

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.

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:
 - 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?
 - 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?
 - c) Line 106, 109-110. It is not clear what the term “contiguous regions” means here. Please add some details to clarify
 - 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.
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.
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.
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.
 - 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.
 - b) The storm track in the NA appears more zonal in ISCA compared to ERA5.
 - 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.

Minor Comments

1. Lines 74-75. Calculation of the eddy kinetic energy. Here u' and v' are computed by subtracting 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).
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-existing cyclone.
3. Following on from minor comment #2, does the tracking scheme require a closed pressure contour to detect a cyclone?
4. Line 132, “hourly time resolution”. Is this the model timestep and the output frequency? It could be stated more clearly.
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.
6. Line 146, ZonalSST experiment. “SSTs are zonalised in both the tropics and extra-tropics” – does this also include the polar regions / high latitudes?
7. Line 146, Table S1. The latitude bands for the tropics and extra-tropics should be added to the table / table caption.
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.
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 algorithm deals with merging cyclones though (minor comment #2).
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).
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.
12. Lines 205 – 206. This is also shown in Figure 7.
13. Lines 207 – 211. The new group of 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.
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 interpreting the ISCA results I fully agree with it but it needs to be made clear this is not trying to explain the ISCA results.
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.
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.

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.
2. Figure S1. It would help to add the jet / 250-hPa wind speed contours to this figure.
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 $1 - 2 \text{ day}^{-1}$ whereas in Figure S4 the values are between $5 - 7 \text{ day}^{-1}$.
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 equation?
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).
6. Figure 7, Are SSTs over land the land surface temperature? Or should these data points over land be blocked out in this figure?