

The Cryosphere Discuss., author comment AC1
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Reply on RC1

Wayne de Jager and Marcello Vichi

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-AC1>, 2021

Dear Dr Thomas Lavergne

Firstly, I would like to thank you for the time you have given to provide us with constructive suggestions to improve the quality of our paper. Your efforts are very much appreciated, and we look forward to taking your comments into consideration when preparing a 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**.

"In summary, I recommend that an analysis of the propagation of uncertainties to the vorticity metric is performed and added to the manuscript. The OSI SAF sea-ice drift products have documented the uncertainties they have on their drift components (through quantitative uncertainties and validation against buoys), and these should be used to tell 1) are the differences in vorticity observed between the products just the expression of a noise or are they seeing actually different vorticity patterns, and 2) is one of the products more accurate than the others in terms of vorticity, also concerning the "additional rotational energy" claim. If an uncertainty propagation analysis cannot be conducted, it is strongly recommended that a thorough discussion is added about the significance of the documented uncertainties on sea-ice drift components on the conclusions (again also concerning the "additional rotational energy"). "

We will be following your recommendation to do an uncertainty analysis for our revised version. We agree that this knowledge is valuable and so a complete analysis of the propagation of uncertainty to the vorticity metric will be done. This analysis will attempt to answer (a) if there is a real difference in the measured vorticity between products or if any discrepancy is just noise, and (b) if any discrepancy is found, to expand our analysis to better describe why this discrepancy may exist and which measurements are closer to the truth. A preliminary analysis has indicated that the mean uncertainty of the vorticity estimates is of lower magnitude than the difference between the data sets. This will be further confirmed and discussed in the revised manuscript.

"Page 4, Methodology question 1: how are missing vectors dealt with for the single-sensor products: ASCAT has many missing vectors especially at lower latitudes (outskirt of the domain), the multisensor product has many more. How is the vorticity computed in case a vector is missing?"

At every grid point i,j , the vorticity value is computed using vector measurements at $i-1, i+1, j-1$ and $j+1$. Therefore, for every missing displacement vector, no vorticity is computed for that grid point and its immediate neighbors. The computation of an ice feature's mean vorticity uses valid vorticity values within the circular domain only, and therefore the total vorticity value is simply divided by a smaller number should there be any missing values within the domain. The revised manuscript will indicate more clearly that there is a minimum number of valid vorticity measurements required, a parameter which was set at 80%, 85% and 90% with similar results in each repetition (results not shown but currently mentioned in Results and Discussion sections; this point will be further remarked in the revised version).

"Page 4, Methodology question 2: did you use all the vectors, irrespective of their status_flag, or did you remove some of the more dubious flags?"

Currently all vectors have been used irrespective of their status_flag. In our revised paper, we will be considering only displacement vectors with a corresponding uncertainty value, such that we can quantify any potential noise, as requested in the first comment.

"Page 4, Methodology question 3: Do the subdomains D_r overlap? Specify in the text. If yes, the vorticity events thus contributed several times?"

Yes, vorticity features in the subdomain can overlap in both space and time. This is because we are not attempting to count the number of vorticity events, but rather to compare the rotational energy in the ice as detected from various products. This will be specified in the text.

"Page 4, Methodology question 5: "Any subdomain with a mean vorticity of zero is ignored". It seems unlikely that the mean vorticity would return exactly 0. Is you test against 0 exactly, or within a range around 0 (what range?). If exactly 0 it could be worth stating in which (frequent) conditions the vorticity is exactly 0."

Yes, you are correct that a mean vorticity of zero is unlikely, in fact it did not occur at all. This comment was simply to inform the reader that in case a feature was measured with zero vorticity, it was neither characterized as a cyclonic or anticyclonic feature, and thus not weighing down either distribution's spreads. It will be rephrased in the revised manuscript.

"Page 4, Methodology question 6: Have you looked at the intensity of "significant" cyclonic and anti-cyclonic events (intensity of events above a vorticity threshold)? It could indicate if the difference you observe build from low-signal / noise events, and if the products agree better on the major events (that are possibly more relevant to the original objective of partitioning the dynamic vs thermodynamic contributions)?"

We agree with the reviewer on the importance of the events with significantly high vorticity. A spatial analysis was done on 'significant' features (i.e., the 90-95th percentiles) and briefly mentioned in Discussion and Conclusions. Here it was seen that the better coverage of the merged, AMSR-2, and SSMI family products detected a similar spatial distribution of the significant features, while the ASCAT product is spatially limited to the Weddell Sea due to the large number of missing values. An 'intensity spread' analysis will be considered for the revised submission and included within the limitations of the brief comment format.

"Page 8, line 198: Here again, the merged product is described as detecting a "disproportionally large frequency" but what if it is the most accurate of the four, and it is the larger noise in the 3 other products that leads to an underestimation of the high intensity features?"

We believe that describing the relative distribution as "disproportionately large" does not imply that one frequency distribution is more correct than another, but rather that it is relatively unique when compared to the other three products. We understand that this qualifier may be misinterpreted as if "out of proportions" would indicate a discrepancy from a known value. The wording will be improved in the revised manuscript.

"Page 8, first lines: Here again, what is the impact of the multi-sensor product having fewer missing vectors than the 3 single-sensor products?"

This is what our results presented in the manuscript indicate. The reduction in gaps is likely to induce a change in the distribution of the more intense cyclonic events detected in the region. There is indeed an impact, but currently, in the absence of independent observations that would corroborate our findings, we are unable to identify whether this is an artifact or a feature. This discussion will be included in the revised manuscript.

Thank you for your comments on typos and editorial suggestions. We will address all of your comments in the revised version.