Review of paper "Brief communication: lack of agreement in remote sensing detection of cyclonic drift caused by Atlantic weather in Antarctic sea ice"

1 Content

The authors de Jager and Vichi present a brief study on the intercomparison of four sea-ice drift datasets, namely the OSI-SAF merged drift product OSI-405-c and its three constituents. These are drift products derived from AMSR2, ASCAT and SSMI/S. The study focuses on the detection of cyclonic and anticyclonic rotation in the Atlantic sector of the Antarctic pack ice zone. For the years 2015-2020, the most intense cyclonic and anticyclonic features of each 48-hour period are compared statistically. The authors find that there is stronger cyclonic than anticyclonic vorticity. Comparing the products, the authors report that the merged product shows more high cyclonic vorticity values than each of the input products. This is interpreted by the authors as additional energy introduced by the merging scheme.

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 single-sensor 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.

3 Specific comments

3.1 Abstract

L11: Concerning the word "alternative": A bit misleading because it sounds as if the concept of sea-ice extent (SIE) would be used to quantify changes in sea-ice dynamics. I suggest to leave out "alternative".
L18: For me, the processing chain is merely the technical implementation of the merging method, therefore I would suggest to refer to the merging method here instead of the processing chain.
L18/19: I suggest to add that only cyclonic momentum is added.

3.2 Introduction

General: Should mention that, unlike in the Arctic, SIE in the Antarctic was quite constant until recently
General: When describing your motivation, you might also want to mention more explicitly that we expect increased sea-ice drift in future, given the thinning of sea ice and the increased storminess.
L29: Do "scarce" and "sparse" not effectively mean the same thing?
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"
L38: Why is the variability dramatic? I would suggest something more objective and less drastic like "high" or "pronounced"
L39-45: Talking about limitations of SIE, you might also want to refer to Notz (2014)
L56-58: I suggest to restructure and split the sentence: "Ice movement is primarily driven by .... Other factors are waves, ocean tilt...".
L68: Please provide a reference for your statement that the Southern ocean hosts some of the most energetic storms worldwide.
L69: MIZ has not been defined yet.
L71: Much has changed since 2003/2004, please provide more up-to-date references. Also, "it therefore" should be "it is therefore".
L74: Instead of "daily timescales", you could be more specific and speak of "two-daily resolution"
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.
L78: As outlined above, I doubt that the term "processing chain" is appropriate here and suggest to replace it by "merging method" or something similar
L82-83: Isn't your conclusion that the merging introduces additional cyclonic rotation?

In this case, you can also write this here instead of using the weaker formulation "... can induce additional...". Also, you could specify already here that the additional rotational energy comes from cyclonic rotation.

3.3 Data

General: Please provide references for the single drift products. Also, it would be good if you can state here which region and months you use.
L90: What is meant by "SSMI/S instrument range"? Please specify.
L94: weighted by what?
L97: I think "coarse" would be more appropriate than "large" when speaking of resolution.
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.
L101: Please specify the projection (NSIDC projection with the latitude of true scale at 70°S?) or give a reference.

3.4 Methodology

L105: The readability here and in the rest of the paper would be better if you could adopt the practice to use "sea-ice" when speaking about sea-ice properties (sea-ice vorticity, sea-ice concentration etc) and use "sea ice" when referring to it as a noun.
L107-110: Domain and months should be specified in the Data Section. It would also be good if you could state how large the area is in km^2. What is your criterion for the "ice-covered area"? SIC above 15 %? Please state this here.
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.

3.5 Results

General: Please state how many data points there were per year. Was it always the same number?
Figure 1: If your main goal is to compare the products among each other, it might make more sense to have one panel per year instead of one panel per product. If it does not overload the plots, you could also consider merging the panels a–d to one and mark the products by different colors. With the current alignment, I find it hard to compare the results of single years between sensors. Same for Fig. 2.
L124-126: This technical description of the box-and-whisker plot could be moved to the

caption of the Figure.
L131: This is the kind of statement which is hard for me to assess if the four ice motion products are shown in separate panels. Can you give the values to which you refer here in a Table?
L132ff: What exactly do you refer to by "spread"? Interquartile range? Range between whiskers? Further, it would be good to also at least mention the actual magnitude of the cyclonic features, not only the spread, even if the latter is your main focus.
L139: What do you mean by "high levels of interannual variability"? I do not have the impression that the medians or interquartile ranges in Fig. 1b vary more than in a, c or d.
L141: "which being detected": Should this be "which was detected"?
L144: Should mention the reduced y-axis range of Fig. 2 compared to Fig. 1.
L170-176: Please put the IQR and σ values in a Table, this would be much easier to grasp and would improve the readability.

3.6 Discussion and Conclusions

L187: "...increasing trend...": Do you refer to the spread or to the absolute values of vorticity?
L187: Please discuss the robustness of this trend, given that your study period is quite short. Is this also found by other studies?
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.
L204-205: Please give a reference or explain why you expect disproportionately high frequency of low-intensity features in the Eastern Weddell Sea.
L209: See my comment to your L74. Please describe the method briefly, since it was the main motivation for your work.
L212-219: Please give directions/ideas how to find out the reason for the mismatch in the cyclonic drift features.

References

D. Notz. Sea-ice extent and its trend provide limited metrics of model performance. *The Cryosphere*, 8(1):229–243, 2014. doi: 10.5194/tc-8-229-2014. URL `https://tc.copernicus.org/articles/8/229/2014/`.