Review of tc-2021-144: Lack of agreement in remote sensing detection of cyclonic drift caused by Atlantic weather in Antarctic sea ice.
By: W. Jager and M. Vichi

The authors are interested in discriminating the relative contribution of thermodynamic and dynamic processes to the year-to-year variability and trends of the sea-ice cover in the Southern Ocean. Since satellite-based sea-ice concentration data alone do not allow such partition, they look at satellite-based sea-ice drift fields and specifically the detection and quantification of cyclonic and anticyclonic drift patterns in the Atlantic sector of the Southern Ocean. They access four sea-ice drift products from the EUMETSAT OSI SAF (one from ASCAT, one from AMSR2, one from SSMIS, and a multi-sensor product combining the first three) and report large discrepancies between the relative vorticity fields computed from these four products, both in the annual distributions and the general distribution of the intensity features detected. The authors conclude that the observed disagreement between the satellite-based products impede further analysis of the partition of dynamic and thermodynamic contributions to the evolution of the sea-ice cover.

This manuscript is framed as a short communication in which the authors report on a negative result: the discrepancy between the relative vorticity fields derived from satellite-based sea-ice drift products is such that these cannot be used for the intended purpose. The satellite-based products should be made more consistent, the vorticity results be more similar, before they can be used in further scientific analysis. The satellite-based products are not reliable enough for this type of analysis.

I have a major comment about this manuscript (leading to requesting additional analysis or at least adding a detailed discussion), and a series of more minor comments.

Major comment:

No satellite-based product is a perfect measurement. The noise in the raw satellite observations and the uncertainties introduced by the retrieval algorithms all contribute to a retrieval noise. In addition, differences in timing of the various satellites orbiting the Earth can result in representativity uncertainties: the same algorithm applied to different satellite missions will result in different geophysical fields just because the timing of the observations are different. The EUMETSAT OSI SAF sea-ice products do provide numerical estimates of these uncertainties in the product files (since June 1st 2017) and has conducted validation against buoy data (see the validation reports). The uncertainties in the components of the sea-ice drift vectors will naturally propagate into uncertainties of the vorticity metric. How the drift uncertainty propagates into the vorticity metric can be estimated theoretically (Dierking et al., 2020) or numerically e.g. using Monte-Carlo simulations. An analysis of the propagation of uncertainty from the uncertainty in the drift components to the vorticity metric, and how the uncertainty on vorticity relates to the differences observed between the four products is missing. For example, do the 4 vorticities agree within their error bars, or are they really returning “no agreeable pattern” as is stated (page 5, line 132)? In other words: are all 4 products seeing the same signal but with a lot of noise (the products are collectively inadequate for this application), or are they seeing different signals (in which case some of the products can be better OR the timing and spatial coverage differences have a large impact on the vorticity)? Without this analysis, we cannot put an error bar on the retrieved vorticity values, and cannot discuss if one of the satellite-based product is more reliable than the others for estimating vorticity. This would however have been an interesting results of this study: if one of the product leads to a lower uncertainty on the vorticity, then this product could potentially be used for later analysis (it would be a more useful conclusion than stating that none of the products are usable).

This is also relevant when it is observed that “the processing chain used in the development of the multi-sensor merged ice drift product can induce additional rotational energy into the resultant vector field”. It is known (e.g. from the validation reports produced with the OSI SAF sea-ice drift products) that the multi-sensor product is “smoother” (in space) than the single-sensor products, because it reduces the retrieval uncertainties but also act as an averager of the daily sub-drift variability (captured by the single-sensor products at different observation times). It is thus maybe not surprising that the multi-sensor product shows higher “rotational energy” and it might very well be that it is the more correct product (your phrasing suggests that energy is added by the algorithm, while it would rather be that the single-sensor products miss some of the energy because they are more noisy).

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”).

Other comments and questions:

Page 2, line 34: Isn’t SIC rather defined as the proportion of ice-covered water to total area (ice-covered or not).

Page 4, line 111: “horizontal and vertical components” this refers to a 2D project map with components along the vertical and horizontal directions of the grid, but can be mis-read as the vertical (3D) component of the sea-ice drift. You mean “x and y axis of the grid”.

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 multi-sensor product has many more. How is the vorticity computed in case a vector is missing?

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?

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

Page 4, Methodology question 4: At the beginning of section 4 you refer to the “intensity distribution … identified by the algorithm...” but your section 3 Methodology does not clearly define an “intensity”. Add this in section 3, or be more specific in section 4.

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.

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)?

Page 8, line 181: Before stating that there is a “a large discrepancy between products”, we need to quantify what “large” is wrt the uncertainties: is it just noise or really discrepancies (observing different signals).

Page 8, line 198: Here again, the merged product is described as detecting a “disproportionaly 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?

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?

Typos and editorial suggestions:

Page 3: line 71: missing “is” (it is necessary).

Page 3: line 74: consider having “detection” before “quantification” (1st detect, then quantify?)

Page 3, line 78: maybe “shared” or “common” would be better than “unique”?

Page 3, line 85: “range” → “family”

Page 3: line 93: The multi-sensor merged product does more than “treats missing data...”, suggest replacing “treats missing data by means of a two-step” with “implements a two-step”

Page 4, line 113: “dx and dy are the grid spacing (62.5km)”.

Page 5 Figure 1: suggest to write the product name (ASCAT, AMSR, etc…) in the plot area in addition to of (a), (b), etc…

Page 6 Figure 2: same suggestion as for Figure 1.

References:

Dierking, W., Stern, H. L., and Hutchings, J. K.: Estimating statistical errors in retrievals of ice velocity and deformation parameters from satellite images and buoy arrays, The Cryosphere, 14, 2999–3016, https://doi.org/10.5194/tc-14-2999-2020, 2020.