TC-2021-144: Response to reviewers

Overview of revised manuscript

Following comments received from the reviewers, we have decided to add a further subsection to our revised Results section, which now includes two subsubsections each with a new figure. Reviewer 2 requested to add some examples of the vorticity fields that are used by the detection algorithm. We have therefore provided two case studies: Case Study 1 (Sect. 4.1.1) which considers the effect of an atmospheric cyclone on the underlying sea ice dynamics (illustrated by the new Fig. 1), and Case Study 2 (Sect. 4.1.2) which considers the effect of a persistent high-pressure cell on the underlying sea ice dynamics (illustrated by the new Fig. 2). Both conditions were observed during expeditions of the SA Agulhas 2 in the region. This addition also allowed us to expand on a few considerations regarding the comparison between the products and their relative quality. The previous Sec. 4.1 is now Sec. 4.2, which also contains references to the case studies to highlight how they compare with the bulk of the rotational features detected by the algorithm. The contents of the previous Sect. 4.1 and Sect. 4.2 are now found in Sect. 4.2 and Sect. 4.3 respectively, along with their corresponding figures, now labelled Fig. 3 and 4.

The other major change is made following comments by Reviewer 1, who suggested we convert the scatterplots of the previous Fig 1. and Fig. 2 into 2-D histograms. We agree with Reviewer 1 that this would improve the readability of our figures, and these modified figures are now found in the revised Results Sect. 4.2 labeled as Fig. 3 and Fig. 4 respectively. The 2-D histograms now show the clustering of data points around the regression line more clearly, which aids the visualization of the discrepancy in the number of features detected by each product with the colormap scheme. The values of the slope, intercept, standard error of the slope and the number of features detected are now also shown on the figure panels, as suggested by both Reviewer 1 and 2.

Response to Reviewer 1

Dear Dr Valentin Ludwig

Once again, 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 after a second round of review. 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**.

1. Content

The authors de Jager and Vichi re-submitted a paper which analyses the differences between different satellite-based sea-ice drift products from EUMETSAT OSI-SAF (product OSI-SAF 405 c), namely five single-sensor-based products (AMSR2, ASCAT, 3x SSMIx) and a merged product based on the formerly mentioned ones. Much content has been added to expand the manuscript from the previously submitted "Brief communication" to a full-fledged research article. The major differences are the inclusion of an uncertainty estimate and analysis, the discussion of spatial variability and the restriction of the analysis to high-quality data as per the quality flags of the drift product.

2. General Comments

I appreciate the effort which the authors made to submit this expanded version. I fully agree with their decision to extend the manuscript to a full research article, following the handling editor's recommendation. I enjoyed reading this manuscript and found my concerns from the first round of review addressed satisfactorily. However, I still have some comments which I would like to see addressed before publication. Line numbers in my specific comments correspond to the track-change version of the revised manuscript.

3. Specific Comments

3.1 Abstract

L14: " . . . Antarctic ice characteristics.": You might consider removing "Antarctic", since the need for methods to quantify sea-ice changes is also there in the Arctic.

The adjective "Antarctic" has been removed, and the sentence now reads: "…develop methods to quantify changes in sea-ice dynamics that would indicate trends in the ice characteristics."

3.2 Introduction

General: I find the Introduction quite long. The part after L72 was interesting and well written, but the part before could for my taste be condensed. I suggest to build your 1 motivation on the increased storminess and the recent changes in Antarctic sea ice, but not so much on having an additional measure for assessing climatological trends as a complement to SIE.

We thank the reviewer for their comment and agree that our motivation and introduction text could be improved to better introduce the reader to the necessary concepts, but without detracting from the main points being presented. We have therefore removed some unnecessary text from the introduction – and rearranged other portions – to streamline the ideas into a more coherent flow. As your comment suggests, we build our motivation around the increased storminess and variability since 2014, which then instinctively proposes the need to detect and quantify the effect of atmospheric weather on sea ice and apply those considerations onto the existing datasets. We feel that the revised introduction better prepares the reader to understand the usefulness of our results, but without deviating too much from the main messages communicated in the Discussion.

L44-45: A reference would be good, for example IPCC AR6.

The IPCC AR6 2021 report has been referenced in text as suggested. The sentence now reads: "…presented to highlight the effects of global warming (Masson-Delmotte et al., 2021)." The corresponding citation has also been added to the references section:

Masson-Delmotte, V., Zhai, P., Pirani, A., Connors, S. L., Péan, C., Berger, S., Caud, N., Chen, Y., Goldfarb, L., Gomis, M. I., Huang, M., Leitzell, K., Lonnoy, E., Matthews, J. B. R., Maycock, T. K., Waterfield, T., Yelekçi, O., Yu, R. and B., Z.: Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change., 2021.

L72: You start your sentence with "Studies. . . ", but then refer to only one. I suggest to either rephrase or add more references.

We have now added a second reference (Kwok et al., 2017) with the corresponding citation in the references section:

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.

L77: "El Nino" → "El Niño"

The eñe character has been corrected in the revised version.

L85: Replace "ice floes" by "ice pack"

The word "floes" has been replaced with "pack", and the sentence now reads: "…as strong winds induce synoptic scale rotation into the ice pack while carrying…"

L96: I suggest to use present tense throughout the paper whenever possible, especially when describing your own work.

In our revised version we have now used present tense throughout.

L102: Please mention that all products are from OSI-SAF.

The sentence now reads: "…by computing the sea-ice vorticity using satellite ice drift estimates from EUMETSAT OSI SAF and quantifying the rotational drift of the sea ice within circular domains."

L105: "assess" → "to assess"

This sentence has been corrected and now reads: "…and to assess to what extent ice drift product…"

L106: Please specify the period by saying which years you refer to.

The specific temporal range has been included and the sentence now reads: "…to capture changes over the period of data availability (2013-2020)."

3.3 <u>Data</u>

Generally: Please spell out the acronyms when you use them for the first time.

In the revised version, we have expanded the following acronyms in sections:

1 Introduction:

> **"EUMETSAT" now reads "European Organization for the Exploitation of Meteorological Satellites (EUMETSAT)…"**
>
> **"OSI SAF" now reads "Ocean and Sea Ice Satellite Application Facility (OSI SAF) …"**

2 Data:

> **"AMSR-2" now reads "…Advanced Microwave Scanning Radiometer 2 (AMSR-2) …"**
>
> **"ASCAT" now reads "…Advanced Scatterometer (ASCAT) …"**

“SSM/I” now reads “…Special Sensor Microwave Imager (SSM/I) …”

“SSMI/S” now reads “…Special Sensor Microwave Imager/Sounder (SSMIS) …”

“JAXA” now reads “…Japan Aerospace Exploration Agency (JAXA) …”

“DMSP” now reads “…Defense Meteorological Satellite Program (DMSP) …”

L123: Is it intentional that you write "SSMIS-f17" or should it be SSMIS-F17 in the following lines)?

No, this detail was overlooked. In our revised version, we have capitalized the “F” in the DMSP platform names (Sect. 2 and Sect. 4.2).

L125:"SSMI/S" → "SSMIS"

This sentence has been rephrased and now reads “… motion vectors derived from the SSM/I and SSMIS instruments are grouped to provide a continuous dataset of measurement from 2013. This group will be analyzed as a single product and referred to as the SSMI/S product.”

L136: Please provide a reference (URL) for the NSIDC projection.

A URL reference for the polar stereographic projection has been included in the revised version. The sentence now reads “Antarctic OSI-405-c motion vectors are mapped onto a NSIDC polar stereographic projection (https://nsidc.org/data/polar-stereo/ps_grids.html) and are available …”

L139: A reference for the limitations would be nice

Two citations have been added, namely Lavergne (2016) and Sumata et al. (2015), with the following text added to the reference section:

Lavergne, T.: Algorithm Theoretical Basis Document (ATBD) for the OSI SAF Low Resolution Ocean & Sea Ice SAF Algorithm Theoretical Basis Document for the OSI SAF Low Resolution Sea Ice Drift Product GBL LR SID — OSI-405-c, , (July), doi:10.13140/RG.2.1.3082.3920, 2016.

Sumata, H., Kwok, R., Gerdes, R., Kauker, F. and Karcher, M.: Uncertainty of Arctic summer ice drift assessed by high-resolution SAR data, J. Geophys. Res. Ocean., 120(8), 5285–5301, doi:10.1002/2015JC010810, 2015.

3.4 Methodology

L145: "SSMI/S" → "SSMIS". Please also check for other occurrences in the paper.

Here the acronym "SSMI/S" is referring to the name we have termed the grouped ice drift datasets derived from the both the SSM/I and SSMIS instruments. It is not specifically referring to either the SSM/I or SSMIS products. In Sect. 2, we mention that "this group will be analyzed as a single product and referred to as the SSMI/S product."

L152: "Acceptable" sounds quite subjective. Readers who are not familiar with the meaning of the single flags (such as myself) might wonder why you choose flag 20 as criterion. Please give very briefly some information about the meaning of the flag numbers, and your reason to choose flag 20 as threshold.

In the revised Methodology section (Sect. 3), we have included an explanation of the product's flag values and clarified the reasons for our choice. The following text has been added: "Low-quality flagged drift estimates (i.e., flag values 0-19 in the OSI-405-c product) were rejected, while only those flagged with a good quality index (i.e., flag values 20-30) were considered. Rejected and good quality index flags are determined by the quality of the drift estimate retrieval and are made available in the OSI-405-c product. Nominal quality estimates (i.e., flag value of 30) have the lowest retrieval uncertainty and were measured independently of its neighbours, while flag values 20-29 included drift estimates that required a correction or interpolation scheme from neighbouring locations and therefore have a larger uncertainty. Rejection quality flags correspond to locations where no valid drift estimate could be made, and therefore no vorticity values are computed at those grid points or their adjacent neighbours (more information on the rejection and quality index flags can be found in Sect. 4.4 of the EUMETSAT OSI SAF Low Resolution Sea Ice Drift User Manual found at: https://osisaf-hl.met.no/sites/osisaf-hl/files/user_manuals/osisaf_cdop2_ss2_pum_sea-ice-drift-lr_v1p8.pdf). The 20-30 range of flag values was chosen for this analysis because each drift estimate has a corresponding drift uncertainty (as from 1st June 2017), and so while some non-nominal drift vectors may be of degraded quality, this potential noise was quantified and propagated through the vorticity computation."

L159-161: I wondered if there may be cases where there is one cyclonic and one anticyclonic feature in Dr. If yes, taking their mean would erroneously return comparably small rotational activity. Can this happen? Please elaborate on this in the Discussion.

Yes, you are correct that it is possible for one single feature (Dr) to partially capture the vorticity of both a cyclonic and anticyclonic feature. In these cases, the resultant mean vorticity would be relatively low, and therefore represented in the low-intensity portions of Fig. 3, 4 and 5. However, it is unlikely that two vorticity features at the scale of atmospheric weather would be so close to each other that the opposing rotation of both features are captured in a single 450 km radius. These cases are therefore rare and make up a very small number of the total ≈ 800,000 features detected from 2017-2020 (or 2016-2020 in the case of Fig. 5). We have included the following text in the revised Discussion (Sect. 5) to communicate this point to the reader: "It is also necessary to consider that the search radius of the feature detection algorithm described in Sect. 3 can be affected by contiguous cyclonic and anticyclonic features in the sea ice. Such condition would

have a neutralizing effect on the value of its mean vorticity, and so these kinds of features should be represented in the low-intensity portions of Fig. 3, 4 and 5. Assuming a random distribution of these features, we expect them to be highly heterogenous; however, only the variability of anticyclonic features detected by the merged product show higher heterogeneity in the low-intensity features (Fig. 4g). None of the products report low-intensity cyclonic features with high variability (Fig 4b, 4d, 4f, and 4h). We thus conclude that close, dipole-like features in the vorticity field are relatively uncommon, or that they are spatially more extended, and it is thus unlikely that two opposing rotation features are equally captured in the same 450 km search radius."

L168: 1) "resolution" should be "grid spacing". Also, I suggest to simply write ". . . a grid spacing of 62,5 km is fine enough to capture these features.", the current formulation sounds a bit odd.

The sentence has been rephrased and now reads: "…and therefore the 62.5 km grid spacing of the OSI-405-c product is fine enough to capture these features."

L172: Dierking et al. (2020) is missing in your reference list.

The following missing reference has now been added to the revised reference list:

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, Cryosphere, doi:10.5194/tc-14-2999-2020.

Equation 2: ΔT is probably constant, right? Does this mean that σ_{vort} scales linearly with σ_{tr}? Please mention this briefly.

No, ΔT is not necessarily constant. The OSI-405-c product derives drift estimates from two overlapping satellite swath measurements at t_0 and t_1. These swaths, however, are not exactly 48 h apart, but instead can range from 40-56 h apart. In the case of the single-sensor drift estimates, this means that ΔT = 48 h ± 8 h in the Antarctic (± 5 h in the Arctic) depending on the timing of the overlapping swaths. In the case of the merged product, the product combines at least three single-sensor measurements into a single estimate, and so t_0 and t_1 are ambiguous. Measurements at t_0 and t_1 are therefore artificially set to midday (12h00 UTC) exactly 48 h apart. In the revised Methodology section (Sect. 3), we have added the following sentences: "In this case, L remains constant at 62.5 km. For the single-sensor products, ΔT varies within a 48 h ± 8 h range depending on the timing of the two overlapping satellite swaths, while ΔT for the merged product is artificially set to 48 h."

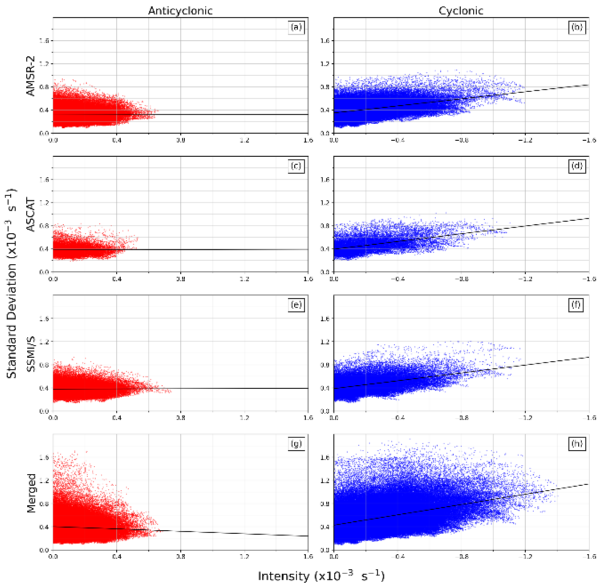
General: How is the merged product's uncertainty calculated? As a Gaussian error propagation based on the other three product's uncertainties? Please describe this.

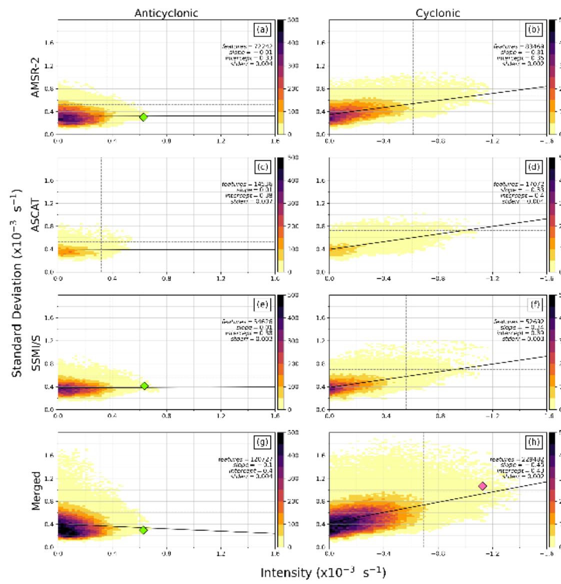
Yes, you are correct that the merged product's uncertainty values are computed with a Gaussian error propagation function based on the uncertainties of the single-sensor uncertainty values available at that grid point. We have clarified this in the results, which now reads: "This result is intuitive as the merged product is created using a combination of the single-sensor products and its uncertainty is computed using a Gaussian error propagation function based on the uncertainties of its constituents, and thus the resultant uncertainty of the merged product is expectedly more variable than that of the single-sensor products (Lavergne, 2016)."

3.5 Results

Figs 1&2: These are important Figures, but it would be really helpful to use a density plot or 2d histogram instead of a simple scatter plot. With this amount of points, it is visually impossible to see the actual distribution of values. This would also help to assess the reliability and representativeness of the trend lines. Can you provide some estimation of the robustness of the fit lines, for example a standard error of the linear regression? Also, it would be good to add the number of points, the slope and the intercept, either in the Figures themselves or in the caption.

We thank the reviewer on their suggestion of how to improve the readability of our figures. We have followed their advice and changed our Figures 1 and 2 in the original Results (Sect. 4.1) from scatterplots to two-dimensional histograms in the revised version. We decided to use counts instead of density to highlight the difference between the products, since the ASCAT product has much less points. Since we are interested in comparing the available products, we found that a density plot made them more homogeneous and discarded important features. Note that these figures are now labelled Fig. 3 and 4 and found in Sect. 4.2 in the revised version. These histograms required a new colormap scheme, and so the previous "Red for anticyclonic features" and "Blue for cyclonic features" scheme has been discarded. A color-bar corresponding to the new two-dimensional histogram has also been added. The slope, intercept, standard error of the slope and number of features are also now included on each panel of the revised figures. Below we show the original (left) and revised (right) variability vs intensity figure:





General: I was a bit surprised about the discrepancy between AMSR2 and SSMIS. Since they are both passive-microwave based, I would have expected them to be closer to each other than to the active-microwave based ASCAT data. Can you elaborate on this at some point?

The AMSR-2 and SSMI/S products are indeed the most similar, however, there is still a discrepancy in the vorticity fields computed by each product. This is partly caused by the quality of the drift vectors according to each product. As shown in the previous answer and in the revised Fig. 3 and 4, the SSMI/S product has less valid vorticity points than the AMSR-2 product. The case studies we added in the revised Sec. 4.1 are also meant to give some examples of these differences. The impact of rejection quality estimates is illustrated in the new Fig. 1 and 2, in which we demonstrate that the SSMI/S product has a slightly higher frequency of rejection-quality estimates compared to the AMSR-2. It is important to note that for every pixel that has a rejection quality drift estimate, no vorticity value can be computed for any of its adjacent pixels. This means that 1-4 pixels of the vorticity field are lost for every 1 pixel rejected, and so the differences between displacement fields appears to be exaggerated in the vorticity metric. We add the following comment into the revised discussion: "The better coverage of good quality-flagged drift estimates from the AMSR-2 product means that its resultant vorticity field has better coverage than the SSMI/S product, despite both being passive-microwave based, while the coverage of the active-microwave based ASCAT product is considerably worse than the other three products."

L218: What do you mean by "gradient factor"? The slope of the fit line? If yes, I would suggest to call it "slope".

We have rephrased "gradient factor" to "slope" throughout this portion of text in Results (Sect. 4.1). The text now reads: "The merged product had the largest slope of 0.44 for cyclonic features

(Fig. 2h), and the single-sensor AMSR-2 (Fig. 2b), ASCAT (Fig. 2d) and SSMI/S (Fig. 2f) products had smaller slopes of 0.31, 0.33 and 0.34 respectively. No obvious relationship between intensity and associated variability was seen for anticyclonic features, with the single-sensor AMSR-2 (Fig. 2a), ASCAT (Fig. 2c) and SSMI/S (Fig. 2e) products all showing a slope of approximately 0.01. The merged product had an inversed proportionately slope of -0.10 (Fig. 2g), indicating that intense anticyclonic features are more homogenous."

L237: "intensity" → "intensities"

The word "intensity" has been corrected to "intensities", and the sentence now reads: "Results show that the intensities of cyclonic features…".

L238-245: Are these the same features that went into Figures 1 and 2? If yes, you could consider to add the numbers of features either to Table 1 or to the Figure captions. Also, why does AMSR2 detect so much more features than SSMIS?

No, these are not the same pool of features in Fig. 1 and 2. Fig. 1 and 2 (which are now labelled Fig. 3 and 4) include all features detected from 2017-2020, as this is the period in which uncertainty values are included in the OSI-405-c drift estimates. The values mentioned in lines 238-245 (and represented in the previously labelled Fig. 3, but now labelled Fig. 5 in the revised version) are the number of features detected from 2016-2020, as this is the longest period of overlap available for all four products (no AMSR-2 derived estimates are available before September 2015).

As mentioned in a previous comment, the SSMI/S product has a slightly higher frequency of rejection-quality estimates compared to the AMSR-2. It is important to note that for every pixel that has a rejection quality drift estimate, no vorticity value can be computed for any of its adjacent pixels. This means that 1-4 pixels of the vorticity field are lost for every 1 pixel rejected based on the poor-quality drift estimate, and so the differences between vorticity fields appears enhanced in the vorticity metric. In the revised version, the slight difference in rejection quality estimates is illustrated in the new Fig. 1 and 2. We add the following comment into the revised discussion: "The better coverage of good quality-flagged drift estimates from the AMSR-2 product means that its resultant vorticity field has better coverage than the SSMI/S product, despite both being passive-microwave based, while the coverage of the active-microwave based ASCAT product is considerably worse than the other three products."

L261: ". . . than _for_ anticyclonic features."

This sentence has been corrected and now reads: "The 95th percentile intensity threshold was therefore 1.5-2.0 times larger for cyclonic features than for anticyclonic features."

L270: "into" → "of"

The word "into" has been replaced with "of", and the sentence now reads: "A further analysis of the interannual variability…".

L278: What is meant by "statistical " mean?

We acknowledge that the adjective "statistical" is redundant in this context and so it has been removed. The sentence now reads: "The mean, standard deviation and intensity threshold…"

L279: AMSR2 is in use since late 2012, it is the drift product which is available since 2015.

We have modified our phrasing in text to clarify that we are referring to the AMSR-2 drift product and not the AMSR-2 instrument. The sentence now reads: "Note that the AMSR-2 drift product is only available from September 2015…"

3.6 <u>Discussion and Conclusions</u>

L308: Please describe briefly what is new about your method.

To briefly clarify what is novel about our method, the following sentence has been included in the first paragraph of the Discussion (Sect. 5): "To our knowledge, this methodological process is the first attempt to quantify synoptic scale vorticity features in sea ice using remote sensing techniques, with the aim to establish an indicator of rotational drift in the sea ice field by which to detect current and future changes in the ice dynamics"

L311: Would be more consistent to write that you use four products.

We have corrected this, and the sentence now reads: "Four products were used in this study…"

L317f: From this formulation, it sounds like the 450 km would be 6-7 times the scale of atmospheric weather. I suggest to reshuffle the sentence between the dashes and write " which is the scale of atmospheric weather and about 6 - 7 times. . . " to be clearer.

We have rephrased this sentence as not to incorrectly imply that our choice of radius is 6-7 times larger than atmospheric weather. The sentence now reads: "For this reason, our detection algorithm identifies circular ice drift features with a radius of 450 km ± 50 km – which is the scale of atmospheric weather and about 6-7 times the spatial resolution of the products – and quantifies the characteristics of the vorticity field within its circumference."

L321ff: Can you speculate on possible physical or methodological reasons for the discrepancy between a) the single-sensor products and b) the single-sensor products and the merged product? Having an *idea about this would add much relevance to the study and help it to be even more than a description of an interesting finding.*

As mentioned earlier, the discrepancy between products is partly caused by the quality of the drift vectors varying between products. The merged product has the best coverage of good quality drift estimates, followed by the AMSR-2, SSMI/S and ASCAT products. It was also noticed that the discrepancy between products is enhanced when the flag quality restriction is relaxed (results not shown but mentioned in text (Sect. 4.2)). In the revised section, there is more focus on the differences in the coverage of good quality drift estimates between products. This includes an added explanation of the flag meanings in the Methodology (Sect. 3); an additional subsection in the Results (Case Studies 1 and 2 in Sect. 4.1.1 and 4.1.2 respectively) where two maps are included (Fig. 1 and 2); and added text in the Discussion.

Based on these additions, we have offered our interpretation of the results in the revised version as follows:

"These examples underline the different quality of the drift retrieval between products, which ultimately leads to a difference in the coverage of the computed vorticity field and in the detection of features"

and

"Our analysis of a few study cases gives some hints that the better coverage of the merged product increases the detection of rotational drift in the ice compared to the single-sensor products… The better coverage of the merged product seemingly makes it a good candidate for synoptic scale vorticity analysis. However, we speculate that the large variability introduced by the merging process may also cause an artificial intensification of cyclonic rotation. This is because the more extreme gradients between adjacent drift vectors in a heterogenous drift-field are manifesting into an exaggerated vorticity field. However, in the absence of independent observations that would corroborate our findings, we are unable to fully identify whether this is an artefact or a feature."

L341f: It is not entirely clear to me why more variability would artificially intensify cyclonic rotation. Please explain in more detail.

Please see the answer to the previous comment, in which we better explained this remark.

L343: "observation" → "observations"

This correction has been made in the revised version (Sect. 5).

L352: See comment to L279.

This sentence has been corrected and now reads: "The AMSR-2 detected the same uniformity since its derived drift product became available in September 2015."

L357-359: Are all the references needed? if not, please pick the two or three most relevant ones.

We have removed two of these references, and the sentence now reads: "Among other causes that involve atmospheric and oceanic components (Blanchard-Wrigglesworth et al., 2021; Meehl et al., 2019; Stuecker et al., 2017; Wang et al., 2019a), it has been argued that an intensification in polar storms in 2016 contributed to the anomalously quickened SIE decline from 2015…"

General: It would add much value if you could give the reader a recommendation in which cases the merged product is more appropriate and in which cases one might be better off with one of the single-sensor products.

While we are reluctant to recommend which product is best suitable for vorticity analysis due to the lack of dedicated experimental data validating the various products at the synoptic scale, we have added the text described a few comments above in the revised discussion. We suggest, based on the visual analysis of selected cases exemplified in the presented case studies 1 and 2, that the better coverage of the merged product likely makes it the best candidate. We do, however, remind the reader that it is not known if the merged product may be amplifying the cyclonic signal detected, or whether the higher cyclonic intensities detected are a good estimate of true conditions. We have included the following text in the Discussion (Sect. 5): "The better coverage of the merged product seemingly makes it a good candidate for synoptic scale vorticity analysis; however, we speculate that the large variability introduced by the merging process may also cause an artificial intensification of cyclonic rotation. This is because the more extreme gradients between adjacent drift vectors in a heterogenous drift-field are manifesting into an exaggerated vorticity field. However, in the absence of independent observations that would corroborate our findings, we are unable to fully identify whether this is an artefact or a feature."

3.7 <u>Code</u>

Thank you for including the code for the analysis. It would be good, however, if you could add a small readme file to the repository which instructs the user how to use the code. Even though the code is plain and well-written, I think that it would be of value if you could add some comments, so that potential users can use it right away.

We thank the reviewer for this recommendation. We have taken their advice and updated the online GitHub repository with a README file. The link has been updated in the revised manuscript: https://github.com/waynedejagerUCT/Rotational_Drift_From_Space.git

Response to Reviewer 2

Dear Reviewer 2

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 for this second round of review. 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**.

This paper presents an analysis of sea ice vorticity in the Atlantic sector of the Southern Ocean. In particular, it examines 4 datasets of sea ice velocity determined from feature tracking. Three of the datasets are from single sensors, while the fourth is a merged product.

The main conclusions seem to be that i) cyclonic ice drift features are more common, intense, and variable than anticyclonic features in this region; ii) the merged product contains more intense cyclonic features than the other products; iii) an increase in cyclonic features occurred after the well-known decline in ice extent in this region around 2014.

I believe these results are interesting and important and do deserve publication. However, I have a few issues with the paper as it stands. I was not shown any evidence that the metric used here is an essential climate variable (cyclonic ice drift driven by atmospheric forcing). I also feel that each of the three conclusions I have enumerated above are shown but not explained. So this left me as a reader both confused about exactly what is shown and also wondering why those features have occurred. I think my main problem stems from the fact that all of the figures shown are purely statistical, and do not invoke the physics of sea ice in this region.

This manuscript was originally meant to be a brief communication that has been expanded upon request of the handling editor and thanks to the comments of two other reviewers. We agree with the Reviewer that this expanded version does require a proper contextualization of the physical features, which we did not include adequately in the submitted version. We have realized the importance of showing maps of the vorticity features, which would clarify their scales and how they are connected to the atmospheric drivers. As mentioned in the overview, a major change to the manuscript is the addition of another Results subsection (Sect. 4.1), where two case studies are presented. The purpose of these case studies is to help invoke a better understanding of the patterns of movement in the sea-ice field. The two new figures presented here help illustrate the rotational patterns of the sea ice relative to the overlying mean sea level pressure contours, where it can be seen that the vorticity feature is consistent with the structure of the weather feature.

Regarding the enumerated conclusions mentioned, we have expanded our discussion to provide possible reasons for these conclusions:

"i) cyclonic ice drift features are more common, intense, and variable than anticyclonic features in this region"

In our revised version, we argue that this "is primarily because atmospheric cyclones are more effective in engendering rotational motion into the underlying sea ice, or whether the feature tracking method of drift retrieval is overly sensitive to the conditions under an atmospheric cyclone." Our work presents the methodology which has led to this information. The underlying mechanisms will be the subject of future work, but we suggest that an improved understanding of how well satellite drift products are able to capture vorticity in sea ice is still needed.

"ii) the merged product contains more intense cyclonic features than the other products"

The merged product detected more features (both cyclonic and anticyclonic) primarily due to its substantially better coverage than the single-sensor products. This is now presented with the help of the case studies in the new Sec. 41, where the difference between the products is made evident. Upon request from the other reviewer, we have also improved the new Fig. 3 and 4 (formerly 1 and 2) to show the different distribution of the detected feature. We have included the following text into the revised Discussion: "The better coverage of good quality-flagged drift estimates from the AMSR-2 product means that its resultant vorticity field has better coverage than the SSMI/S product, despite both being passive-microwave based, while the coverage of the active-microwave based ASCAT product is considerably worse than the other three products."

iii) an increase in cyclonic features occurred after the well-known decline in ice extent in this region around 2014."

In our original manuscript Discussion (Sect. 5), we provide a possible explanation of this result: "Among other causes that involve atmospheric and oceanic components (Blanchard-Wrigglesworth et al., 2021; Meehl et al., 2019; Stuecker et al., 2017; Wang et al., 2019a), it has been argued that an intensification in polar storms in 2016 contributed to the anomalously quickened SIE decline from 2015, as the overlying winds of these synoptic features induced changes in the sea-ice dynamics (Wang et al., 2019b). Our results show that sea ice in the Atlantic sector was more susceptible to cyclonic rotational features after 2015, which can be interpreted as a response to an increased incidence of polar cyclones. If the sea ice was thinner and more prone to free-drift motion in general, then we would expect both an increase in cyclonic and anticyclonic rotation, but instead, only an increase in the intensity of cyclonic features is detected." We have then expanded our discussion to include that it is necessary to have a dedicated observational effort to assess the qualities of the products and their use to compute vorticity: "Prior to performing further analysis of drift variability and longer-term trends linked to polar atmosphere variability, further validation of the vorticity metric with *in situ* experiments is required to better discern the differences between products. We therefore argue for the need of a concerted experiment to increase the number of observations of sea-ice drift in Antarctic sea ice to assess the quality of the products and their use to quantify rotational features. A better understanding of these features will enable us to confidently use rotational drift of sea ice as a potential derived index to detect climatic trends." We have also modified the abstract to reflect the changes in the revised text and commented on the need for a longer time series of data before any climatic trends can be assessed.

Lastly, We have removed the references to any new essential climate variable and indicated that the proposed measure of rotational drift is a potential index to detect climatic trends in Antarctic sea ice

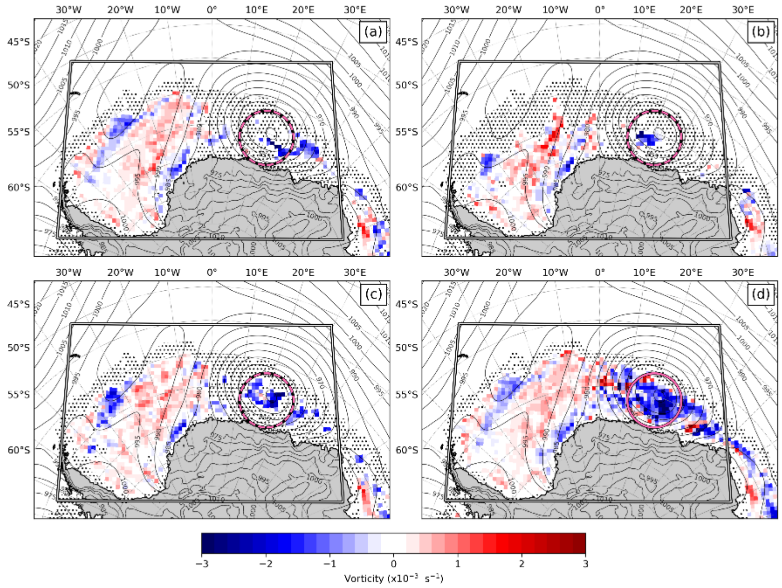
As I understand it, the key metric that is being plotted is the grid-scale vorticity averaged over overlapping circles with a 450km radius. The paper frequently refers to atmospheric cyclones/anticyclones and seems to assume throughout that all vorticity detected by this metric is associated with cyclones/anticyclones. However, I don't see that demonstrated anywhere. I can imagine many other things that would create vorticity in these data, e.g. coastal currents, ice edge currents, ocean eddies, tidal effects, artefacts in the data from missing data, interpolation or edge effects from different satellite swaths, etc. One piece of evidence that non-atmospheric-cyclone vorticity is important is that the standard deviation of vorticity within the circles is large. Does that mean there are strong sub-circle features, which means smaller scale, which means not cyclones? So, what is the basis for claiming that the vorticity captured by this metric is from atmospheric cyclones/anticyclones? I think this may be particularly important when just considering the 95th percentile. Perhaps those are the relatively few circles that have strong sub-circle features, e.g. ocean eddies or floes at the ice edge or satellite processing artefacts? I think that demonstrating where the vorticity comes from in a general sense is crucial to the conclusions, and I feel it is missing in the current manuscript.

The aim of this paper was not to systematically relate ice-drift rotation with atmospheric features, but to analyze whether existing ice-drift product are sufficiently good to allow such an analysis. Nevertheless, there is evidence in the literature of a tight relationship between synoptic weather and sea-ice movement in the Atlantic sector of the Southern Ocean. We have streamlined the introduction (as also requested by another reviewer) to better highlight our founding argument. Rotational features found in the sea ice may originate from both oceanic and atmospheric drivers, and while some initial studies may indicate that sub-mesoscale oceanic processes under the ice may be concurrent drivers (Biddle and Swart, 2020; Stössel et al., 2018), there is larger evidence of the role played by atmospheric cyclones in driving sea-ice motion. The two cases presented in the newly added Sect. 4.1 present examples of both an atmospheric cyclone and anticyclone, where the scale and location of the sea-ice vorticity features are consistent with the overlying weather conditions. These two cases are examples of other events that can be visually observed when combining the sea-ice drift and atmospheric reanalysis. These particular cases have been selected based on known synoptic events observed during the winter expeditions of the SA Agulhas 2 in the region

I think the paper needs to show some maps of vorticity. At present I don't have a good feel for what area is under consideration, or what its vorticity looks like. I haven't studied vorticity plots elsewhere in the literature and so I have no intuition here – maybe I missed something? I think the reader needs to know what the metric is, and I don't think that can be shown by starting with considering higher statistics. I think maps could be constructed such as mean vorticity, mean cyclonic-only vorticity, snapshots of interesting cases (e.g. high cyclonic vorticity), etc.

We thank the reviewer for this suggestion, which made us realize the importance of showing individual cases of the vorticity field instead of just the systematic statistical analysis. As mentioned in the answer to the first comment, we included a new subsection in the Results, now Sect. 4.1. This includes two case studies, both of which provide a map of the sea-ice vorticity field and the overlying mean sea level pressure conditions. The purpose of this subsection is to help the reader better visualize the structure of the vorticity features being detected by our algorithm described in the methodology (Sect. 3). Furthermore, the mean intensity, variability and uncertainty of these

particular features are also indicated with markers in the modified Fig. 3, 4 and 6. This allows the reader to gain some intuition of what each of these statistical figures represent in the sea-ice drift data. The newly added figure 1 is shown below as a demonstration:



Here, for the merged product in panel (d) it is illustrated that the sea-ice velocity field shows an area of negative vorticity (blue) in the same location as the high-pressure cell described in the revised Results Sect. 4.1.2. This map allows the reader to see the relationship between the ice motion and local atmospheric structure. Furthermore, these images also illustrate the discrepancy between products (where each product is a different panel), as well as the area of rejection drift vectors indicated with the dotted hatching.

Figure 1 etc. Why are the cyclonic features more intense on average than the anticyclonic features? I think the explanation here really depends upon whether they are dictated by atmospheric forcing or other features. I feel that maps are particularly needed here, to show where the cyclonic/anticyclonic features are and what they look like. If the explanation lies in the atmospheric forcing (cyclones are more intense than anticyclones) then that could be shown and discussed and literature cited.

We agree with the reviewer that this feature might be linked to the dominance of extratropical cyclones that establish the background winter weather conditions to which sea-ice dynamics is subjected. This hypothesis is however dependent on the reliability of the satellite products to detect sea-ice drift in a consistent way. In this work we aim to explain the rationale for the detection algorithm and to intercompare the different products. Despite the differences between the single sensors and merged products, there is a consistent indication that anticyclones are less intense than cyclones and there is a change in the intensity of the latter after 2015. We argue for the need of a concerted experiment to increase the number of observations of ice drift in Antarctic sea ice, but the presented results have given us some confidence on the methodology and its eventual application to the study of the underlying drivers. We however prefer to leave this component of the analysis to a further work.

Line 225: The merged product has a higher frequency of high cyclonic anomalies than the single-sensor products. Why is this? It is sensitive to the quality flag, but reducing the threshold for that flag just includes data that are suspect and so that is not a good test, in my opinion. Is it because there are more data, e.g. near the ice edge? Does mapping the vorticity fields for the different sensors help here? Mapping the data availability?

The newly added Case Studies 1 and 2 (Sect. 4.1) give a better interpretation of how influential the quality flag of the drift estimates are to the resultant vorticity field. This is illustrated with the new Fig. 1 and 2, which provide examples of both atmospheric cyclonic and anticyclonic conditions respectively. Here we show that the vorticity field of the merged product was considerably larger than that of the single-sensor products. In our discussion, we comment: "Unsurprisingly, the merged product has the best coverage in the case of cyclones and anticyclones. This is because the merged product processes drift estimates from multiple sensors and is therefore more likely to have a good quality-flagged drift estimate at each grid point. The better coverage of good quality-flagged drift estimates from the AMSR-2 product means that its resultant vorticity field has better coverage than the SSMI/S product, despite both being passive-microwave based, while the coverage of the active-microwave based ASCAT product is considerably worse than the other three products." Furthermore, it is also shown in Case Study 1 that polar storms tend to modify the ice properties rapidly, reducing the quality of the drift estimates in the affected areas. This often results in single-sensor products being unable to detect cyclonic features in this area, as they fail to meet the valid vorticity threshold based on the flag requirements describe in Sect. 3. The merged product is less susceptible to this, and higher quality drift estimates are retrieved in regions of high cyclonic vorticity.

None of the regressions plotted in figures 1 and 2 any statistical significance test. P values should be calculated and quoted on the plots, and the text should be altered wherever the trends are not significant.

In our revised version, we have modified these figures (now labelled Fig. 3 and 4) from scatterplots to 2-D histograms (as requested by another reviewer), and included the slope, intercept, standard error of the slop and the number of features on each panel. The 2-D histogram allows for better visualization of data around the line of best fit, particularly in areas of the plot where the data points have a high density. The large number of features detected (~800,000) means that the statistics shown on each panel are robust.

Line 137: I understand that for a given time, the circles overlap in space. However, I do not understand how they could overlap in time? And how you could allow them to overlap in time but not space?

This comment was meant to inform the reader that the algorithm will never extract more than one feature at the exact same location more than once per 48 h dataset. In other words, there are no duplicate features being extracted, although persistent features may be detected over multiple 48 h datasets. We do, however, acknowledge that this portion of text is unclear and unnecessary, and so it

has been rephased to “Each of these subdomains represent a vorticity feature, which can partially overlap one another in space.

Equation (2): Is sigma_tr a displacement error, in units of metres?

The σ_{tr}^2 displacement error in the OSI-405-c products are in units of kilometres. We have now included additional text in the revised Methodology section with the units of each variable in Eq. 2.

Optional: The paper does not consider seasons at all. That seems strange to me as I would imagine that the ice vorticity is very different in the summer and winter, both through the ice mechanics and the atmospheric forcing. I do not think it is essential that the authors introduce seasons to the paper, but I would do that personally as I think it would add a lot of insight.

This is unfortunately a limitation of the satellite drift products. While we do agree that a seasonal consideration would be interesting and useful, the reliability of the drift products decreases in melting conditions or during times of very low sea-ice coverage (such as very early autumn when the ice band around the continent is narrow). This would mean that a substantially higher number of drift estimates would be rejected based on the status flag criteria described in the methods (Sect. 3). This is further exacerbated by the fact that for every 1 pixel with a rejection quality drift estimate, the vorticity coverage could decrease by 1-4 pixels (due to vorticity value at each pixel requiring a drift estimate at its adjacent pixels). We have included two references (Lavergne, 2016; Sumata et al., 2015) which show that the quality of the drift estimates deteriorate outside of the winter months:

> **Lavergne, T.: Algorithm Theoretical Basis Document (ATBD) for the OSI SAF Low Resolution Ocean & Sea Ice SAF Algorithm Theoretical Basis Document for the OSI SAF Low Resolution Sea Ice Drift Product GBL LR SID — OSI-405-c, , (July), doi:10.13140/RG.2.1.3082.3920, 2016.**
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> **Sumata, H., Kwok, R., Gerdes, R., Kauker, F. and Karcher, M.: Uncertainty of Arctic summer ice drift assessed by high-resolution SAR data, J. Geophys. Res. Ocean., 120(8), 5285–5301, doi:10.1002/2015JC010810, 2015.**

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