of defining the threshold on the monthly climatology? How different would this look from the "variability" metric, e.g. in Figs 3-4? Why is this metric any better or worse?

Finally, there is no discussion of the influence on retrieval uncertainty on  $\sigma$  results, and there ought to be. Such errors directly impact the variability measure but will not impact the SIC thresholding (unless occurring at 15% or 80% SIC), which is why extent and the MIZ are designed in the way they are. There can be immense variability day-to-day, and errors for non-compact ice can be high. Without a formal assessment of the impact of measurement uncertainty, it is not possible to assess whether there is any true variability being measured. A particular problem raised in the PM observational literature is the "truncation" of SIC estimates (see Kern et al 2019) - most algorithms frequently can return  $SIC > 1$ , and then set  $SIC > 1$  to 1. But this can bias the statistics of metrics like  $\sigma$ , and shouldn't because it reflect a real "observation". The OSI-450 product is a good choice here because it actually reports the true SIC estimate, which can be used in your assessment of the variability and extent (this field is `raw_ice_conc_values` in OSI-450 output).

Finally, the discussion circles around the meaning of variability without doing any direct comparison to other observations. I have mentioned the many asides to MIZ physics and ice types, which is not reflected in the product itself (and is readily admitted by the author, see L270), nor supported in the analysis. These should be a major part of what

makes this definition useful, but they do not support its inclusion.

## Suggestions

---

I make two overarching suggestions which I hope would render this article a significant contribution to the sea-ice literature.

First, the author should relate the new definition to some physical properties of the sea ice cover relevant for those who might be interested in this definition. It is true that the current MIZ definition was simply defined operationally. But an alternate definition should have additional reasons for its suggestion. This would require the use of alternate data, i.e. a case study in a particular region with imagery, or similar, to give evidence that high  $\sigma$  regions are indeed compatible with a physical definition of the MIZ. Datasets on sea ice age, floe size, waves, surface roughness, etc, all do exist and could be used to further this effort.

Second, the author should separate the aforementioned sources of variability into that due to ice-edge retreat, real inter-monthly variability in ice conditions, and PM uncertainty. This is necessary to know whether  $\sigma$  actually contains useful information or is just reflecting uncertainty at the ice edge. Perhaps it is! That might be a useful back-door way of observing the MIZ, but without knowing it is impossible to do more than speculate.

If the proposed MIZ definition can be better grounded physically, and its relationship to sources of uncertainty that are not MIZ-related can be sorted out, I think that would make a publishable contribution to the scientific literature.

References:

-----

Ivanova et al. Inter-comparison and evaluation of sea ice algorithms: towards further identification of challenges and optimal approach using passive microwave observations. 2015.

Kern et al Satellite passive microwave sea-ice concentration data set inter-comparison for Arctic summer conditions (2020)

Kern et al Satellite passive microwave sea-ice concentration data set intercomparison: closed ice and ship-based observations (2019)

Horvat. Marginal ice zone fraction benchmarks sea ice and climate model skill. (2021).

Rolph et al. Changes of the Arctic marginal ice zone during the satellite era. 2020.