TC-144-2021

Overview of our revised manuscript:

Following the suggestions made by the reviewers – as well as the handling editor's suggestion to expand the paper beyond a brief communication – we have modified and added a considerable amount to our revised version. Due to the significance of the new results and our intention to provide a logical flow in communicating our results, we have also changed the ordering of some of our previous results as described in the following.

Firstly, we have added a further component to our revised manuscript which investigates the role of uncertainty propagation in the computation of the vorticity, as this was a major suggestion made by Reviewer 1 (Sections 3 and 4.1). To do this, we ignored drift estimates corresponding with the rejection quality flags (i.e., flag values 0-20 in the OSI-405-c products), and only used those with a quality index flag 20-30. This decision was taken to ensure that there is sufficient ice-covered area to perform our analysis for all the products. While we acknowledge that quality flags 20, 21 and 22 may be of degraded quality, the displacement uncertainty of these flags is made available in the dataset (as of 1st June 2017) and thus we can quantify this potential noise. The method of our uncertainty propagation can be found as described in Section 2.4 of Dierking et al. (2020). To reduce the influence of using non-nominal quality flags, we decided to redefine our period of analysis to between 1st June – 31st October (previously was 1st April – 30th November). This was decided because the freezing months have a higher proportion of nominal-quality drift estimates. Furthermore, the initial manuscript showed results between 65° W and 10° E, but the revised manuscript shows results between 65° W and 50° E. This change was made to reduce the influence of the region boundary 10° E, an area where sea-ice coverage band is wide. The sea-ice coverage at 50° E is thin relative to the scale of features, and so no features are detected here because of the insufficient ice cover, and not by an artificial boundary parameter. We found that the uncertainty of the vorticity metric was 3 orders of magnitude smaller than the mean vorticity of the same area (revised Fig. 1), and so with these results we can assess trends in feature intensity more confidently.

Furthermore, the spatial variability of vorticity measurements within the circumference of the detected features was also analyzed. This was done to contrast the relative significance of the feature's uncertainty compared to its variability, and it was found that the variability is of the same order of magnitude as the feature intensity (revised Fig. 2), and so should be considered when investigating trends.

We then expanded our analysis to include year 2016-2020. This decision was made to maximize the period of overlap between products, mostly limited by the September 2015 launch of the AMSR-2. All features (not just the daily maxima and minima) in this period are then compared for both cyclonic and anticyclonic features (revised Fig. 3). Here we see both differences between cyclonic and anticyclonic distributions, and the same (though reduced) discrepancy of cyclonic distributions between products that was reported in our first submission.

Finally, it was suggested by both reviewers that we consider an analysis of the most 'significant' features. We agreed that this would be a useful addition to our revised manuscript. To do this, we defined our "major features" as the 95th percentile of most intense features independently for both

cyclonic and anticyclonic features, where the intensity of a feature is defined as the mean vorticity of the area within its circumference. Figure 3 now includes a visualization of the percentile threshold. We then extend our analysis back further to include all available OSI-405-c drift estimates, which starts in 2013 for the Southern Ocean. However, when analyzing the interannual variability, it was noticed that no major cyclonic features from 2013 or 2014 exceeded the 95th percentile threshold (similarly for the 90th percentile too). This meant that using a fixed percentile threshold was not an effective means to communicate the results of the trend analysis. We therefore decided to quantify a 95th percentile threshold independently per year. This means that the results discussed here do not describe the interannual variability relative to a fixed-in-time rotational drift intensity, but rather how the most intense features of each year vary relative to that of other years. This decision allowed us to show the pronounced change in the distribution of the most intense events of each year. More specifically, it is shown that there is little change in the distribution of major anticyclonic events, but substantial change in the distribution of major cyclonic events (revised Fig. 4). There is a rapid increase in the intensity of the major events from 2014-2017.

The results of our revised version concluded that:

1. **The uncertainty of the vorticity metric is very small when using the OSI-405-c product.**
2. **There is a dominance of cyclonic drift detected by all products, but the products indicate different distributions. This discrepancy is particularly large when using the merged product, which is characterized by the most intense cyclones. Given our analysis of the uncertainties and variability in relation to the intensity of the events, we speculate that this may be a consequence of the increased number of features introduced during the merging and 48-hour detection window.**
3. **There is an abrupt increase in cyclonic drift between 2014-2017, which we potentially attribute to the increased polar storms reported in the literature during this period.**

Response to Reviewer 1

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.

In this document we have provided an overview of the major changes we have made for our revised manuscript after acting upon the suggestions made by you and Reviewer 2. We have also responded to each of your specific comments, and those of Reviewer 2, in a point-by-point format. Your original comment appears in *italics*, while our response appears in **bold**.

Responses to your specific 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").

As mentioned in the above overview, we have included a uncertainty propagation in our analysis. The uncertainty was estimated theoretically in Sec. 2 following Dierking et al. (2020) as suggested by the reviewer and assessed the results in Sec. 4.1 in the new Fig. 1. We complemented this analysis with an additional paragraph and a new Fig. 2, which allowed us to better qualify the differences between the products. This gives us confidence in our assessment of the product discrepancies communicated in our revised manuscript. It is worth noting, however, that while we do consider the role of uncertainty in our analysis, assessing the performance of each product in would require a full vorticity validation against *in situ* measurements, such as those measured by an array of drift boys, etc. Currently, there is not a sufficient collection of *in situ* measurements to do this analysis, and so the revised manuscript just highlights the performances of each product relative to the others. We have therefore carefully revised our phasing as not to imply that any product is more correct than another.

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

Corrected. Our revised definition of SIC is "a measure of the proportion of ice-covered water to total area" (Sec. 1)

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".

Corrected. Our revised phrasing in Sec. 3 refers to the x and y axis of the grid, and u and v components of the drift vectors are referred to as zonal and meridional drift components respectively.

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. The finite difference formulation has been added to the revised manuscript in Eq. (2). 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 version has clarified in Sec. 3 that "A minimum pixel validity threshold of T is applied to every subdomain D_r, ensuring that each classified feature has an adequate number of valid vorticity values within its circumference. Subdomains that fail to meet the minimum pixel validity threshold are ignored, thus reducing the algorithm's susceptibility to classifying small regions of intense vorticity at the ice edge or coastline as features. This process is repeated independently per product with varying r (500, 450 and 400 km) and T (90, 85 and 80 %) parameter values, meaning that all identified features contain 180-220 valid vorticity values within their circumference, depending on the choice of r and T". For the ASCAT sensor, most of the features detected occur in the Weddell Sea, as this is the region in which drift estimates can consistently be retrieved. The overall number of features detected using the ASCAT product is thus expectedly lower than that of the other products because its area of ice drift estimates is smaller. To overcome these limitations, in the revised analysis we have extended the region to 50°E and redefined our period of analysis to 1st June – 31st October (previously 1st April – 30th November) because the freezing months have a larger coverage of drift estimates, and they are of a higher quality flag value.

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?

We thank the reviewer for this suggestion that made us reconsider the initial analysis and strengthen it in its present form. In our revised manuscript, we used drift estimates flagged of values 20-30 (previously we used all available drift estimates, irrespective of flag status). While we acknowledge that flags 20, 21 and 22 may be of degraded quality, this potential noise can be quantified in our uncertainty analysis. Furthermore, as mentioned above, we have decided to redefine our period of analysis to 1st June – 31st October (previously 1st April – 30th November) because the freezing months have a larger proportion of higher quality flag values.

Page 4, Methodology question 3: Do the subdomains Dr 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. It has been specified in in Sec. 3 that "Each of these subdomains represent a vorticity feature, which can overlap in space or in time, but never in both".

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.

Corrected. In our revised version, we define feature intensity as "the mean vorticity of all values contained within its circumference" in our methodology section (Sec. 3).

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.*

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 spread. This has been rephrased in Sec. 3 as "We define the feature intensity as the mean vorticity of all values contained within its circumference, and the feature variability as the standard deviation of all vorticity values contained within its circumference. Therefore, a negative intensity feature represents a circular area of sea ice dominated by cyclonic rotation, while a positive intensity feature represents an area dominated by anticyclonic rotation."

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 followed yours and Reviewer 2's suggestion and added an analysis on the intensity of "significant features" in the revised version. A temporal analysis was done on the most intense features (here defined as the 95th percentiles) and a new section 4.2 was added. This 95th percentile was then compared against products between 2013-2020 (or 2015-2020 for the AMSR-2 product due to the availability of data). The results, further discussed in Sec. 5, revealed an abrupt increase in cyclonic drift between 2014-2017, which we potentially attributed to the increased polar storms reported in the literature during this period.

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).

We have rephrased this in our revised version and commented on the role of uncertainties in our analysis. As described in an earlier answer to your comments, we have found that the uncertainties of the vorticity measurements are negligible, and that these discrepancies are not a symptom of the uncertainty of drift retrieval (Sec. 4.1).

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?

We have rephased this so as not to imply that any product is more or less accurate. Our uncertainty analysis also indicates that the uncertainty in the vorticity caused by the propagation of the drift uncertainty does not play a major role in the discrepancies detected (Sec. 4.1).

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. Besides the analysis of uncertainties described in the earlier points, we added a study of the variability of vorticity within the search region for each product (new Fig. 2). This allowed us to detect an increased dispersion at higher cyclone intensities in the merged product, which is larger than the signal found in the single sensors. We attribute this to the gap-filling procedure, and we have briefly discussed it in Sec. 5. There is indeed an impact, but currently, in the absence of independent observations that would corroborate our findings, we are unable to fully identify whether this is an artifact or a feature. The revised discussion comments on this further.

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

This has been corrected.

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

This has been corrected. The sentence now reads "… detection and quantification of…".

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

This has been corrected. The sentence now reads "... which undergo shared processing chains…".

Page 3, line 85: "range" → "family"

This has been corrected. The sentence now reads "... SSMI/S product 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"

This has been corrected. The sentence now reads "... implements a two-step process.".

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

This has been corrected. The sentence now reads "... and Δx and Δy is the length and width of a pixel, which in our case are both 62.5 km".

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

Product labels have been added to all figures to aid the inter-product comparisons. The revised figures now combine the cyclonic and anticyclonic results, resulting in an 8-panel figure, where each row separates products and each column separates cyclonic and anticyclonic features. This has been done to the newly added uncertainty (Fig. 1) and variability (Fig. 2) components, and the revised interannual variability analysis (Fig. 4)

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

Same as mentioned previously.

Response to Reviewer 2

Dear Dr Valentin Ludwig

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.

In this document we have provided an overview of the major changes we have made for our revised manuscript after acting upon the suggestions made by you and Reviewer 1. We have also responded to each of your specific comments, and those of Reviewer 1, in a point-by-point format. Your original comment appears in *italics*, while our response appears in **bold**.

Responses to your specific comments:

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.

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.

As mentioned in the above overview, a fundamental component of our revised version is the addition of an uncertainty and spatial variability analysis (Sec. 4.1). The results of these components allow us to conclude that this mismatch is between products is not a consequence of the uncertainty of the

drift retrieval. We can also conclude that the spatial variability of vorticity estimates is directly proportional to the intensification of cyclonic rotation. This suggests that in regions of intense cyclonic ice drift, either:

1. **sub-synoptic scale ice dynamics result in a patchy, variable vorticity field at the synoptic scale, and these are the dynamics being captured by the ice drift products. Or**
2. **the feature-tracking method of drift retrieval performs poorly under conditions of rapid dynamic and thermodynamic changes in ice properties which is a common phenomenon in ice under polar storms.**

Our revised version shows that the variability increases more relative to the feature's cyclonic intensity for the merged product than any other product (Sec. 4.1), suggesting that the higher variability detected in the merged product is linked to a larger intensity estimate. Our analysis, however, does not allow us to discern whether the merging process results in the addition cyclonic energy in the sea-ice field, or whether the better-quality coverage of the merged product means we can more accurately extract the true variability and intensity of these features. Without a full validation of the vorticity metric – which is challenging with so such few *in situ* drift measurements – we can only hypothesize which product is performing better. We have clarified in our Discussion and Conclusions (Sec. 5). 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.

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".

We have removed the word "alternative" from our abstract.

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.

This comment is no longer included in the abstract, but we have rephased the description of the 'processing chain' to "merging process" in the Discussion and Conclusions (Sec. 5).

L18/19: I suggest to add that only cyclonic momentum is added.

In our revised abstract, we have removed the comment suggesting that momentum is added in the merging process. Instead, we have reported that that the cyclonic features detected by the merged product are of a higher intensity and spatial variability. In our discussion (Sec. 5), we have clarified

that although we can report on the differences between products, we cannot imply which product is more accurate without a validation of the vorticity metric.

3.2 Introduction

General: Should mention that, unlike in the Arctic, SIE in the Antarctic was quite constant until recently

In our revised Introduction (Sec. 1), we have added that "Antarctic SIE trends have historically been relatively constant, however, are recently characterized by pronounced variability…"

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.

We have added to our revised Introduction (Sec. 1) that "…*this phenomenon is likely to grow in influence as extratropical cyclones shift poleward and polar storms intensify (Chang, 2017; Tamarin-Brodsky and Kaspi, 2017)*"

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

Simplified to "sparse" only in Sec. 1.

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"

Removed and simplified to "…used to estimate the SIE…"

L38: Why is the variability dramatic? I would suggest something more objective and less drastic like "high" or "pronounced"

Rephased to "pronounced variability"

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. . . ".

Added comment to our Introduction (Sec. 1): "Modelled attempts to simulate the sea-ice extent have shown that uncertainties in this internal variability introduces a far greater bias than that of the satellite retrieval process (Notz, 2014)". Sentence has been split as suggested.

L68: Please provide a reference for your statement that the Southern ocean hosts some of the most energetic storms worldwide.

Comment removed.

L69: MIZ has not been defined yet.

Definition of MIZ added into the Introduction (Sec. 1) before the acronym is used.

L71: Much has changed since 2003/2004, please provide more up-to-date references. Also, "it therefore" should be "it is therefore".

Updated references to (Chang, 2017; Tamarin-Brodsky and Kaspi, 2017) in Sec. 1.

L74: Instead of "daily timescales", you could be more specific and speak of "two-daily resolution"

Comment rephrased to "48 h" in the Introduction (Sec. 1) and Discussion (Sec. 5).

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.

We have clarified our methodology in Sec. 3 that "the algorithm generates virtual circular subdomains D_r of radius r centred at every grid point in our vorticity field. Each of these subdomains represent a vorticity feature, which can overlap in space or in time, but never in both. We define the feature intensity as the mean vorticity of all values contained within its circumference, and the feature variability as the standard deviation of all vorticity values contained within its circumference. Therefore, a negative intensity feature represents a circular area of sea ice dominated by cyclonic rotation, while a positive intensity feature represents an area dominated by anticyclonic rotation. A minimum pixel validity threshold of T is applied to every subdomain D_r, ensuring that each classified feature has an adequate number of valid vorticity values within its circumference.".

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

This comment has been removed from the revised Introduction, but we have rephrased "procession chain" to "merging process" in the revised Discussion and Conclusions (Sec. 5).

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.

We have rephrased the final paragraph of the introduction (Sec. 1) to better align with the results determined in our revised version. The differences we observed in the first submission have partly reduced when considering only the high-quality data, as suggested by Reviewer 1. As explained in

the preamble, we also performed a more quantitative analysis of the uncertainties, which strengthened our results and allowed to perform a comparison of the products over the years. The revised results show that the uncertainty of the vorticity metric is negligible when using a coarse resolution drift product, and that there exists a discrepancy between the cyclonic drift detected by the various products. The products however agree in detecting an increase in the intensity of cyclonic rotation in sea ice since 2014-15 (excluding AMSR2, which is only available after this period). We focus on presenting our methodology as a useful technique for future climate index studies, and comment on the necessity to conduct a validation of the vorticity metric before we confidently assess the discrepancies between products.

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.

Our data availability section has been updated with a link to an FTP server in which a user can access all the EUMETSAT OSI-SAF Low Resolution Drift products used in our revised version.

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

For this analysis, motion vectors derived from the SSMI/S instrument family are grouped to provide a continuous dataset of measurements since 2013. This group will be analyzed as one product, termed the SSMI/S product. This has been clarified in the revised Data section (Sec. 2) as "… motion vectors derived from the SSMI/S instrument family are grouped to provide a continuous dataset of measurements since 2013. This group will be analyzed as a single product and referred to as the SSMI/S product."

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 (Lavergne et al., 2010). This has been clarified to "…the weighting of each single-sensor product is inversely proportional to the validated error of that product" in the revised Data section (Sec. 2).

L97: I think "coarse" would be more appropriate than "large" when speaking of resolution.

Rephrased from "large" to "coarse".

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). 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 have added this consideration in Sec. 5 as follows: "…our detection algorithm identifies circular ice drift features with a radius of 450 km ± 50 km – which is about 6-7 times the spatial resolution of the products and of the scale of atmospheric weather – and quantifies the characteristics of the vorticity field within this circumference."

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

Antarctic OSI-405-c motion vectors are mapped onto a NSIDC polar stereographic projection (latitude of true scale at 70°S). This has been added in the Data section of the revised manuscript.

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.

In the revised version, we hyphenate the term when speaking of related properties, and not when using it as a noun. This has been done throughout the manuscript.

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.

In our revised version, we have included the spatial and temporal ranges in the Data section (Sec. 2) as follows: "Due to the limitations of measuring sea-ice drift in melting conditions and during periods of insufficient ice cover, only the months of June-October were considered, and our analysis focused on the Atlantic Sector of the Southern Ocean, spanning the area between 65° W and 50° E.". The initial manuscript showed results between 1st April – 31st November. The revised manuscript shows results between 1st June – 31st October. This change was made because the freezing months had a larger area of ice coverage with fewer rejection quality flags. The initial manuscript showed results between 65° W and 10° E. The revised manuscript shows results between 65° W and 50° E. This change was made to reduce the influence of the region boundary 10° E, an area where sea-ice coverage band is wide. The sea-ice coverage at 50° E is thin relative to the scale of features being detected, and so the algorithm is not affected by an artificial boundary parameter. The total area of a feature is approximately 6.36×10^5 –5.72×10^5 km^2 depending on the choice of free parameters

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 improved our methods section explaining how features are identified and categorized. This now reads as:

"…the algorithm generates virtual circular subdomains D_r of radius r centred at every grid point in our vorticity field. Each of these subdomains represent a vorticity feature, which can overlap in space or in time, but never in both. We define the feature intensity as the mean vorticity of all values contained within its circumference, and the feature variability as the standard deviation of all vorticity values contained within its circumference. Therefore, a negative intensity feature represents a circular area of sea ice dominated by cyclonic rotation, while a positive intensity feature represents an area dominated by anticyclonic rotation. A minimum pixel validity threshold of T is applied to every subdomain D_r, ensuring that each classified feature has an adequate number of valid vorticity values within its circumference. Subdomains that fail to meet the minimum pixel validity threshold are ignored, thus reducing the algorithm's susceptibility to classifying small regions of intense vorticity at the ice edge or coastline as features".

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 have only used 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 allowed us to quantify the confidence of our vorticity measurements. An analysis was done on 'significant' features (i.e., the 95th percentile) as suggested by yourself and Reviewer 1.

3.5 Results

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

We now include the number of features detected in our revised Results section (Sec. 4.1). "A total of 311 758 anticyclonic features were detected over this period, with the most being returned by the merged product (144 491), followed by the AMSR-2 (84 171), SSMI/S (64 263) and ASCAT (18 383) products. The number of cyclonic features returned was similar in the single-sensor AMSR-2 (105 889), SSMI/S (66 111) and ASCAT (21 991) products, while the merged product detected approximately twice as many cyclonic features (295 484) as anticyclonic features. A total of 489 475 cyclonic features were returned by all four products, 57 % more relative to the number of anticyclonic features detected."

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.

We have now made a single figure of 8 panels (4 rows; 2 columns) for Figure 1, 2 and 4 of our revised version, where each product results can be compared between rows, and cyclonic and anticyclonic results can be compared between columns.

L124-126: This technical description of the box-and-whisker plot could be moved to the caption of the Figure.

Figures 1 and 2 of the initial manuscript showed that interannual distribution of all cyclonic and anticyclonic features respectively. Following yours and Reviewer 1's suggestion to consider 'significant' features, Figure 4 in Results (Sec. 4.2) now shows the interannual distribution of the 95th percentile of features. Both cyclonic and anticyclonic features are shown on the same figure to aid the visual comparison, as this was another suggestion made by yourself. We have now moved the technical description of the box-and-whisker plot to the caption of Figure 4. Figures 1 and 2 now show the results of the uncertainty and variability analysis respectively, which are two components added to the revised manuscript as mentioned in the preamble.

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?

As mentioned above Figures 1 and 2 of the initial manuscript showed that interannual distribution of all cyclonic and anticyclonic features respectively, and following yours and Reviewer 1's suggestion to consider 'significant' features, Figure 4 in Results (Sec. 4.2) now shows the interannual distribution of the 95th percentile of features. Both cyclonic and anticyclonic features are shown on the same figure to aid the visual comparison. This means that Figure 4 has 8 panels (4 rows; 2 columns), where each product results can be compared between rows, and cyclonic and anticyclonic results can be compared between columns. Furthermore, we have now added Table 2 in the revised Results (Sec. 4.1) which shows the statistical mean and standard deviation of the data spread to supplement the communication the interannual variability analysis shown in the revised Figure 4.

L132: 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.

In our revised Results section (Sec. 4.2), we have quantified the interannual intensity distribution by the mean and standard deviation of the 95th percentile of cyclonic and anticyclonic features, as these two metrics indicate both the magnitude of intensity and the 'spread' of the distribution. These metrics have been reported in the text (Sec. 4.1) and in Table 2.

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.

We have modified our interannual variability component of our analysis in our revised version. Following suggestions made by yourself and Reviewer 1, we now present the interannual variability of the 95th percentile of features in Section 4.2. This distribution is illustrated in the new Figure 4. Furthermore, we have quantified the distribution as the mean ± standard deviation of the 95th percentile of features. These values have also been reported in Table 2, which was another addition to the revised manuscript following your suggestion.

L141: "which being detected": Should this be "which was detected"?

This comment has been corrected as suggested.

L144: Should mention the reduced y-axis range of Fig. 2 compared to Fig. 1.

As mentioned above Figures 1 and 2 of the initial manuscript showed that interannual distribution of all cyclonic and anticyclonic features respectively, and following yours and Reviewer 1's suggestion to consider 'significant' features, Figure 4 in Results (Sec. 4.2) now shows the interannual distribution of the 95th percentile of features. Both cyclonic and anticyclonic features are shown on the same figure to aid the visual comparison. We have also scaled the y-axis to the same range on all subplots in Fig. 4 to aid comparison.

L170-176: Please put the IQR and σ values in a Table, this would be much easier to grasp and would improve the readability.

We have included Table 2 in our revised Results (Sec. 4.2), which includes the mean, standard deviation and 95th percentile threshold for each year to supplement the intensity distribution illustrated in the new Figure 4. The mean ± standard deviation of the uncertainty and spatial variability analysis components in Section 4.1 have also been reported in a table format (Table 1).

3.6 Discussion and Conclusions

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

In Section 4.2 of our revised Results, we consider the mean ± standard deviation of the 95th percentile of features. The interannual distribution of these features is illustrated with box-and-whisker plots in revised Figure 4, while the mean, standard deviation and 95th percentile thresholds are reported in Table 2. We have clarified our comments that "there is no obvious interannual trend shown in anticyclonic intensity between 2013–2020 (Fig. 4a, c, e and g), visually indicated by the box-and-whisker rectangles remaining relatively constant in all four products and with few outlier features detected. This was further supported by the relatively small variability in the mean intensity and standard deviation between years (Table 2). Conversely, a clear cyclonic interannual trend was evident, characterized by relatively low-intensity features in 2013 and 2014, followed by an abrupt increase in intensity from 2015–2017 (Fig. 4b, d, f and h). From 2014–2017, the mean of the most intense cyclones increased by a factor of 2.8, 2.6 and 1.7 for the ASCAT, SSMI/S and merged products respectively (Table 2). Much like the variability-intensity cyclonic trends described earlier (Fig. 2b,

2d, 2f and 2h), it was again evident that the standard deviation increases with the intensity from 2014–2017 for each available product (Table 2).”

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

In our revised version, we have made two changes which improve the robustness of our analysis. Firstly, we have incorporated an uncertainty analysis into our analysis as suggested by Reviewer 1 (Sec. 3 and Sec 4.1). We theoretically propagated the data error onto the vorticity estimates according to Dierking et al. (2020). This gives us confidence that the discussed trends are not caused by noise in the drift retrieval estimation. Secondly, we have modified our trend analysis to consider the 95th percentile of each year (please see more details in the preamble). This resulted in a new section 4.2 added to the revised version. This has allowed us to see a clear change in the distribution of cyclonic drift, particular from 2014-2017. 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. There is, however, evidence that an increase in the influence of polar storms between 2014-2017 primarily drove the rapid decrease in SIE over the same time period (Wang et al., 2019), which coincide with the increase cyclonic drift reported in our revised version. These considerations have been added in the revised Discussion in section 5.

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 explained in the preamble, we have expanded our analysis beyond a brief communication at the suggestion of the handling editor. For this reason, we include a more in-depth analysis of the product discrepancies, separating our revised Result section 4 into two subsections. Sec 4.1 is dedicated to the analysis of uncertainties and the comparison between products, which is an improved version of our earlier brief communication. Here we rephrase our results as not to imply any product is more correct than another, as this would require a full validation of the vorticity metric to confirm. Instead, we highlight likely – or less likely - drivers of this discrepancy. Firstly, we show that the uncertainty is unlikely to cause the difference between products. Secondly, we report a much higher variability in the spatial vorticity field when detecting cyclonic features compared to that of anticyclonic features. We also show that this variability increases with higher intensity features, and that the merged product detected the highest intensity and most variable features. We offer an interpretation of these results in the revised Section 5: “Our analysis, however, does not allow us to discern whether the merging process causes an amplification of cyclonic drift in the sea-ice field, or whether the better coverage of the merged product means it can more accurately detect the rotational drift in the ice compared to the single-sensor products. We hypothesize that the large variability introduced by the merging process is causing an artificial intensification of cyclonic rotation. However, in the absence of independent observation that would corroborate our findings, we are unable to fully identify whether this is an artefact or a feature”.

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

This comment referred to a spatial analysis done using the most intense features detected by each of the four products (not shown in the original manuscript), which has not been included or commented on in the revised version. This was expected due to the patterns in coverage of the ASCAT product. More specifically, the ASCAT product usually has the lowest coverage our of any of the products, and the Eastern Weddell Sea is the region in which the ASCAT consistently had valid drift estimates, compared to the poorer coverage closer to the MIZ. Furthermore, the vorticity field in the Eastern Weddell Sea was typically of a lower intensity than that closer to the MIZ. We decided to redefine our period of analysis to between 1st June – 31st October (previously was 1st April – 30th November) to improve the quality and coverage of single-sensor drift estimates used in this study. This was decided because the freezing months have a better coverage of drift estimates. Furthermore, **the initial manuscript showed results between 65° W and 10° E, but the revised manuscript shows results between 65° W and 50° E. This change was made to reduce the influence of the region boundary 10° E, an area where sea-ice coverage band is wide. The sea-ice coverage at 50° E is thin relative to the scale of features, and so no features are detected here because of the insufficient ice cover, and not by an artificial boundary parameter. This increased the available sea-ice area for all products on which the algorithm could identify rotational features**

L209: See my comment to your L74. Please describe the method briefly, since it was the main motivation for your work.

We have clarified our methodology in Section 3 that "the algorithm generates virtual circular subdomains D_r of radius r centred at every grid point in our vorticity field. Each of these subdomains represent a vorticity feature, which can overlap in space or in time, but never in both. We define the feature intensity as the mean vorticity of all values contained within its circumference, and the feature variability as the standard deviation of all vorticity values contained within its circumference. Therefore, a negative intensity feature represents a circular area of sea ice dominated by cyclonic rotation, while a positive intensity feature represents an area dominated by anticyclonic rotation. A minimum pixel validity threshold of T is applied to every subdomain D_r, ensuring that each classified feature has an adequate number of valid vorticity values within its circumference.".

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

In the revised Discussion section (Sec. 5), we have proposed a potential reason for the product disagreement as suggest by yourself, while also not implying that any product is more accurate than another until a validation of the vorticity metric is done, as this was a concern raised by Reviewer 1. We comment: "Our analysis, however, does not allow us to discern whether the merging process causes an amplification of cyclonic drift in the sea-ice field, or whether the better coverage of the merged product means it can more accurately detect the rotational drift in the ice compared to the single-sensor products. We hypothesize that the large variability introduced by the merging process is causing an artificial intensification of cyclonic rotation. However, in the absence of independent

observation that would corroborate our findings, we are unable to fully identify whether this is an artefact or a feature. Furthermore, the feature-tracking method of drift retrieval may be susceptible to error under conditions of rapid dynamic and thermodynamic changes in sea-ice properties, such as in the event of a strong cyclone traversing the sea ice. It is therefore necessary to consider that rapidly moving ice floes may be blurring the rotational drift we are attempting to estimate over a 48 h period".

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