Review of "Interhemispheric perspective on the most extreme surface winds in the storm tracks" by Aleksa Stanković, Rodrigo Caballero and Gabriele Messori.

This is an interesting and well written study which investigates large-scale processes leading to extreme surface winds in the mid-latitudes and more specifically attempts to explain why the most extreme surface winds are stronger in the northern hemisphere storm tracks whereas bulk measures of storm track activity such as median wind speed and eddy kinetic energy are largest in the southern hemisphere. The methods are generally appropriate although some parts are not clearly explained (see major comment 1 below) and I have reservations about the coarse resolution of the ISCA model simulations (major comments 2 and 4 below). The main results will be of interest to the scientific community, particularly the strong correlation found between the surface winds and the mid-troposphere Eady Growth Rate, however, not all conclusions are easily understood with the evidence presented (major comment 3 below). I also note a number of minor comments below which when addressed will certainly improve this manuscript.

We thank the reviewer for the time taken to thoroughly go through our paper and give comments that helped to improve it. Please see our responses to the specific comments below.

Major comments

1. Section 2.2. especially between lines 95 – 114. This section is quite hard to understand and there are many parts which are not completely clear. I appreciate the same approach was taken in the author's previous work (Stanković et al, 2024), however the method needs to be fully understandable to reader who is not familiar with this previous paper. Specific issues are:
    1. a) Equation 1, lines 95-96. It is not clear what the footprint is here which the severity index is summed over. Is this the cyclone footprint?
    2. b) Line 105, "We calculate the severity in each basin (defined by the masks described earlier)". Does masks refer to the storm track or the cyclone here?
    3. c) Line 106, 109-110. It is not clear what the term "contiguous regions" means here. Please add some details to clarify
    4. d) Lastly, it is still not clear to me how extreme winds are allocated / assigned to a cyclone especially when more than one cyclone is present.

    We absolutely agree with you that the methodology needs to be understandable as a stand-alone section of this paper. We rewrote significant parts of the Section 2.2 to address this comment and all of its sub-comments. With the specific issues 1-4 in mind, the referenced parts of the Section 2.2 now have the following form on Pages 4-5, lines 105-133:

    "We find the cyclones associated with the top 100 most extreme wind events in each basin (i.e. *top 100 extremes*) in two steps. In the first step, we investigate all winter days within the previously defined storm track basins and find the top 100 most extreme wind events. In the second step, we connect these wind events to the cyclones which we further analyze.

The top 100 most extreme wind events are calculated by ranking every winter day in each storm track basin with respect to a wind severity quantified by a meteorological wind severity index. This procedure also consists of two steps. We first calculate a daily meteorological wind severity index for each grid cell within the storm track regions. As the wind severity index is only calculated if the daily maximum windspeed exceeds the local 98th percentile (see the Eq. 1 below), the index is equal to zero over a majority of the grid cells on any given day. An extreme wind footprint is defined as a contiguous region of wind exceedances over the local 98th percentiles, namely an area consisting of connected non-zero grid cells (no minimum area threshold is imposed on the footprint size). Our last step for finding the top 100 most extreme wind events consists of calculating the wind severity index for each wind footprint. As two or more extreme wind-causing cyclones can be simultaneously present in the same basin on a given day, there can be more than one extreme wind footprint. In this case, we only select the footprint with the maximum value of severity for that day (a visual illustration of the method can be found in Fig. 2 of Stanković et al., 2024). Lastly, we choose the 100 days with the highest severities in each basin.

We calculate a meteorological wind severity index (S) as follows:

$$\mathrm{S} = \sum_{i \in \text{footprint}} \left(\frac{v_i}{v_{98_i}} - 1 \right)^3, \qquad (1)$$

where i indexes all the grid cells within the wind footprint, v_i is daily maximum 10-m wind speed at grid point i, v_{98i} is the local 98th percentile with respect to the winter climatology from 1979 to 2020. This index, or similar forms of it, have long been used to study windstorms associated with cyclones (e.g., Klawa and Ulbrich, 2003; Leckebusch et al., 2008; Hanley and Caballero, 2012b; Moemken et al., 2024; Flynn et al., 2025). Although the wide-spread use of this index stems from its usefulness in modeling insurance-related windstorm losses, it also has physical grounding as the cube of the wind speed represents the flux of kinetic energy. Furthermore, it is reliant on an extreme percentile threshold which motivates its use for extreme weather analysis. Therefore, we use it to rank extreme wind events even though we are not directly interested in analyzing insurance losses. This same procedure was adopted in Stanković et al., (2024) to identify extreme windstorms over the central North Atlantic.

After finding the top 100 most extreme wind footprints, we connect them to objectively identified cyclones. We examine the wind footprints during each of the top 100 days and find the time and location of the highest daily surface windspeed within them. Then, based on the objectively identified cyclone tracks, we identify the cyclone centre closest to the location of maximum windspeed and use it to follow the cyclone back in time."

2. The horizontal resolution of the ISCA simulations, T42, is very coarse and is unlikely to resolve cyclones well. In Vallis et al (2018) they state " *...although with a spectral core certain horizontal resolutions are preferable, for example T42, T63,*

or T213." Therefore, I question why the simulations were performed at the coarsest of these resolution. It would be interesting at least to see how the control simulation output differs between T42 and T213 resolution e.g Figures 1c and 1d.

Thank you for raising this issue, as it is one of the most crucial aspects of the modeling part of the paper.

We chose to first test ISCA at T42 resolution because we have planned to do a lot of runs and wanted to keep computational costs manageable. Furthermore, we wanted to test usefulness of this resolution as it is the only readily available configuration of ISCA with all standardized input files needed for it to properly run (even though you rightly point out that ISCA could be run at higher resolutions). Although we believe that the team behind ISCA would be open to sharing advices on using T213 resolution, for example, we did not want to put an emphasis on that at this point in our study when it turned out that ISCA at T42 resolution captures the climatology and asymmetry well in the control simulation. The main reason behind it being that we think that the fact that ISCA adequately reproduces the main storm track statistics even with its lowest resolution (T42) is an important and interesting result in itself (more justification is given below in the newly added paragraph). Therefore, basing our results on simulations at this resolution was a deliberate choice on our part.

Because we agree with you that this issue did not receive a proper treatment in the previous version of the manuscript, we have added a new paragraph in the Summary and Discussion section which expands on it. The following paragraph which explains our reasoning is now on Page 13, lines 395-401:

"The fact that ISCA represents the climatological hemispheric asymmetry in extreme near-surface winds at a coarse resolution (T42) is another useful finding of our study. First, it points out that the hemispheric asymmetry is most likely primarily driven by large- and planetary-scale processes, as finer-scale processes are neither resolved at T42 resolution nor in ISCA's parametrization schemes. Second, documenting the asymmetry at a low resolution with simple parametrizations is a first natural step in studying this phenomenon across a hierarchy of climate models. Moving to more complex models could provide further insights into the hemispheric asymmetry and dynamics of the extreme cyclones by enabling step-wise investigation of processes at increasingly small scales."

We have further reflected on the limitations of using a coarse resolution model further in the Discussion section, in the sentence added after considering a comment from another reviewer. The sentence below is on Page 13, line 407-411:

"... although the ISCA model is able to represent the main features of the Earth's climate relevant for our study (like PDFs of surface winds in the storm track regions), it does not fully capture the presence of pre-existing downstream cyclones in the evolution of individual cyclones associated with extreme winds.

Assessing whether ISCA run at a higher resolution could better represent cyclone-cyclone interactions in top extremes as documented in ERA5 is a potential avenue for future research."

3. Lines 197 – 199 and 203 - 204. I struggle to follow the logic here. In all storm tracks there is a pre-existing cyclone downstream of the developing intense cyclone. However, here it is argued that the presence of this pre-existing cyclone is why the PV gradient is intensified and the jet is stronger. However, in the southern hemisphere, the PV gradient remains weak despite the presence of the pre-existing cyclone. Also see lines Line 368 – 369 in the conclusions where the same comment is valid.

   We do think that our logic in the mentioned lines is consistent – pre-existing downstream cyclones are present in all storm track basins, and advection of the background mean-state PV gradient by that pre-existing cyclone can be connected to locally intensified PV gradients and stronger jets, but the *magnitude* of that effect is weaker in the Southern Hemisphere than in the Northern Hemisphere. We subsequently connect these differences in magnitudes of PV gradient and the jet to differences in potential for explosive growth in the lines that follow those referenced in this comment.

   However, we agree with your comment that the difference in PV gradients between the hemispheres is substantial. Part of the explanation is likely tied to different climatological background state (please see our response to the minor comment #10 as well). As can be seen on Fig. 3, values of PV poleward of cyclones in the Southern Hemisphere are lower than those in the Northern Hemisphere. Therefore, the pre-existing cyclones acting in the same way in both hemispheres are acting on a different background state. This makes, as we have argued, a *quantitative* difference between the hemispheres.

   We further agree with you that paragraph where the lines referenced in the comment (lines 197-199 and 203-204 in the old version of the manuscript) was not written clearly enough and that it could seem to be contradictory. A lack of clear language in this part of the manuscript could affect the referenced lines 368-369 as well.

   As a response to this comment, we added more material in the referenced paragraph between the previous lines 197-199 and 203-204. We hope that with this addition our reasoning is clearer and the logic of our argument easier to follow. The following sentences are added on Page 8, lines 240-243:

   "However, the absolute values of upper-level PV poleward of pre-existing cyclones are larger in the NH than in the SH (Fig. 3). This is another quantitative difference between the hemispheres which could potentially limit the effectiveness of pre-existing cyclones in increasing the upper-level PV gradients in the SO. This is important as the composites of EGR show…"

4. Lines 219 – 221. Here it is stated that ISCA is able to realistically represent the spatial pattern of extreme winds. However, when Figure 1a and 1c are compared

there are some notable differences that are somewhat glossed over here. These differences are listed next, however, I do wonder if the coarse resolution is one reason for the more zonal stormtracks.

Thank you for this comment and we agree that we should have added more discussion about spatial discrepancies between extreme winds in ISCA and ERA5. Please see our responses to your specific comments below.

1. a) In both the NA and NP the maximum in ERA5 is more towards the west / start of the storm track whereas ISCA has the maximum more in the centre / end of the storm track.

   We agree with you and we have pointed to this discrepancy in the new version of the manuscript.

2. b) The storm track in the NA appears more zonal in ISCA compared to ERA5.

   Yes, it is more zonal in ISCA, but it still contains tilt which is one of the main characteristics of the NA storm track. We have commented on this in the new version.

3. c) The SH storm track seems to lack its spiral characteristic in ISCA which is visible in ERA5 e.g. the ERA5 extreme winds have their maximum much closer to Antarctica in the Pacific sector compared to ISCA.

   We also agree with this observation.

In order to respond to this comment and all of its sub-comments, we have significantly expanded our discussion on differences in spatial characteristics of extreme winds between ISCA and ERA5 and included all of the raised concerns in the new version of the manuscript. Specifically, the following text is now on Page 9, lines 266-272:

"At the same time, there are some spatial discrepancies between representations of storm tracks in ISCA and ERA5. Most notably, when compared to ERA5, the peaks of the 98th percentiles in ISCA are shifted equatorward in the Pacific sector of the SO and eastward in the NA and NP basins. In addition, while ISCA does reproduce the tilt of the NA storm track, the tilt is not as large as in ERA5. Many of the biases that ISCA has are not surprising as they are reminiscent of biases of other, more complex climate models (e.g., Shaw et al., 2016, Koskentausta et al., 2025). Since our goal is to better understand general differences between storm tracks in an idealized setting, we deem ISCA to be a suitable tool for this purpose."

Minor Comments

1. Lines 74-75. Calculation of the eddy kinetic energy. Here u' and v' are computed by subtracing the "local monthly means for each month" (of u and v). Does this lead to a slight discontinuity at the end of each month? Usually a 30-day running mean is used (as is done in section 2.3).

   This is a valid point, as computation of u' and v' relative to the local monthly means would indeed introduce small discontinuities in case of the events that happen close to the end/beginning of each month. We also acknowledge that our way of calculating the EKE is not the only way to it,. It is rather just one of the ways commonly used in studies like ours (e.g., by Shaw et al. (2022) who defined transient eddies "as deviations from a monthly average, that is an average over a particular month not the climatological monthly average" or, similarly, Park & Kim (2021)). Because of this, we do not expect that the slight discontinuities would qualitatively affect our results. However, as a response to this comment, we now do acknowledge the potential discontinuities at Page 3, lines 83-85:

   "The way we calculate u' and v' is common in studies like ours (e.g., Park and Kim, 2021; Shaw et al., 2022) and while it can cause some discontinuities for the events that occur at month boundaries, we do not expect these discontinuities to qualitatively alter our results."

2. Section 2.2. How does the cyclone tracking algorithm deal with cyclones that merge? I believe this should be stated in this section given the result presented later on that the cyclone leading to the extreme winds merges with a pre-exisiting cyclone.

   The cyclone-tracking algorithm used here identifies a new category of "multi-centre" cyclones. In the case of a merger, instead of deciding which cyclone centre is the centre of a merged cyclone, or consider each cyclone centre as an individual cyclone, it keeps information about different cyclone centres and classifies the cyclone as a multi-centre cyclone. Therefore, in cases when extreme cyclones form a multi-centre cyclone with pre-existing cyclone, the information about the location of individual cyclone centres that comprise it is not lost.

   As a response to this comment, we have added the following description, now on Page 4, lines 98-100:

   "…multi-centre cyclones, which are comprised of several MSLP minima. Therefore, in the event of two cyclones merging, the information about the centres of the merged cyclones is retained and they are collectively seen as a part of the same, multi-centre cyclone structure."

3. Following on from minor comment #2, does the tracking scheme require a closed pressure contour to detect a cyclone?

   The tracking scheme does require a closed MSLP contour to detect a cyclone. However, based on the topological methods developed by Engelke et al. (2021), the cyclonic feature identified in this way can contain several other features, i.e. different centres which can form a multi-centre cyclone together.

4. Line 132, “hourly time resolution”. Is this the model timestep and the output frequency? It could be stated more clearly.

   Thank you, we have clarified this in the new version of the manuscript (line 151-152). Time resolution mentioned previously has to do with the output frequency, an atmospheric model has a timestep of 720s (12 min).

5. Line 140, ISCA simulation. Are only the SSTs changed? What about sea ice? Also are the land-surface / soil temperatures changed in the sensitivity simulations? These details should be mentioned.

   We do not change sea ice nor the land-surface/soil temperatures in our experiments. In connection with another comment from the other reviewer, we rewrote this part of the manuscript and we hope that these details are now clear (now around the lines 162-166 on Page 6).

6. Line 146, ZonalSST experiment. “SSTs are zonalised in both the tropics and extra-tropics” – does this also include the polar regions / high latitudes?

   The polar regions/high latitudes are zonalized up to the latitudes where the sea ice is present. These latitudes are shown on Figure 8a-c. Therefore, based on the exact definition of high latitudes, some parts of them do have zonalized SSTs. We also hope that the answer to the next comment and changes done in the manuscript in order to respond to it clarify this.

7. Line 146, Table S1. The latitude bands for the tropics and extra-tropics should be added to the table / table caption.

   Thank you for the suggestion. We have added the following description of the tropical and extratropical latitude bands to the caption for Table S1 (now on Page 32):

   “In experiments 3, 4, 7 and 8, SSTs are zonalized poleward of 30°N (S), until the latitudes covered by sea ice are reached. There is a transition across the 20 - 30 °N (S) latitude bands computed by smoothly increasing the weight from 0 to 1 using a cosine interpolation, with values of weight being 0 equatorward of 20°N (S) and 1 poleward of 30°N (S). In experiments 5 and 6, SSTs are zonalized across the latitude band from 20°N to 20°S, with a smooth transition across the 20 - 35 °N (S) latitude bands.”

8. Line 168. Time of cyclogenesis. On average, when do the top 100 cyclones have their genesis relative to the time of maximum winds? Here it implies this is about 2 days before, but in Figure 4 the time axis starts from -4 days.

   Thank you for pointing to a possible point of confusion. As the referenced line states, the majority of top 100 cyclones have cyclogenesis around 2 days before the time of maximum wind severity. However, some cyclones in each basin are identified by the tracking algorithm before this time. Even though the number of those that can be identified at t = - 4 days is small (18, 14 and 26 in the North

Atlantic, North Pacific and Southern Ocean, respectively) compared to those that originate around t = -2 days it was enough to construct the time series in Figure 4. As a response to this comment, we have made the x-axis in the previous Figure 4 (now Figure 5, shown on Page 25) shorter and starting from t = - 2.5 days (i.e., around the time where the majority of storms originate). We hope that this change helps to avoid potential confusion and adds to consistency with the line referenced in the comment.

9. Line 171 – 172 “the two cyclones appear to merge”, can this be checked with the cyclone tracks rather than stating “appear to”. This does depend on how the cyclone tracking algoritm deals with merging cyclones though (minor comment #2).

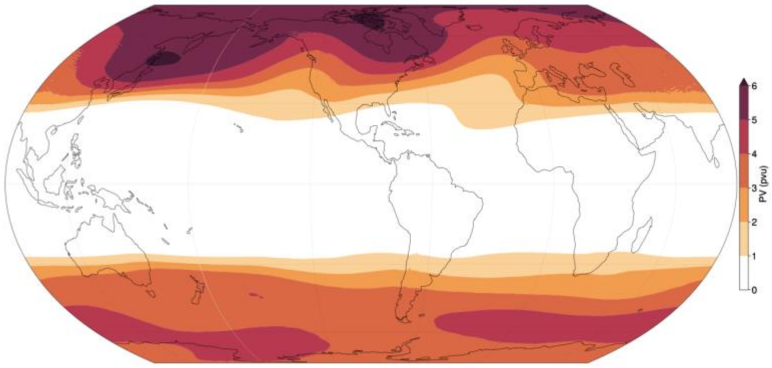
   We agree that the referenced sentence sounds too vague. We have checked how many of top 100 cyclones are objectively identified as multi-centre cyclones by the cyclone tracking algorithm. 49, 53 and 56 out of top 100 cyclones in the North Atlantic, North Pacific and Southern Ocean, respectively, are classified as multi-centre cyclones by the cyclone tracking algorithm +- 6 hours around t = 0 days.

   As a response to this comment, we rewrote the referenced sentence and added details discussed above. The following sentence is now on Page 7, line 197-200:

   “At t = 0 d (Fig. 2c,f,i), the two cyclones form a broad area of large negative MSLP anomalies. Around half of top 100 extremes in each basin form a multi-centre cyclone with pre-existing cyclones (49, 53 and 56 in the NA, NP and SO, respectively, are classified as multi-centre cyclones by the cyclone tracking algorithm +- 6 hours around t = 0 d).”

10. Line 178 – 179 /Figure 3 “Upper-level PV poleward of the top 100 extremes is greater in the NH than in the SH, making the PV gradient in the NH much larger than in the SH”. Why is this the case? In Figure 3g, h and I the SH cyclones appear much more like cut-off lows (detached from the high latitude PV reservoir) compared to the NH cyclones. Is this the case? (As an aside, I find it interesting that the NH cyclones in Figure 3 show quite a clear signal of cyclonic wave breaking).

    Greater values of upper-level PV in the NH are also an important quantitative difference between the basins. We have reflected more on it and identified it as such as a response to the major comment #3. When it comes to why that is the case, a part of the answer probably lies in the differences in the stationary wave patterns between the hemispheres. The figure below shows the respective winter climatologies of upper-level PV defined as in the manuscript (mean between 200 – 300 hPa). The figure shows a wavy pattern present only in the NH, with clearly identifiable throughs with high values of PV over both NA and NP storm tracks. The stationary waves are much stronger in the NH than in the SH because of the stronger forcing by the larger continents and more extensive orography (e.g., Wallace et al. (2023)), which can also play a role in the interaction between top extremes and pre-existing cyclones. Namely, if a pre-existing cyclone is situated to the east of these ridges in the NH, it will be advecting the PV from a reservoir where their absolute values are higher, which would consequently also make the PV anomalies more positive. We think that this is an interesting question that could potentially be more formally studied in some future work.



Upper-level (200-300 hPa) PV winter climatology for winter season (DJF in the NH and JJA in the SH)

We also agree that the NH cyclones show a signal of cyclonic wave breaking. When it comes to the SH cyclones looking more like cut-off lows, that could potentially be the case. At the same the pre-existing cyclone centers are at roughly the same distance in the SH as in the NH. To objectively verify whether this is happening, we would need to objectively test it, which is also a question worth exploring in future studies.

11. Line 199 – 204 and 374 – 376. The composites of the EGR are in the supplement but are important to the main conclusions of this paper – one of the three bullet points in the conclusions focuses on this and it is also mentioned in the abstract.Therefore, the authors should strongly consider moving Figure S1 to the main manuscript.

    Thank you for this suggestion. We agree with you and we have moved Figure S1 to the main manuscript where it is now Figure 4 (Page 24). We have accordingly changed the text in the manuscript to accompany for these differences. In addition, because of your figure comment #2 with which we also agree, this figure now contains 250 hPa wind speed contours on it. This restructuring of the composite figures also slightly influenced Figure 1 (where the 250 hPa wind speed contours are removed, while 10-m wind speed contours are added based on a comment from the other reviewer) and Figure 3 (where the MSLP anomalies contours are removed as they are now shown on the current Figure 4).

12. Lines 205 – 206. This is also shown in Figure 7.

This scatterplot is actually only shown in the Supplementary Material, as we wrote. The lines referenced are part of ERA5 section and its results are not on Figure 6 (we assume that was the figure referenced in this comment and not Figure 7).

13. Lines 207 – 211. The new group is cyclones is quite hard to understand both in terms of what they are and also why they have been considered. This text needs to be clarified.

    We agree that we should have been clearer about the new group of cyclones and that we should have given it more explanation. We rewrote the part that deals with the new group of cyclones and the following section is now found on Page 8, lines 250-254:

    "Furthermore, to test whether the relationship between EGR and 10-m winds holds more generally, we identify a new group of cyclones in each basin. We find the new group of cyclones by identifying cyclone tracks for which the maximum value of median EGR around the cyclone centers during the cyclones' lifetimes is ≈ 30 % lower than in the case of top 100 extremes (Fig. 5a). Time-series of 10-m winds around the new group of cyclones confirm our expectation that the cyclones with lower maximum values of EGR also have lower 10-m winds during their lifetime (see Supplementary Material)."

14. Line 255. How well is the impact of latent heat release on the storm track captured by ISCA? If this statement is more general rather than intrepreting the ISCA results I fully agree with it but its needs to be made clear this is not trying to explain the ISCA results.

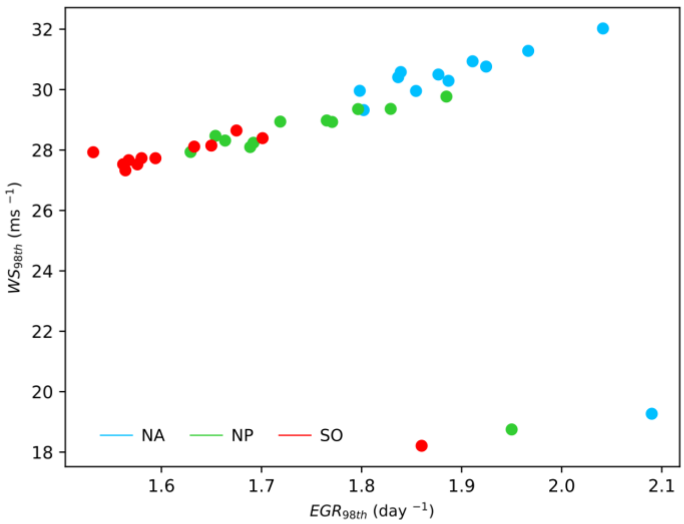
    Yes, you are right to point out that the statement mentioned above is a more general one. We rewrote the sentence and we hope that we have made our response clearer. The following sentence is now on Page 10, line 306-308, instead of the previously mentioned sentence:

    "In more general terms, latent heat release helps to set the mean thermal structure of the troposphere, indirectly affecting EGR. This means that EGR implicitly and at least partly captures the role of moist processes."

15. Line 252-253 / Figures 6 and S4. I understand why the wind speeds in ISCA are larger than in ERA5 as they are at a higher level, however, it is not clear why the EGR in ISCA is much larger compared to ERA5. This needs to be explained as it is almost a factor of 3 larger in ISCA compared to in ERA5.

    We thank you for this comment. As you rightly point out, while differences in wind speeds between ISCA and ERA5 are understandable, the values of mid-tropospheric EGR are not. The difference in values came from an error in the calculation of vertical derivatives in ISCA. We apologize for this oversight.

    After proper recalculation of EGR in ISCA, their values are now comparable with ERA5 values, as can be seen from the Figure below (now Supplementary). As a response to this comment, we have properly calculated EGR from ISCA and replaced previous Figures 6 and S4 (Figure 7 on Page 27 and Figure S3 on Page 31 in the new version of the manuscript).



Scatterplot of winter basin-wide 98th percentiles of near-surface wind speed (y-axis) versus winter basin-wide 98th percentiles of mid-tropospheric Eady growth rates (x-axis) across 11 different ISCA experiments (upper-right corner) and ERA5 (lower-left corner).

We want to emphasize that new values of EGR do not change any of our previous conclusions, as the correlation between extreme values of EGR and wind speeds is equally strong (r=0.97, still significant at 1% level). Therefore, we have not made any changes in the text of the manuscript.

We thank you once again for spotting a mistake and allowing us to correct it.

16. Section 4, Summary and Discussion. The first part of this reads much more like conclusions than a summary or discussion. I think some text would fit better in the currently rather short conclusions section.

We have considered moving some of the text from the Section 4 to Section 5. However, after expanding our Section 4 with further discussion motivated by various comments from both reviews, we decided not to transfer any parts of Section 4 to Section 5. The main reason being that we think that the new and more thorough Discussion section needs a longer Summary. We hope that you will find our choice justifiable.

As a response to this comment, we rewrote a part of the Introduction where we outline the structure of the paper to make our reasoning clearer. On Page 3, lines 64-65:

"We summarize the results, discuss their implication and provide a future outlook in Sect. 4. Finally, we list our main findings in Sect. 5."

Figure comments:

1. Figure 2, Please state in the caption that the southern hemisphere figures have been rotated so that south is to the top of the page.

   As suggested, we have added the following sentence to the caption of the Figure 2:

   "The y-axis for the SO composites (g-i) has been flipped so that the southernmost parts are at the top of the figures."

2. Figure S1. It would help to add the jet / 250-hPa wind speed contours to this figure.

   We have added the 250 hPa wind speed contours to this figure. As mentioned in the response to the minor comment #11, this figure is now Figure 4 of the main manuscript.

3. Figure S1 and Figure S4. The values of the Eady Growth Rate do not appear consistent between these figures. In Figure S1, the values are typically 1 – 2 day^{-1} whereas in Figure S4 the values are between 5 – 7 day^{-1}.

   We would like to point out that the values of the EGR are consistent between the previous Figures S1 and S4. The values of EGR that are between 5 – 7 day^{-1} are values calculated from ISCA, while the (previous) Figure S1 shows the values of EGR in ERA5. Values of EGR in ERA5 depicted on Figure S4 (lower left corner) are also 1 – 2 day^{-1}, which is consistent with Figure S1.

   After addressing the minor comment #15, the confusion caused by previous scatterplot figure should not be present in the new version of the manuscript.

4. Figure 5b and 5c. I find the titles of these plots quite confusing and therefore I am not 100% I understand what is plotted here. Why is there a -1? Should there be some brackets in this eqaution?

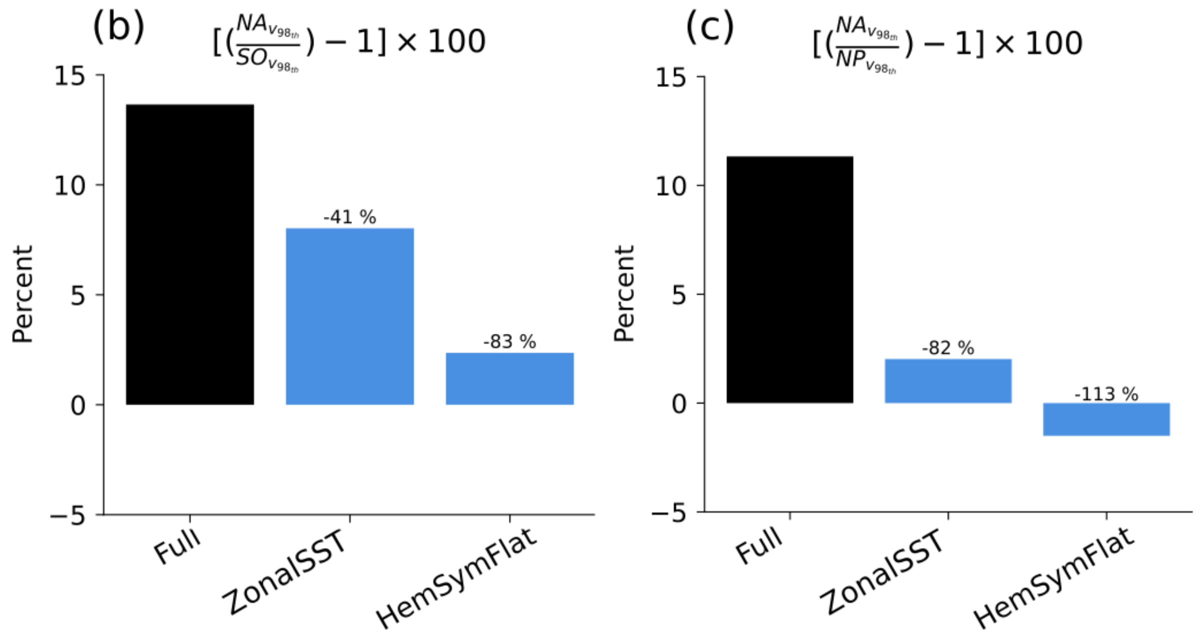
   We agree that the titles of these plots should have been formatted in a better way. As stated in the caption, what is plotted is:

   "the relative difference in the percentile values between the North Atlantic and the Southern Ocean (b) and the North Atlantic and the North Pacific (c)"

   , or explained in a slightly different way on lines 282:

   "difference of the basin-wide 98th percentiles of near-surface winds in the NA and SO/NP divided by SO/NP "

   We have changed the tiles by reformatting and adding brackets, so that the previous Figure 5b,c (now Figure 6b,c) looks like:



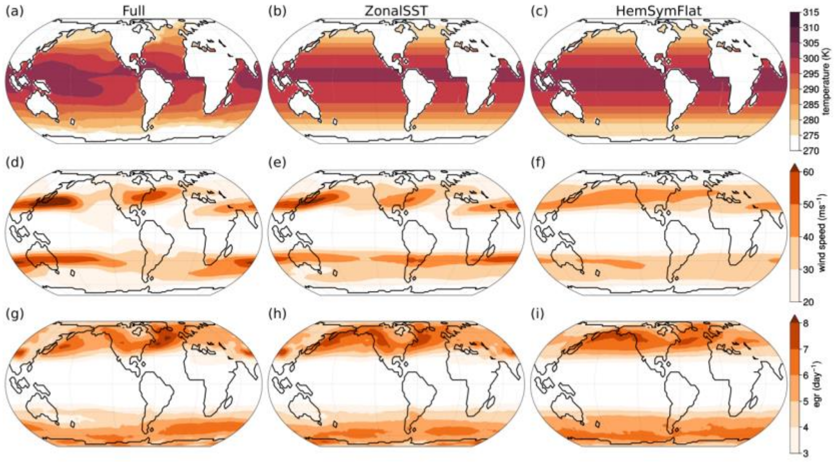
The relative difference in the 98th percentiles of surface wind speeds between the North Atlantic and the Southern Ocean (b) and the North Atlantic and the North Pacific (c) with the selected ISCA experiments.

5. Figure 6, Could add in the caption that ERA5 values are shown in Figure S4. It would also make it much easier for a reader if a legend could be added showing which colour is for which storm track (as is done in Figure S4).

   We agree with both comments – we have added a legend to the previous Figure 6 (now Figure 7) and pointed to the figure which contains ERA5 in the caption of Figure 6.

6. Figure 7, Are SSTs over land the land surface temperature? Or should these data points over land be blocked out in this figure?

   Yes, the data points over land should have been blocked, as we were only changing the SSTs and not the land surface temperature. The new version of the previous Figure 7 (now Figure 8) can be seen below.



Climatologies of SSTs (a-c), upper-level winds (d-f) and EGR (g-i) during respective winter seasons across the selected ISCA experiments.

References

Engelke, W., Masood, T. B., Beran, J., Caballero, R., & Hotz, I. (2021). Topology-based feature design and tracking for multi-center cyclones. In *Topological methods in data analysis and visualization VI: Theory, applications, and software* (pp. 71-85). Cham: Springer International Publishing.

Park, H. J., & Kim, K. Y. (2021). Influence of Northern Hemispheric winter warming on the Pacific storm track. *Climate Dynamics*, *56*(5), 1487-1506.

Shaw, T. A., Miyawaki, O., & Donohoe, A. (2022). Stormier Southern Hemisphere induced by topography and ocean circulation. *Proceedings of the National Academy of Sciences*, *119*(50), e2123512119.

Wallace, J. M., Battisti, D. S., Thompson, D. W., & Hartmann, D. L. (2023). *The atmospheric general circulation*. Cambridge University Press.