Reviewer response

Response colour code:

- *Authors' response to reviewer*
- *Text added to revised manuscript*
- *Text from original unrevised manuscript*

All referenced line numbers refer to the revised manuscript with tracked changes.

Reviewer 1

General Comments

The manuscript addresses the quality of near surface winds in three reanalyses at four coastal stations in Antarctica. The study is motivated by the importance of the southern hemisphere polar easterlies to the climate system, especially the coupling between the easterly low level windsthat encircle Antarctica, the Antarctic coastal current and Antarctic slope front. The Self Organizing Maps -method is applied to analyze the different weather regimes and their importance to the performance of the reanalyses.

The topic of this study is of importance to the community. It increases the understanding on the sources of low-level wind speed errors in the reanalyses, and gives recommendations on the use of reanalyses. The data is appropriate: the study addresses the newest atmospheric reanalyses, and uses diverse datasets in the assessment (satellite observations and in-situ observations from meteorological stations and radiosondes). A potential data-related weakness is that the ASCAT-observations are assimilated to the reanalyses. However, a large part of the assessment is based on independent in situ observations, so the assimilation of ASCAT winds should not be a crusial problem for the overall results.

Methods are suitable for the study. A wide range of basic statistic is shown. Application of SOM gives good insight in the role of different weather regimes. The SOM method is well described, and the essential question of the number of nodes is discussed in the manuscript and in the supplement.

The overall structure of the text is clear and the quality of the plots is good. However, the manuscript could profit from a more tight focus on the relevant results. This would also allow to narrow down the number of figures, which is currently quite high. I will provide more suggestion on this in the following paragraphs.

The results support well conclusions, and the conclusion are physically sound. With the broad analysis that you have done, you could give even bolder statements in Section 4.1. I will give some suggestions related to this in the following sections.

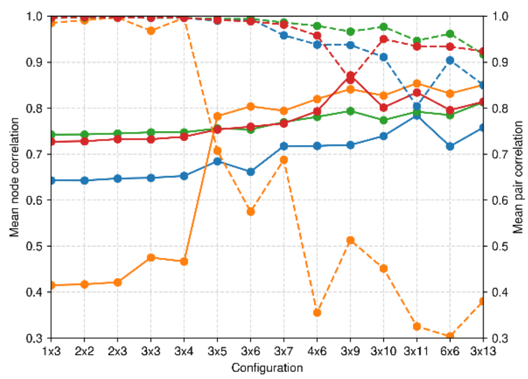
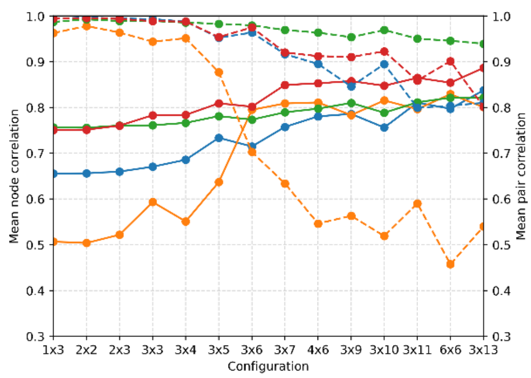
We are grateful to the reviewer for their detailed review, as well as for taking the time to make suggestions with worked examples. We agree with the main critique of needing to tighten the focus of the paper and have addressed these concerns in the responses below.

Specific Comments

SOM methods and the number of nodes

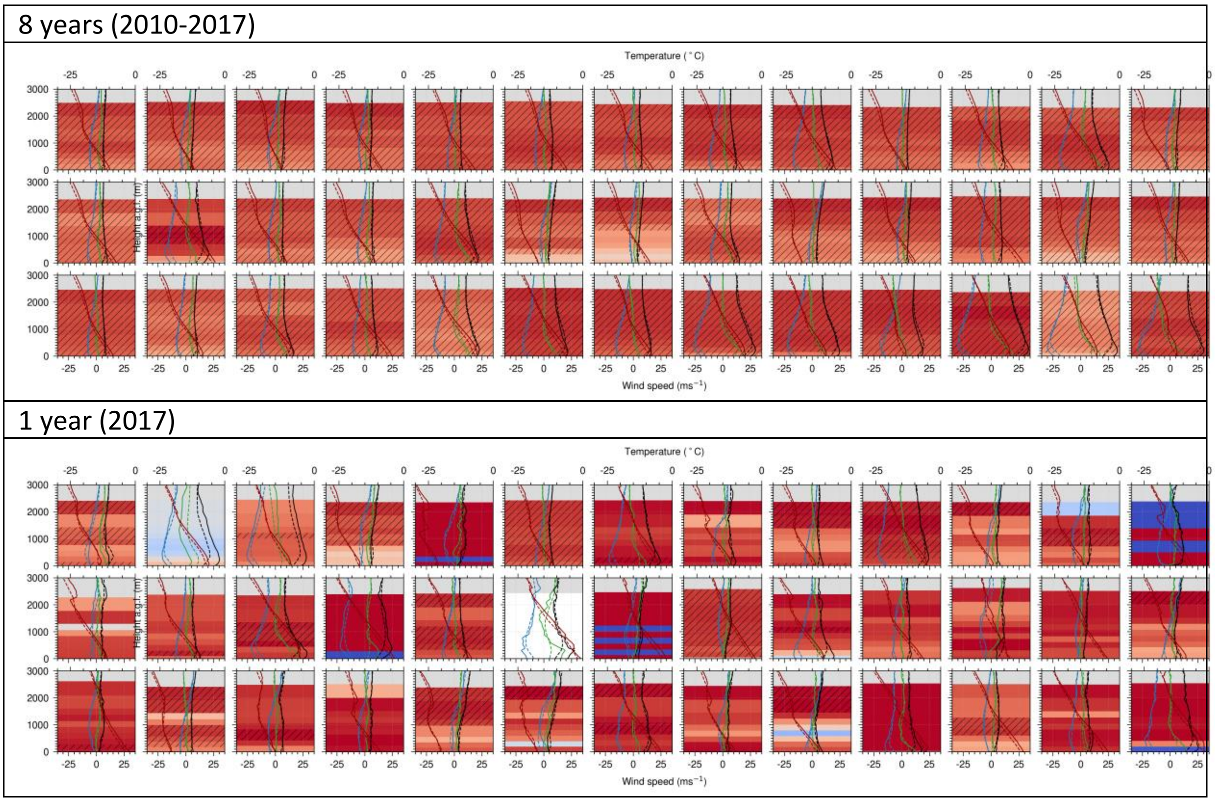
Could you comment on how the length of the timeseries affected the number of nodes that you selected? Your timeseries is not very long, so I assume that it also had an impact on the number of SOM nodes.

Thank you for raising this: we would like the SOMs for each station to be stable and representative. One test of the effect of the length of the time series we can do is to repeat the analysis of Appendix B for an even shorter time series. Appendix B tests how the realism (how well do the nodes correlate with the actual data points assigned to them?) and uniqueness (how different are the nodes from each other?) varies with different SOM configurations (1x3 up to 3x13). Below we show the results from the original analysis as shown in Figure B1, compared to the results from an analysis with only one year of data (2017):

8 years (2010-2017)	1 year (2017)
	

Casey is shown in green, DDU in red, Mawson in blue and Neumayer in orange. Node correlation is the solid lines (realism, higher is good) and pair correlation is the dashed line (similarity of nodes, lower is good). The results are very similar to what we had for 2010-2017. The node correlations are a bit higher in the one-year case for smaller SOM configurations because the SOMs are quite generalized but individual data points are more likely to fit those generalized SOMs as they're from the same year of data.

We might then also expect that with stable and realistic SOM nodes we would at least be able to get a large enough sample of matched obs/reanalysis data points that they are correlated (significantly), or more specifically that the insignificance of the correlation is not a result of the sample size being too small. For the one year case, the number of nodes really matters for this. Below is a comparison of a very large node array (3x13) for the 8 year period (above) and 1 year period (below) at Mawson. Levels where the obs vs reanalysis correlations of wind speed are significant at the 99% level are hatched:



With only one year of data, there are many nodes and levels for which correlations are not significant as there are very few matched data points (often less than 10). With 8 years of data, we could have many more nodes than the original 2x3 grid and still find significant correlations for many levels/nodes. Effectively, even with a relatively short time series of 8 years, we would have to get to a very large number of nodes before the significance of the correlations fell below the 99% level.

To select an appropriate number of SOM nodes, we consider both the realism and uniqueness we would get from our array of nodes, but we also consider the value that additional nodes add to the evaluation of reanalysis performance. Ultimately, our selection of SOM configuration was mostly driven by this, i.e. that including additional nodes would be interesting but reveal no major new 'performance regimes' in the reanalyses. As you can see from the above analysis, the size of the available time series was only a minor consideration.

We have added a comment about the role of time series length to Appendix B:

"The effect of the length of the time series upon the selection of SOM configuration was also tested. Repeating the analysis used to produce Figure B1 yielded very similar results with the time series

artificially reduced from 8 years to 1. MSLP composites were also found to be very visually similar to Figure 5, including when driven with hourly data instead of 12-hourly (not shown)."

Number of figures

There are 10 figures and 1 table in the manuscript, and 8 figures in the supplementary material. Most of the figures contain several panels making them packed with information. In my opinion, the number of figures and subplots could be reduced, without loosing the essential message of the manuscript. I suggest that the authors consider the following changes:

- Could Table 1 be removed? It is a good overview of the weather regimes and clearly useful in the analysis phase of the study. However, I don't think that it is necessary in the final manuscript as the same information comes clear through the figures (especially Fig. 5), and the text.

We agree with the reviewer that more could be done to reduce the volume of graphical information shown in the paper. On the reviewer's recommendation, we have removed Table 1 from the body of the manuscript. It is instead available in the Supplementary Material which now accompanies the paper.

- Could you select only 2-3 of the SOM nodes to be shown in figures 7-10? For example, you could select only the three most frequent nodes for each station. This would reduce the subplots from 6 to 3, and make the plots more visible. Also, this selection would allow to keep examples of a katabatic case and a strong wind case, which you discuss the most in the text. You could keep the text almost as it is, as you focus already mostly to these two flow regimes. However, these most frequent nodes don't necessarily capture the strongest cases, so some changes will be required. The most frequent nodes, ie. the nodes to keep would be (according to Fig. 5):
  - Casey, figure 7: (0,1), (1,0), and (1,2)
  - DDU, figure 8: (0,0), (0,2), and (1,1)
  - Mawson, figure 9: (0,0), (1,0), and (1,2)
  - Neumayer, figure 10: (0,0), (1,0), and (1,1)

After testing the effect that this change (reducing to the three most frequent nodes in the main text) would have upon the displayed results, we found that all the main points made in the results could still be made with the nodes displayed when selecting just the top three. Some adjustments to the text were needed to accommodate this change, however. For example, one reference to nodes at DDU has had to be updated to refer to a more frequently occurring (but quite visually similar) node. We have therefore adopted the reviewer's suggestion, but made the full set of SOMs available in the Supplementary Material. We also note in the caption of Figures 7-10 how much of the total frequency the displayed nodes account for.

Implications for the use of reanalysis datasets (section 4.1 of the manuscript)

As you do a thorough analysis of the performance of the reanalysis, you could highlight these results more in section 4.1. For example, you could provide a ranking for the reanalyses similar to what is done in Jonassen et al. 2019, JGR-Atmospheres (https://doi.org/10.1029/2019JD030897). I feel that this kind of assessment could help in drawing overall conclusions on the different performance between the reanalyses. You could do this ranking based on the supplement Figure G1. I provide below an example of the ranking for station Casey. If you like this approach, you could calculate the ranks for each station, and justify your recommendation in section 4.1 with the help of these ranking values.

Example ranking for Casey-station: Performance ranks for each node based on Fig G1. The reanalysis that performs best for the particular node gets a rank 1, the second best gets 2, and the third 3 (note: Jonassen et al have the ranks the other way round). The ranking is done for node and each metric (each row in this case) separately. The sum of the scores (for each node and reanalysis) is shown in the "total"-row.

	ERA5			MERRA-2			JRA-55		
R	1	1	1	3	2	3	2	3	2
RMSE	1	1	2	3	3	3	2	2	2
MB	2	2	3	3	3	2	1	1	2
IQRB	1	1	3	2	2	1	3	3	2
Total:	**5**	**5**	**9**	**11**	**10**	**9**	**8**	**9**	**8**
R	1	1	1	2	3	3	3	2	2
RMSE	1	1	2	3	3	3	2	2	1
MB	3	2	3	2	3	2	1	1	1
IQRB	1	2	1	2	3	3	1	1	1
Total:	**6**	**6**	**7**	**9**	**12**	**11**	**7**	**6**	**5**

Total ranks for all nodes and metrics.

The smaller the number the better the overall performance. The best possible score is 24 (reanalysis performing better that the two others in all flow regimes and according to all metrics), and the worst score is 72. You see here also, that for nodes (0,2) and (1,2) JRA performs better than ERA5. JRA-55 is also better than ERA5 in terms of mean bias.

The total scores for Casey are:

- ERA5 38 (as from 5+5+9+6+6+7)
- JRA-55 43
- MERRA-2 62

Thank you for making the suggestion and for setting out an example so clearly.

We have calculated these ranking scores for each reanalysis, station, metric and SOM node. However, the interpretation of a score for a given SOM node is not as straightforward as, for example, a score for a particular percentile range. Some nodes are fairly similar to other nodes and, critically, some nodes are more frequent than others. We therefore opt instead to calculate a rank score (1, 2 or 3 in order of performance) for each node and metric but to weight that rank by the frequency of the node (e.g. the reanalysis with the highest r value for a node with a frequency of 20% gets a 0.2). Then, the score for that metric/station/reanalysis is the sum of those weighted node scores. We then assign equal weighting to each metric (r, MB, RMSE, IQRB) and sum them together for a final score for each station, which gives the following ordering:

Casey: 1. ERA5, 2. JRA55, 3. MERRA-2

DDU: 1. ERA5, 2. JRA55, 3. MERRA-2

Mawson: 1. JRA-55, 2. MERRA-2, 3. ERA-5

Neumayer: 1. ERA5, 2. MERRA-2, 3. JRA-55

And a final overall ranking of 1. ERA5, 2. JRA-55 and 3. MERRA-2.

But note this places a lot of emphasis on bias and only a small amount of emphasis on getting the variability right, whereas arguably the latter is the most desirable thing (and difficult) for the reanalysis to do. ERA5 has the highest scores for correlation coefficient by a greater margin and for all four stations.

Furthermore, we feel that the methodology of this paper is perhaps not best suited for an evaluation centred upon summary statistics; only a small subset of stations are used and the surface wind measurements are only quasi-independent of the reanalysis due to the assimilation of sonde data. Instead, the emphasis of the paper is on understanding differences between the reanalyses by comparing them in detail in different local conditions and against different datasets. Some of these characteristics can be captured by summary statistics, others less so. In our revisions, we have opted to include the rankings in Appendix D and have rewritten the second paragraph of Section 4.1 accordingly:

"Alongside the detailed breakdown of performance statistics across various metrics, rankings for each reanalysis by station and by metric (using only surface wind speed) are given in Appendix D. These indicate that ERA5 exhibits the best performance overall, especially for r (as with ASCAT). Higher resolution in ERA5 also supports more realistic coastal orography; steeper coastal slopes are in turn likely to be important for realistic terrain-driven baroclinicity and katabatic forcing (Fulton et al., 2017). Temperature profiles also appear to be most realistic in ERA5, with JRA-55 and MERRA-2 exhibiting substantial cold biases in the lowest 1000m a.g.l. and a decline in wind speed r values near the top of the easterly jet region in some SOM states. However, JRA-55 and MERRA-2 exhibit lower mean bias and bias in interquartile range. Negative bias is especially large at high wind speeds in ERA5 when compared with both station measurements and ASCAT. The contribution of this substantial and time-varying bias to long-term trends and wind stress in the coastal sector warrants further investigation but these results suggest that using ERA5 to analyse coastal extremes requires careful consideration of the effect that a systematic negative bias could have upon the results."

Supplementary information

I don't think you need to have Appendix D at all, because the figures D are mentioned only once on page 6 line 134.

We agree that this should not have been presented as an Appendix. Appendices which only contain plots should have been Supplementary Material rather than appendices, so we have moved Appendix D, E and F to the Supplement. Appendix G (now Appendix D) has had text added following the previous comment.

Technical Corrections:

- If I'm not mistaken, the time period of the study is mentioned quite late in the text (page 9): could you mention it earlier?

Added this to line 85:

"Near-surface winds are evaluated against in-situ and scatterometer-derived winds for the period January 2010 to December 2017."

- Caption of Fig 3: I find it hard to understand panels (a) and (b). Could you try to rephrase the caption?

Rephrased to: "Mean wind speed performance metrics by (a) latitude and (b) longitude against ASCAT for ERA5 (blue), MERRA-2 (orange) and JRA-55 (green). Includes correlation coefficient (solid) and RMSE (dashed)."

- Page 10, line 226: "[…] around the coast (Figure 3b)" -> again, I cannot grasp the idea. Do you mean that ERA5 has the highest correlation anywhere around the coast, no matter what the latitude is?

Rephrased this sentence to "Latitude-mean correlation coefficients are highest in ERA5 at all longitudes". I.e. each value shown on the line in Figure 3b is an average across all coastal latitudes (which are demarcated in red in Figure 4) at that particular longitude. So ERA5 has the highest correlation anywhere around the coast, no matter what the longitude is (the coast of Antarctica spans all longitudes).

- Page 11, Figure 4: why does JRA55 have so much more missing data than ERA5 and MERRA-2? Do you mention it somewhere in the text?

This is referred to earlier in the text: "The time resolution for JRA-55 is coarser (6 hourly), thus the rounded ASCAT timestep can only be ``matched'' on four timesteps (i.e. 00, 06, 12, 18 hours), and as a result certain overpasses cannot be collocated following our time-collocation methodology. The points that cannot be collocated are left missing." But it is not clear from this that this is the cause of the missing data in Figure 4c so we have added to the text at line 261:

"Note that regions of missing data in JRA-55 are where not enough points could be collocated (i.e. fewer than 50) between ASCAT and the reanalysis due to the reduced frequency (6-hourly) of data compared to ERA5 and MERRA-2 (hourly)."

- Page 11, line 245-246: "[…] JRA-55 does not have as consistent a sign of difference in wind speed compared to the other two reanalyses." -> this is correct, but it seem to me also, that JRA-55 bias pattern is similar to that of ERA5 and MERRA-2. According to Fig 4, it seem that JRA has systematically higher wind speeds than ERA5 and MERRA2, but the bias pattern is similar. This might be worth mentioning.

Agreed, but note line 259 which states "the largest differences between the reanalyses and observations are found in the near-coastal region". (i.e. for all three reanalyses). Also added to line 268: "but some similar patterns emerge, including more pronounced bias close to the east Antarctic coast and along the western peninsula."

- Page 13, Line 284-285: "[…]nodes (1, 1) and (1, 2) have comparable large-scale pressure gradients but the low-level pressure contours associated with the offshore low are orientated along the coast for (1, 1) whereas in (1, 2) they are oriented more perpendicular to the coast. " -> I think that an essential difference between DDU nodes (1,1) and (1,2) is also that the low pressure center is located on different side of the station.

Added: "This is due to the differing position of the low pressure centre with respect to the station"

- Page 14, Table 1: As I mentioned, I don't think that this table is necessary for the final manuscript.

This has been moved to the Supplementary Material.

- Page 16, line 300-301: " they have been marked with a '[K]' in Figures 5 to 6 and 7 to 10. "-> It is a good idea to mark the katabatic cases in the figures, however, there is a slight risk to confuse K with kelvin-units. Maybe you could use "Kat." ?

Agreed – thank you for your suggestion which we have adopted in Figures 5-10 as well as in the text where mentioned.

- Titles for sections 3.2.2 to 3.2.5 could be more informative. For example "Wind and temperature profiles at XXX" or "Effect of weather regimes on the wind and temperature profiles at XXX"

We do agree that the short subsection titles are uninformative alone, but we feel the longer point about the role of weather regimes is captured in the title of the encompassing section "State-dependent performance against station measurements: SOM regimes". To avoid making the sub-titles too long (and note we also look at surface station data), we have updated them to "Performance at…station".

- Figures 7-10: test the significance of the correlation (eg. student t-test for correlations), mark the significance level on the figure, or mention it in the caption.

We tested the significance of all correlations shaded in Figures 7-10 at the 99% level using a Student's t-test and found no height levels/reanalyses/stations/nodes where the correlation was insignificant by that measure. One way of visualizing this would be to hatch the plots, for example as follows:

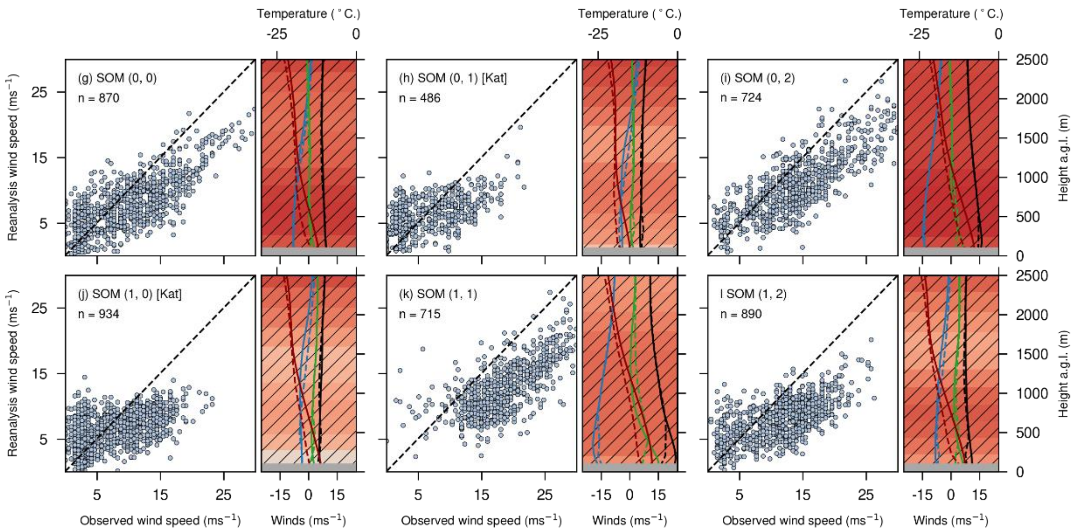


Figure 1 - SOM scatterplots and profiles for ERA5 at Mawson station as in the manuscript, but with hatching where correlations are significant at the 99% level.

but as the reviewer noted the information content of these plots is already large and all regions would be hatched anyway. We therefore opt instead to note in the caption the results of the significance testing:

“All shaded correlation coefficients are significant at the 99% level, according to a two-sided t-test.”

Reviewer 2

Summary

This paper assesses how well Antarctic coastal (predominantly easterly) winds are represented in three contemporary reanalyses products. The authors use self-organizing maps to assess under which synoptic settings these products perform best/worst. The topic of this paper is original and important, as Antarctic coastal winds are pivotal for the interaction between the ocean and ice sheet (melt) underneath ice shelves. The paper is well written albeit somewhat long. The figures could be clarified here and there, please see suggestions below. All in all, these comments should be addressable with minor revisions.

We thank the reviewer for their review, comments and suggestions. Please find below a response to each of the comments. We also agree with the assessment of both reviewers that the paper is quite long, and we note that as part of our response to Reviewer 1 the figure content and complexity of the SOM results has been reduced by moving Table 1 to a Supplement and reducing the displayed SOM scatterplots/profiles to only the three most frequent at each station (but making the full set available again in a Supplement). Fewer appendices are also included in this updated version of the manuscript as those which included only figures have been moved to a Supplement.

Major comments

l. 48: Important to make clear from the outset how ‘synoptic’ and ‘katabatic’ forcing are defined, and what the other processes (advection, thermal wind) entail. “…but its offshore extent is a source of uncertainty”. By its definition, katabatic winds become zero over flat terrain (as confirmed in Figure 6); the resulting winds are either driven by momentum advection or thermal wind effects.

Thanks for this important point. Firstly, we have amended this line to:

“One important driver is katabatic forcing, which sustains shallow terrain-following drainage flow towards the coast (Ball, 1960; Parish and Bromwich, 1987) but its flow often stops abruptly at the coastal margin (e.g. Yu and Cai, 2006;, Tomikawa et al., 2015;, Vignon et al., 2020) and its behaviour in models is sensitive to the representation of the atmospheric boundary layer (King et al., 2001; Parish and Cassano, 2003; Orr et al., 2014). As a result, the indirect effect of katabatic forcing on offshore flow, for example due to momentum advection, is uncertain.”

Secondly, we have added the following to the introduction at line 64:

“Throughout this paper, we refer to 'synoptic' and 'katabatic' forcing to characterise short-term variability. By 'synoptic' we are referring to variations in flow linked to large-scale pressure gradients (order of 1000 km) set up over a timescale of days, for example due to the passage of low pressure systems. 'Katabatic' forcing is instead associated with near-surface thermodynamic processes driving downslope flow, with uncertain variability through time.”

l. 87: Why was the neutral wind product used, and how large is the difference with the non-neutral product? Stability effects can be significant in polar regions. In l. 100 u* is used to calculate the neutral winds, but usually, u* already has the stability correction applied, please confirm/comment.

The neutral wind is used for comparison as it is believed to be more comparable with ASCAT than the non-neutral winds (Herbsach, 2010). We have made this clearer in the text on line 85:

"Near-surface winds are evaluated against in-situ and scatterometer-derived winds for the period January 2010 to December 2017. For the comparison with scatterometer winds the reanalysis neutral 10 m winds are used. This is because the same assumption of a neutral profile is made when deriving ASCAT 10 m winds from surface stress (the feature directly observed by scatterometers) so, as in the ECMWF assimilation scheme (Hersbach, 2010), we consider them to be more comparable."

Regarding MERRA2, by definition u should have the stability correction directly or indirectly. We use the bottom-up text-book (e.g. Stull, R. B. (ed.) (1988). An Introduction to Boundary Layer Meteorology (Springer Netherlands). doi:10.1007/978-94-009-3027-8) equation to calculate neutral wind at 10m.*

Please find below a comparison of the reanalysis wind speeds for ERA5 and MERRA-2 using the stability-dependent winds vs the neutral winds. These plots show stability-dependent winds minus neutral winds. In almost all regions the neutral wind speeds are stronger (blue colours). In both ERA5 and MERRA-2 the difference from using neutral winds is greatest to the north of the Ross Ice Shelf as well as over the Weddell Sea. The Ross Ice Shelf air stream (RAS), as simulated in Parish et al. (2006) [doi:10.1029/2005JD006185], is associated with a highly-stable near-surface layer. This cold layer travelling over open ocean could have a substantial impact on the near-surface stability. Over the Weddell Sea stable flows off ice shelves (Filchner-Ronne) and the multiyear sea ice could be playing a similar role (as seems to be the case off the Amery too).

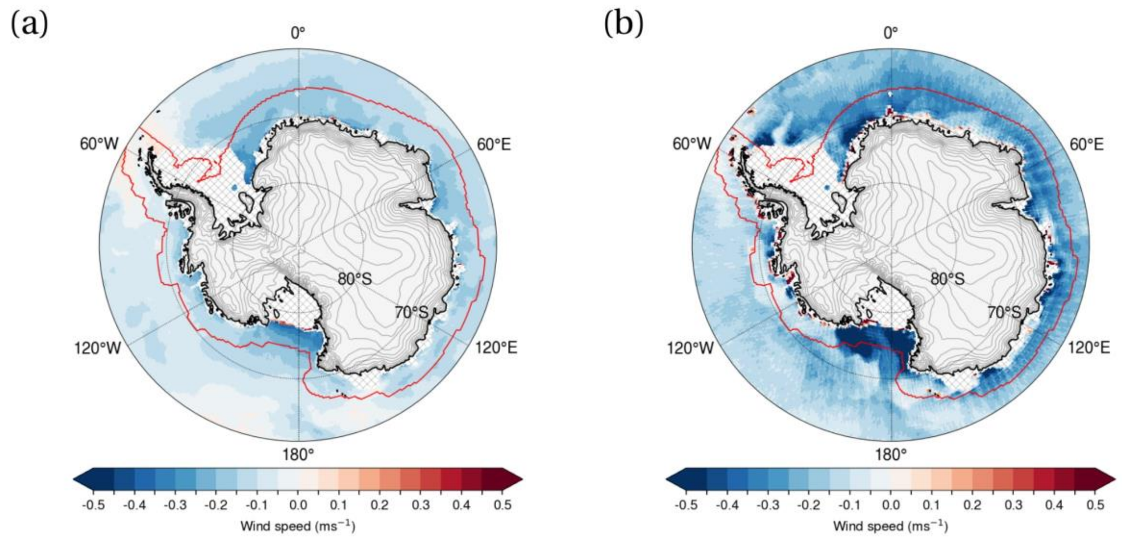


Figure 2 – stability-dependent winds minus neutral wind speeds at 10 m for (a) ERA5 and (b) MERRA-2.

References:

- *Hersbach, H., 2010. Assimilation of scatterometer data as equivalent-neutral wind. ECMWF.*
- *Stull, R.B., 1988. An introduction to boundary layer meteorology (Vol. 13). Springer Science & Business Media.*

l. 122: As the authors acknowledge, ASCAT is assimilated into all three products. This is then the place to comment on how suitable such a product is to assess the quality of the reanalyses. What causes any remaining differences anyway?

Evaluating reanalyses is difficult as most reliable long-term and large-scale wind datasets have been assimilated. We have added the following to the end of Section 2.2:

"We use ASCAT to evaluate reanalysis near-surface winds offshore, but it is important to note that ASCAT wind observations are assimilated into all three reanalyses used in this paper from 2008. A close correspondence between ASCAT and the reanalyses is expected, therefore. Remaining differences could be due to large biases in the first guess model, inadequacies in the assimilation system, scale mismatches and inclusion of observations in our results which were not assimilated by the respective reanalyses. We do not intend to provide an independent evaluation of skill using ASCAT, but instead to compare between reanalyses, and to highlight aspects of the observed wind field and variability which are not captured by the reanalyses in spite of the observational constraints applied."

We also comment on this in the last paragraph of Section 4:

"ASCAT observations and sonde data were assimilated into all three reanalyses, meaning only the 10 m wind observations are quasi-independent in our evaluation. The assimilated observations were likely important for reanalysis performance and so further evaluation with unassimilated observations would be valuable. For example, the effect of assimilating new Antarctic dropsonde observations from the Concordiasi field experiment on ECMWF forecasts was found to be larger away from the coastal regions where most of the existing radiosondes are launched (Boullot et al., 2016) and additional sonde observations from the Year of Polar Prediction improved wind speed forecasts over West Antarctica (Bromwich et al., 2022)."

We also note that representation of extremes is likely to be affected by the assimilation method on line 250:

"It is also possible that the representation of extremes is affected by the assimilation method; for example the assimilation of extreme scatterometer winds into ECMWF analyses is sensitive to the data thinning and quality control procedures (De Chiara et al., 2017)."

Minor and textual comments

l. 23: "Coastal winds also modify sea ice concentrations" Of course they do, but winds over Ross ice shelf are forced differently (barrier winds) and are predominantly southnorth, rather than east-west. So perhaps not the best example for this particular study.

Agreed, but this issue is perhaps more a result of a fact that the first part of the introduction focuses so much on the 'easterly' aspect of the winds, whereas in fact the analysis as a whole relates to the *coastal winds in general, which are quite directionally inconstant. Whereas the long-term mean easterly component is important for ocean circulation, the sea ice distribution is affected by coastal winds in all directions (and arguably the meridional winds are more important). An additional confusion is that the 'Coastal easterlies' is the general name given to near-coastal Antarctic winds (based upon the long-term mean) but they are more generally coast-parallel and in some places have a large meridional component. As our paper focuses more on the short term, we have updated the text to use 'coastal easterlies' as a description for a region or where the winds are described in a climatic context, but otherwise use 'coastal winds' or 'Antarctic coastal winds'. However, we have also included a reference to a paper discussing the role of coastal winds upon sea ice concentrations away from the Ross Ice Shelf, and clarify in the introductions when we are referring to meridional winds (line 22):*

"Antarctic coastal winds also have a large non-easterly component, especially on short timescales. Meridional winds could act as a control on ice shelf stability in some regions and have a major impact on sea ice concentrations, for example off the Ross Ice Shelf (Kurtz and Bromwich, 1985; Petrelli et al., 2008; Mathiot et al., 2012; Silvano et al., 2020) and in other prominent regions of sea ice production (Wang et al., 2021)."

Figure 1: Include in the caption that arrows represent vector average wind speed, and so go to zero length in regions with zero directional constancy.

Done.

Figure 2: per row please include the name of the station.

Done.

Fig. 3a and b: Please include a color legend in the graph.

A legend was added here as well as to Figure B1.

l. 252: surface -> near-surface (throughout for wind)

Done.

l. 260: "although the effects of atmospheric stability are accounted for in our analysis."
Unclear, do you use neutral wind speeds?

Updated this sentence to:

"Some of the observed differences between ASCAT and the reanalyses may be due to inconsistencies in how scatterometer and reanalysis 10m winds are derived, although the effects of atmospheric stability are accounted for in our analysis by making use of the reanalysis neutral winds rather than stability-dependent winds"