# Reply on RC2

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Author comment on "Rotational drift in Antarctic sea ice: pronounced cyclonic features and differences between data products" by Wayne de Jager and Marcello Vichi, The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-144-AC2, 2021

Dear Dr Ludwig Valentin

Firstly, I would like to thank you for the time you have given us and the constructive suggestions to improve the quality of our paper. Your efforts are very much appreciated, and we look forward to addressing your comments when preparing the revised version.

Below we have responded to each of your major and some minor comments. Your original comment appears in italics, while our response appears in bold.

2. General comments

*"The paper is well-written and mostly easy to comprehend. The results are enough, both concerning relevance and quantity, to warrant publication as brief communication. My main point of criticism is that the paper is rather short on the discussion of the results. The finding that the merged product shows more high-intensity features than the singlesensor products is really interesting, but the authors do not give a reason. I understand that an in-depth analysis is beyond the scope of a brief communication, but I would like to see which ideas the authors have for further research, so that a follow-up study could build upon their work. The following questions came to my mind:*

- *What is the reason for the above-mentioned mismatch between the merged product and the single-sensor products?*
- *How do you judge this mismatch? Is it an artifact arising from the merging method or does it bring additional insight which the single-sensor products can not provide?*

*I would ask the authors to elaborate on this in their Discussion section. Also, it would be good to know if similar results have been achieved by other studies."*

**A major component of our revision will be to do a dedicated analysis on the propagation of uncertainties when computing the mean vorticity of a rotational feature in the ice. It is therefore necessary to first do this analysis to help answer both questions. Should the uncertainty analysis show that the product**

mismatch is more than just noise, then the product discrepancy can be further analyzed. A preliminary analysis based on the uncertainty propagation methods discussed in Dierking et al. (2019) and suggested by the other reviewer Dr Lavergne, indicates that the error is of lower magnitude and that the difference is to be attributed to the products. Based on our findings, we will discuss the implications in the revised version. We will offer some hypotheses in the Discussion section, although only the availability of more in situ data in later studies will allow us to determine the relative quality of each product and the advantage of the merged data.

According to our knowledge, this is the first study that used multiple products to quantify rotational features in sea ice. Other studies focused on the use of individual products to determine changes in sea ice dynamics over time (Holland and Kwok, 2012; Kwok et al., 2017). This motivated our work to initially compare the products for a further quantification of the longer-term trends.

### 3.1 Abstract comments

**Thank you for your comments on our abstract, they will all be included in the revised version.**

### 3.2 Introduction comments

*"L29: Do "scarce" and "sparse" not effectively mean the same thing?"*

**We wish to communicate to the reader that not only are in situ measurements of ice drift uncommon and insufficient to meet our analytical requirements particularly in the winter's consolidated ice field (scarce), but also that these few measurements are widely spread across hundreds of kilometers of sea ice (spares) which makes monitoring even large-scale dynamical features difficult. This concept will be clarified in the revised version.**

*"L36: I would suggest to replace "ice edge" by "marginal ice zone", there is seldom a sharp and abrupt transition between sea ice and ocean which would justify the term "edge"."*

**The identification of an "ice edge" is necessary since it is an important component of the OSISAF-405c product's drift retrieval algorithm, and by extension our vorticity computation. However, we do agree with the reviewer comment, and we will make it clearer that the 'edge' is a construct of the processing algorithm and data quality flags, and not a physical line between ice and ocean.**

*"L74: What exactly is the method which you propose? Taking the maxima and minima of*

*the vorticity within the domain as described in L114-L120? Woud be good to state this more clearly, to me it was not immediately clear although it was the initial motivation for your paper."*

**In our revised version, we will add more details on the processing steps taken to extract the mean vorticity of each feature. To summarize briefly, our processing does find the maxima and minima of the vorticity for each domain, but all valid vorticity features (i.e., features which meet the valid pixel criteria of 80/85/90%) across the entire domain are pooled into cyclonic and anticyclonic categories, and then the intensity distribution is done using all detected features. In the revision we will also consider the role of using different quality flags and the related uncertainties. The daily maxima/minima features were used in the spatial distribution analysis which is briefly mentioned in Discussion and Conclusion. We intend to clarify this in the revised version.**

**We thank you for the more minor comments made regarding our choice of wording/phrasing in our introduction, as well as the reference provided.**

3.3 Data comments

*"L90: What is meant by "SSMI/S instrument range"? Please specify"*

**We were referring to the SSMI-F15, SSMI/S-F17 and the SSMI/S-F18 instruments. We will from now, more appropriately, describe this collection of instruments as a "family" as suggested by another referee. We decided to group these instruments together as the objectives of these missions were to launch a successor to each instrument such that an uninterrupted, continuous supply of measurements was available.**

*"L94: weighted by what?"*

**The weighting of each single-sensor data is inversely proportional to the error of that product as shown by its validation. Therefore, the lower the uncertainty of the single-sensor product, the higher its weighting in the computation of the merged product. We will elaborate on this in the revised version as described by Lavergne et al. (2010).**

*"L97: Can you comment on the typical size of the cyclones which you detect in relation to the grid spacing of 62.5 km? Be careful to not mix up grid spacing and resolution."*

**The rotational features found on sea ice may originate from both oceanic and atmospheric drivers, and possibly a combination of both. Less is known about the role of mesoscale and sub-mesoscale oceanic processes in winter, although some initial studies indicate that sub-mesoscale processes may be relevant under ice (Biddle and Swart, 2020). There is larger evidence of the role played by atmospheric cyclones in driving sea-ice motion (Vichi et al, 2019 and references therein). These large-scale synoptic features are of the order of 1000 km. Our analysis is therefore oriented towards capturing these kinds of events,**

assuming that sea ice would be affected at the same scale of the synoptic events. The analysis was done by forcing the search domain as a circle, and we tested the sensitivity to 400/450/500 km radius. The domain included approximately 150-210 grid points.  The product grid size is therefore sufficient to resolve the features of interest. We will add a comment on this choice in the method section.

*"L101: Please specify the projection (NSIDC projection with the latitude of true scale at 70◦S?) or give a reference."*

The original grid of the OSISAF-405 drift products was used. This is the NSIDC polar stereographic projection with the latitude and longitude of the projection center at 90°S and 0°E respectively, and the latitude of true scale at 70°S. We will add the appropriate reference in the revised manuscript.

3.4 Methodology Comments

*"L115: If you choose the maxima/minima of the mean vorticities, you might get into trouble if there are outliers which are not representative of typical cyclonic/anticyclonic features. Can you comment on this? Did you compare the results which you get by taking the extreme values to the results which you would get if using a more robust estimator like the 95th percentile? Would you expect differences arising from this? Please briefly discuss."*

As mentioned earlier, we intend to clarify in our methods section how features are identified and categorized. Regarding your comment on outliers, we agree that erroneously high vorticity pixels will impact the mean vorticity within the search domain. Currently the influence of these high pixels is reduced by the minimum valid pixel requirement, meaning any detected feature will have at least 80/85/90% non-missing vorticity values, which is approximately 190 valid vorticity values for each feature. As indicated in an earlier answer, in our revised version we intend to include the quality of measurements according to the status flag information provided in the dataset, as well as propagate the uncertainty measurements into our vorticity computation. This will allow us to quantify the confidence of our vorticity measurements. A spatial analysis was done on 'significant' features (i.e., the 95th percentile) and briefly mentioned in Discussion and Conclusions, but no frequency distribution was done using these features. According to the tails of the curves shown in Figure 3, we would still expect the merged product to detect higher magnitudes of rotation in the ice, however, removing the lower measured features from the normalized distribution would show this better. This will be considered for the revised section.

Thank you for your comments in this section. In the revised version, we will include references for the single drift products and clarify the spatial and temporal range of our analysis.

3.5 Results comments

*"General: Please state how many data points there were per year. Was it always the same number?"*

**Due to the large differences in coverage between products and the forced valid vorticity threshold, the number of detected features varied between products. The best coverage is expectedly provided by the merged product, where about 40 000 features were detected per winter (June 1st - November 1st). Conversely, the ASCAT's poor coverage often resulted in as little as 2000 features per winter.**

**Thank you for your suggestions on how to improve the presentation of our results and for your comments on the clarification of the terminology used. We will consider all of these points in our revised version.**

3.6 Discussion and Conclusions

*"L187: ". . . increasing trend. . . ": Do you refer to the spread or to the absolute values of vorticity?"*

**In this case, we were referring to the increased spread since the lower whisker remains mostly constant, while the IQR increases in both spread and absolute values, suggesting that there is a more frequent occurrence of higher intensity features detected over time. This will be better explained in the revised version.**

*"L187: Please discuss the robustness of this trend, given that your study period is quite short. Is this also found by other studies?"*

**The aim of this brief communication is to comment on any discrepancies in the detection of vorticity features between the drift products, rather than to comment on any trends seen in the timeseries from 2015-2020. This is the necessary first step to establish how confidently we can use these products to analyze trends in vorticity in future work, once the timeseries are long enough to do so. As mentioned previously, we will be incorporating an uncertainty propagation analysis in our revised version, such that we can quantify the robustness of detected trends in future work.**

*"L197-202: Very interesting indeed to see that the merged product shows more vorticity than any of the others. I would like to see this discussed in more detail. An in-depth discussion would probably be too much, but can you elaborate on potential reasons or give directions for future research? Also, do you trust this result? Would be good to get an idea whether the additionally introduced rotation is valuable information which we can not get from the single-sensor observations or whether it is an artifact of the merging."*

**As mentioned, we will be including an analysis of the uncertainty propagation in**

the vorticity metric. This will both help quantify the confidence with which we report our results and - should the trend be more than just noise - provide additional knowledge as to why this discrepancy between products exists. We also intend to clarify that the merged product is not necessarily overestimating the vorticity of these features, but that the single sensors may be underestimating the vorticity. Incorporating some in situ data into our analysis may allow us to discern which of these scenarios is true.

*"L204-205: Please give a reference or explain why you expect disproportionately high frequency of low-intensity features in the Eastern Weddell Sea."*

This is a result of the spatial coverage of the ASCAT instrument. This region was the most consistently measured area by the ASCAT instrument, and so when considering the 80/85/90% valid value threshold requirement, it is in this region where the coverage consistently meets this condition. Conversely, the patchiness of the ASCAT coverage in other regions meant that this threshold criteria was rarely met, and therefore no vorticity feature was defined. In the revised version we intend to elaborate on the reasons for the coverage discrepancies between products and how this could affect our results.

*"L212-219: Please give directions/ideas how to find out the reason for the mismatch in the cyclonic drift features."*

As mentioned previously, we intend to do an uncertainty analysis in our revised version, as also requested by the other reviewer. This will provide additional knowledge on the confidence to which we can comment on the trend discrepancy between products.

References

Biddle, L. C., & Swart, S. The observed seasonal cycle of submesoscale processes in the Antarctic marginal ice zone. Journal of Geophysical Research: Oceans, 125. https://doi.org/10.1029/2019JC015587. 2020.

Holland, P. R. and Kwok, R.: Wind-driven trends in Antarctic sea-ice drift, Nat. Geosci., 5(12), 872–875, doi:10.1038/ngeo1627, 2012.

Kwok, R., Pang, S. S. and Kacimi, S.: Sea ice drift in the Southern Ocean: Regional patterns, variability, and trends, edited by J. W. Deming and E. C. Carmack, Elem. Sci. Anthr., 5, doi:10.1525/elementa.226, 2017.

Lavergne, T., Eastwood, S., Teffah, Z., Schyberg, H. and Breivik, L.-A.: Sea ice motion from low-resolution satellite sensors: An alternative method and its validation in the Arctic, J. Geophys. Res., 115(C10), C10032, doi:10.1029/2009JC005958, 2010.

Vichi, M., Eayrs, C., Alberello, A., Bekker, A., Bennetts, L., Holland, D., Jong, E., Joubert, W., MacHutchon, K., Messori, G., Mojica, J. F., Onorato, M., Saunders, C., Skatulla, S. and Toffoli, A.: Effects of an Explosive Polar Cyclone Crossing the Antarctic Marginal Ice Zone, Geophys. Res. Lett., 46(11), 5948–5958, doi:10.1029/2019GL082457, 2019.