This is a very nicely written paper that generalises the results of the authors' previous study, showing that the most extreme windstorms in all of the storm track regions have a downstream pre-existing cyclone. The paper also demonstrates that a low-resolution idealised model can capture the hemispheric asymmetry in the extreme winds (where the NH storm tracks have stronger extreme winds than the SH). Experiments with this model indicate that the asymmetries are related to the Eady growth rate, and the topography.

We thank the reviewer for the time and effort taken to review our manuscript and make comments that help to improve its quality. Please see our responses to the comments below.

I have a few comments and suggestions on the manuscript.

1. Every instance of "northern hemisphere" and southern hemisphere should be Northern Hemisphere and Southern Hemisphere.

   Thank you! The new version of the manuscript now has the proper capitalization.

2. Lines 54-55: I think the use of this intermediate complexity climate model is a key point of novelty in this study, therefore it should be given more attention in the introduction. I suggest including a paragraph on the use of such models and what previous studies have found when doing experiments such as the ones you have done.

   This is a good point and we agree with the comment. In connection with another comment from the other reviewer, we have given more attention to the use of climate modeling in this study. More specifically, we have added the following paragraph to the Introduction section on Pages 2-3, lines 55-62:

   "We address the last question by using an intermediate-complexity climate model. Use of climate models with varying complexities has long been recognized as a way to deepen the understanding of the climate system (a good overview is provided in Vallis, 2016). Furthermore, climate modeling is widely applied to study the storm tracks (Shaw et al., 2016). Boundary conditions, such as orography, sea surface temperatures, land-sea contrast and ice cover have been varied to investigate their effects on both the individual and hemispheric storm tracks (e.g., Brayshaw et al., 2009, 2011; Patterson et al., 2020, White et al., 2021, Shaw et al., 2022, Hay et al., 2025). Following these examples, we use a climate model to test whether the boundary conditions documented to be of a general importance for the storm track dynamics also influence the hemispheric differences in extreme surface winds."

3. Line 142: I'm not sure about the benefit of saying that you ran 11 different experiments and will only discuss 3. It might be better to fully justify the experiments that you are including – why choose these set-ups? What were the hypotheses you were thinking about when running the experiments?

   We agree with you that we should have added more theoretical justifications behind the choices of our experiments. In connection to our answer to comment #2, we rewrote parts of the Data and Methods section in which we describe our experiments and explain our hypotheses based on the previous literature. The following paragraph is now on Page 6, lines 161-166:

   "As discussed in the Sect. 1, the previous literature suggests that the storm tracks are heavily influenced by different distributions of sea surface temperatures and the presence of orography and land surfaces. This motivates our ISCA experiments through which we test how different configurations of these boundary conditions influence climatologies of near-surface winds in different basins. Additionally, we design two experiments to test whether the differences in storm tracks are influenced by the presence of Greenland and its orography, which have previously been shown to influence large-scale circulation in the Northern Hemisphere (e.g., Jung and Rhines, 2007; White et al., 2019)."

   We also agree that it can be somewhat confusing that we mention 11 experiments and only discuss 3 of them. Alongside theoretical considerations mentioned in the previous paragraph and the Introduction, the most important reason for only including 3 of them is that their results the most consequential when it comes to reducing asymmetries between the basins. The reason why we have still decided to mention all 11 of them (and put them into Supplementary Material) is because of the previous Figure 6 (now Figure 7) - the figure with the scatterplot. If all 11 experiments are included, our conclusions about the strength of the relationship between extreme EGR and near-surface winds are much more robust, than if we were to base them on only 3 experiments. We agree this should have been clearer in the previous version of the manuscript. We tried to make it clear by writing the following sentences on Page 6, lines 167-169:

   "We run 11 different experiments, which are listed and described in the Supplementary Material. For the brevity, however, we focus our analysis on the three experiments which we find to be the most consequential."

   We hope that this can, together with the first part of our responses to this comment and the comment #2, make the reasoning behind our choices clearer.

4. Line 153: I'm not sure what you mean when you say the "prescribed SSTs are found". Surely if you prescribe them you do not need to find them. Perhaps this just needs rewording.

   You are absolutely right that this is not a correct wording. What we meant to say is that we show the prescribed SSTs in the Results section. This sentence has a new form on Page 6, line 178:

   "The prescribed SSTs are shown and discussed in the Results section."

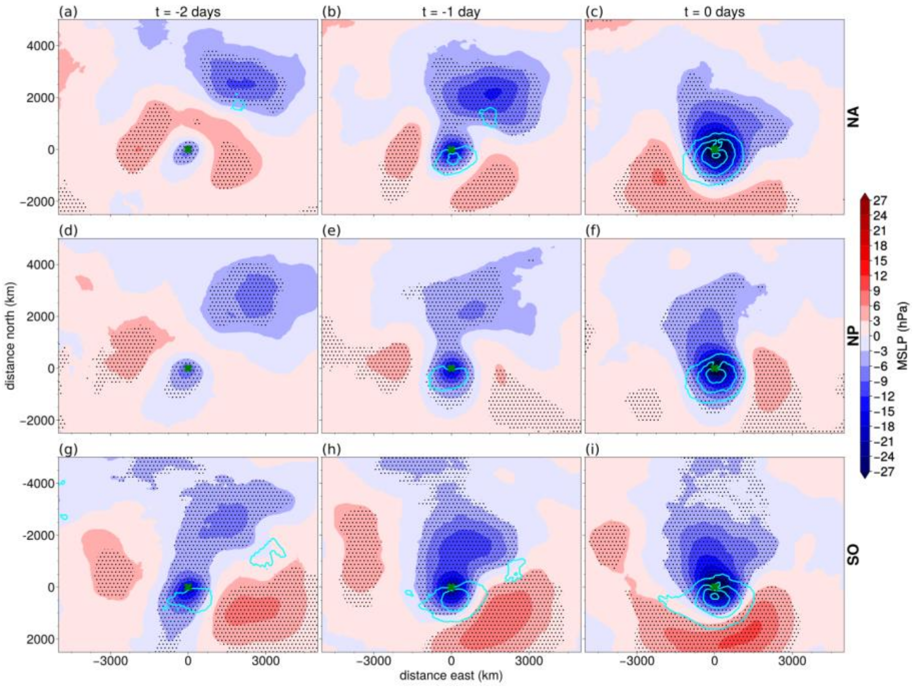
5. Line 157: Can you say how you know the surface winds are at a higher level in the model?

   We agree that we can't simply diagnose whether near-surface winds in ISCA are above 10-m height, as that is not the output in ISCA. As the main point of the abovementioned sentence was to justify why we expect ISCA's near-surface wind to be larger than in ERA5, we have removed the sentence from the manuscript. Instead, to be more precise, the sentence below is put in its place on Page 6, line 182-183:

   "However, we expect the near-surface winds to be less affected by surface friction, as the effects of the friction in ISCA are not fully resolved."

6. Section 3.1: I think it would be really beneficial to include composites of the winds that you are trying to explain. This would help to see if there are structural differences between the NH and SH too.

   Thank for the suggestion. We agree that the composites of 10-m winds are beneficial and add more useful details to our analysis. Because of this comment, we have decided to incorporate the 10-m wind composites into the current Figure 2. The figure below is now part of the main manuscript:



Composites of MSLP anomalies (colors) and 10-m wind speed (contrours) for top 100 extremes in the NA (a-c), NP (d-f) and SO (g-i) centered on the cyclone locations from t=-2 days to t=0 days.

10-m winds are shown as blue (cyan) contours that start from 12 ms^{-1} and increase in steps of 3 ms^{-1}. Because of a slight rearrangement of the composites figures that happened as a response to this comment and a different comment from the other reviewer, the 250 hPa winds are not present on this Figure like they were in the previous version of the manuscript.

The following description of the Figure is now a part of the Results section on Page 7, lines 202-208:

"Figure 2 further shows that the intensification of 10-m wind speeds around top 100 extremes coincides with their deepening. A clearly defined area with 10-m winds exceeding 12 ms^{-1} is detected equatorward of the cyclone centers at t = -1 d in all basins (Fig. 2b,e,h), and is already present in a more confined form in the SO at t = -2 d (Fig. 2g). However, the largest intensification of 10-m wind speeds in all basins happens between t = -1 d and t = 0 d. At t = 0 d (Fig. 2c,f,i), a large footprint of strong 10-m wind speeds stretches from around 500 km poleward to 1500 km equatorward of the cyclone centres of top 100 extremes. At the same time, the highest values of 10-m wind speeds in all basins are close to and equatorward of the cyclone centres of top 100 extremes (Fig. 2c,f,i)."

7. Line 221: typo in "central".

Thank you – corrected.

8. Line 284: Can you refer to a figure here?

   We have added a reference to the figure now, on Page 11, line 335:

   “Flat orography reduces the tilt of the jet and makes it more zonally symmetric in both basins in the NH (Fig. 8 d-f).”

9. Line 287: It would be good to make it clear here that this result generalises the findings of your previous study (rather than being a brand new result). This is also the case for lines 300-303.

   Thank you for pointing this out. When it comes to the previous line 287, we have already tried to make it clear that this is a generalization a few sentences below it at the end of the paragraph with the following sentence (now on Page 11, lines 353-354):

   “Thus, our study generalizes the importance of pre-existing downstream cyclones in the development of cyclones associated with surface wind extremes across the hemispheres, previously documented for the North Atlantic (Stanković et al, 2024).”

   We feel like adding it once again at the beginning could become too repetitive.

   When it comes to another instance mentioned (previous Lines 300-303), we completely agree with you. We have now clarified that the result is a generalization on Page 12, lines 358-361:

   “Simply put, a "large-scale recipe" for generating cyclones with the largest surface wind extremes is similar in all storm track regions. The recipe involves the presence of a pre-existing downstream cyclone situated poleward and eastward of an extreme-causing cyclone (as previously found for the North Atlantic in Stanković et al., 2024), whose deepening rate controls how large a surface wind extreme can be achieved.”

10. Line 315: I think rather than saying when the jet core “weakens” it should say when the jet core “is weaker”.

    Corrected – thank you!

11. Line 339-340: This statement that it is unclear if the model can represent the storms is a key point. I think it would be good to either do the tracking and look at the individual storms in the model, or explain fully why this is not

possible. The model may be quite low resolution, but some tracking algorithms (e.g. the Hodges method) track on T42 vorticity and so it should be possible. You find that the model can represent aspects of the extreme wind climatology, therefore we hope that this is due to the storms that are present in the model. It would be beneficial to show this somehow.

We agree with you and we have put more attention on the cyclone tracking in the model as a response to your comment. We have focused our attention on tracking top 100 extremes in ISCA. We have identified top 100 days with the highest wind severity by repeating the same procedure as in ERA5. From there, we have identified cyclones and tracked them by simply following the absolute MSLP minima back in time, up to t = - 2 d (we repeated the "manual" storm tracking procedure as in Stanković et al., 2024). We have then made composites of MSLP anomalies for top 100 extremes in each basin in the 3 ISCA experiments we have further analyzed.

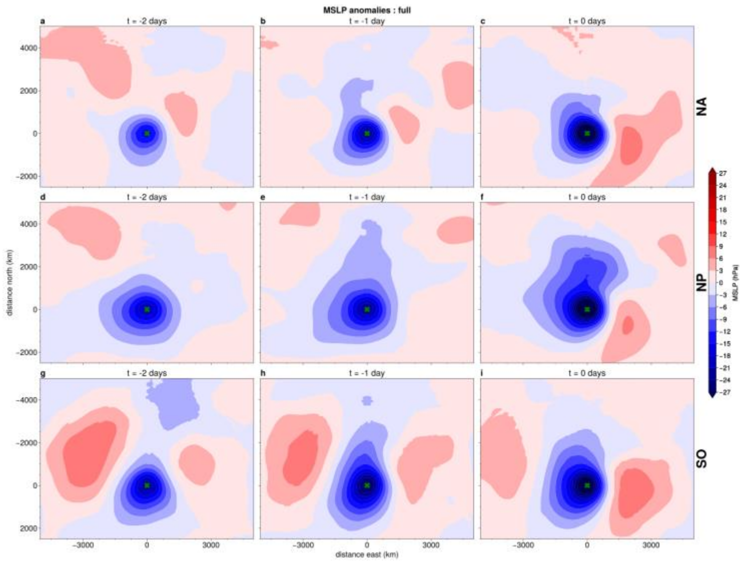
These composites shown below are now a part of the Supplementary Material. As can be seen from the composites for the Full experiment, the signal of pre-existing cyclones is weak.

We have commented these new results at several instances in the new version of the manuscript. First, in the Results section on Page 11, lines 338-346:

"Inasmuch as ISCA shows a strong positive correlation between basin-wide extremes in EGR and near-surface winds across the storm tracks, we note that the evolution of ISCA's top 100 most extreme cyclones is not demonstrably connected with pre-existing cyclones. We find the top 100 extremes in the three selected ISCA experiments in the same way as in ERA5 (by ranking the days after applying Eq. 1) and track the cyclones by connecting the MSLP minima across time-steps (i.e., tracking as in Stanković et al., 2024). Although the composites of MSLP anomalies (shown in the Supplementary Material) do show the presence of negative MSLP anomalies downstream of top 100 extremes in the Full experiment, these anomalies are weak, spatially limited and somewhat differently organized between the basins. Therefore, even though ISCA is able to reproduce the climatological correlation between extremes in EGR and near-surface winds, the evolution of the *individual* most extreme cyclones in ISCA differs from those in ERA5."

Further, we have reflected on significance of these results in the Summary and Discussion section on Page 13, line 407-411:

"Further, 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."



Composite MSLP anomalies in ISCA's Full experiment for 100 extremes in the NA (the first row), NP (the second row) and SO (the third row).