## Authors' response to
## 'Comment on egusphere-2024-3435', Anonymous Referee #2, 16 Dec 2024

We thank the referee for taking the time to review our manuscript. We believe the suggestions made by the referee greatly improved our manuscript. Below you will find the referee's comments in **bold**, our replies in blue, and *italics* for the text that has been modified/added to the manuscript. The additions are highlighted in turquoise in the revised manuscript.

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**The authors evaluate how extratropical transition (ET) in the North Atlantic will change with climate change using high-resolution regional climate simulations. They find a decrease in the number of ET events associated with a decrease in the number of tropical cyclones, with no change in the ratio of cyclone that undergo ET. They show that there is a compensation between more intensification from surface latent heat fluxes but a weakening baroclinic instability. They show a shift in the region of ET but no significant change in the average latitude. This is an excellent and well written paper about an interesting area of research. An excellent paper to have in this journal. I have some minor corrections and clarifications below.**

Thanks for your encouraging words and appreciation of our manuscript.

**L106 - Is there a missing reference here? Otherwise, you need to show this evaluation in a supplement.**

Yes, the reviewer is correct, we forgot to insert the reference: we have now added it. The model evaluation is available in Ingrosso and Pausata (2024).

**L186 - Is the weight of each layer the mass?**

The weight of each layer is calculated as the ratio of the difference between the upper-bound pressure and the lower-bound pressure of the layer to the difference between the upper-bound pressure and the lower-bound pressure of the entire column. We have included this definition of the weight in the manuscript.

**L203 - What do you mean by "i.e. upper" in this paragraph?**

We meant upper troposphere corresponding to the 600-300 hPa layer. We have clarified the definition of upper and lower troposphere in the manuscript. It now reads:

"*The lower troposphere corresponds to the 900-600 hPa layer while the upper troposphere corresponds to the 600-300 hPa layer*".

**L218 - What do you do with cyclones that are diagnosed as having an onset of ET but not completing ET? The paper by Sarro and Evans (2022) (https://doi.org/10.1175/MWR-D-22-0088.1) would be good to reference here. The "instant warm seclusion" they describe, where the cyclones undergo ET but are always warm core, could be relevant.**

Our study includes all TCs that have started an ET. Therefore, instant-warm seclusions, as well as transitioning storms that have not completed their transition within the regional domain are accounted for. Indeed, we have highlighted in the discussion the difficulties of the Cyclone Phase Space methodology in identifying ET completion. We have now clarified this aspect in the methodology and add the reference you have suggested. It now reads:

*"Therefore, all TCs that have started an ET are included in our study, including instant-warm seclusions (Sarro & Evans, 2022), as well as transitioning storms that have not completed their transition within the regional domain."*

**L261 - I'm actually surprised at how close this is. I would have thought that IBTrACS underestimates ET due to reporting biases. Could you comment on this?**

Indeed, there can be reporting biases as mentioned by the reviewer: extratropical transitions in IBTrACS are determined subjectively by various forecasters based on real-time observational data (Zarzycki et al., 2017).

Several studies have explored the topic of ET ratio simulation in different basins over the past years using the CPS methodology with different models, resolutions or reanalyses. The simulated ET ratios that we have summarized in Table R1 and added to the manuscript, are highly diverse and are generally higher than the observations (Hart & Evans, 2001).

Bieli et al. (2019) used JRA-55 and ERA-Interim whereas Zarzycki et al. (2017) used two reanalysis products, ERA-Interim and CFSR, combined with two climate models, CAM-55 et CAM-28. The latter study highlights the importance of the resolution with a 9% increase in the mean annual ET ratio with a higher resolution. Liu et al. (2017) used two reanalysis products, CFRS and JRA-55, combined with two climate models, FLOR et FLOR-FA, for which the SST is artificially corrected through flux-adjustment. This correction leads to a better representation of the ET ratio. Studholme et al. (2015) found a very high mean annual ET ratio (68%), this finding being explained by the simulation of longer tracks, enabling the ET to occur.

The simulated ET ratios that we have summarized in Table R1 and added to the manuscript, are highly diverse and are generally higher than the observations (Hart & Evans, 2001).

However, we can point out that Bieli and al. (2019) or Zarzycki et al. (2017) have also simulated mean annual ET ratios which are close to the observations. In our paper, the ET ratio found in the present-day simulation is 42.7%. However, this value takes into account the adjustments made to the CPS method, as detailed in the Methodology section (L215-216). Indeed, we noticed that for certain tracks, some storms could begin to acquire extratropical characteristics (asymmetry or a cold core) before reverting to tropical cyclones. These "false" transitions were therefore excluded from the transitions. It is important to point out that if another transition occurs, the storm will be considered among the transitioning storms.

After accounting for these "false" transitions, the transition rate decreased from 68.5% (close to the findings of Studholme et al., 2015) to 42.7%.

| Author(s) | Mean ET fraction | Method/data for tracking ETs |
|---|---|---|
| **Hart & Evans (2001)** | 46% | NHC best track labels |
| **Studholme et al. (2015)** | 68% | CPS and k-means clustering, storms tracked in ECMWF operational analysis |
| **Zarzycki et al. (2016) -1** | 55% | CPS, storms tracked in ERA-Interim |
| **Zarzycki et al. (2016) - 2** | 50% | CPS, storms tracked in CFSR |
| **Zarzycki et al. (2016) - 3** | 49% | CPS, storms tracked in CAM-28 |
| **Zarzycki et al. (2016) - 4** | 40% | CPS, storms tracked in CAM-55 |
| **Liu et al. (2017) - 1** | 56% | CPS, storms tracked in CFRS |
| **Liu et al. (2017) - 2** | 50% | CPS, storms tracked in JRA-55 |
| **Liu et al. (2017) - 3** | 57% | CPS, storms tracked in FLOR-FA |
| **Liu et al. (2017) - 4** | 31% | CPS, storms tracked in FLOR |
| **Bieli et al. (2019) - 1** | 47% | CPS, storms tracked in JRA-55 |
| **Bieli et al. (2019) - 2** | 54% | CPS, storms tracked in ERA-Interim |

**Table R1**: ET ratios in the scientific literature

We have clarified this point in the manuscript section 2.8.

**L292 - Is the latitude of minimum pressure dependent on ET? Do you count the minimum post ET or only prior?**

For each storm track, we identified the latitude corresponding to the minimum pressure. Figure 4a (on the left-hand side) shows the latitudes of minimum pressures for all storms while Figure 4b (on the right-hand side) isolates only those that undergo a transition. As a result, the latitude of minimum pressure is not inherently dependent on ET: it can occur either before or after transition. The goal of this analysis was to determine whether storms that undergo ET reach their deepest pressure level further north than those which do not.

We have clarified this point in the manuscript. It now reads:

"*Therefore, the latitude of the minimum pressure is not inherently dependent on ET: it can occur either before or after transition.*"

**Figs 2,3,4 - It would be good to also include IBTrACS on these figures as you did with figure 1, to give some idea of how close the model is to these observations (accepting that they can be biased)**

Thank you for this excellent suggestion. To avoid overcrowding the main figures, we created instead an Appendix to further compare IBTrACS to model simulations. The Appendix comprises five additional figures and two additional tables. We also added a short analysis for each of the latter figures and tables.

**Fig 5 - The Eady growth rate is shown at 200hPa, but earlier you only describe the calculation of Eady growth rate at 500hPa. Also, it is confusing that you say you use data at 400 and 500hPa to get the Eady growth rate at 500hPa. Would this not be better described as the Eady growth rate over that layer or at 450hPa assuming you are using 1st order differences.**

There is little literature that provides an explicit formula for the calculation of the Eady growth rate.

The calculation of the Eady growth rate involves computing the first derivatives of the wind velocity and the potential temperature. For the Eady Growth rate at 500 hPa, we use a forward difference scheme using the 400 hPa and 500 hPa values.

Similarly, for the Eady Growth Rate at 200 hPa - where our goal is to assess the baroclinicity in the upper troposphere - we use a backward difference scheme using the 300 hPa and 200 hPa values. A forward scheme in this case would have required using the 100 hPa values, introducing stratospheric influences, which we aimed to avoid.

We have clarified the computations of the Eady Growth Rate in the manuscript. It now reads:

"*In this study, we mainly focused on mid-troposphere baroclinicity and, therefore, computed the EGR at 500 hPa with a forward scheme, using the geopotential heights, humidity, meridional and zonal wind speeds, and temperatures at 400 hPa and 500 hPa*"

"*To assess the baroclinicity in the upper troposphere, we computed the EGR at 200 hPa with a backward scheme we use a backward difference scheme using the 300 hPa and 200 hPa values. A forward scheme in this case would have required using the 100 hPa values, introducing stratospheric influences, which we aimed to avoid*."

**L333 - The description of this weighting is slightly confusing. The monthly TC number is already in the ET ratio, so cancels out in the weighting and you would be left with the number of ET events in that month divided by the total number of TCs in the year. Is that correct?**

Yes, you are indeed correct. We have changed the manuscript accordingly and it now reads:

*"This indicator is calculated as follows: for each year, the ET ratio is the number of ET events divided by the total number of TCs and then averaged over 30 years."*

**L352 - "Indeed, TCs that are most likely to undergo ET need to sustain a minimum energy level at middle latitudes". This sounds reasonable, but I was wondering if anyone has actually shown this. Can you add a reference?**

We have added the reference of Hart & Evans (2001). Indeed, they stated "This suggests that tropical systems that are more intense in the tropical phase are able to survive for a greater period of time in the non-supportive region (between 960-hPa MPI and σ=0.25). Weaker tropical systems are able to intensify after transition, if they can quickly enter a supportive baroclinic environment after leaving the unsupportive tropical environment." They also proved this assertion when analyzing the seasonal cycle of ETs. Indeed, they found that a high distance between the 960-hPa MPI and a baroclinic zone explains the decreased transition probability found in June and July. On the contrary, transitions probabilities in August-September are found higher and can be partly explained by a lower distance between the 960-hPa MPI line and the baroclinic zone.

**Fig 10 - Can you add some indication of statistical significance to the pattern shown, either to the figure or the discussion**

This figure was obtained using the following methodology: for each grid cell, the algorithm calculated the number of ET onsets (over 30 years) occurring in this specific grid cell relative to the total number of ET onsets in the entire spatial domain (over 30 years). Then, spatial smoothing was applied using a Gaussian kernel. Since this method produces only one value per grid cell for both present-day and future-climate simulations — each already incorporating the full spatial and temporal information—it does not provide multiple independent samples that would allow for standard statistical significance testing. As a result, conventional statistical tests cannot be applied to assess the significance of the patterns shown in the figure.

**L383 - low transitioning -> slow transitioning?**

Thanks for pointing it out, we have now corrected it.

**L298 and L408 - You have used the term "available potential energy" interchangeably with "eady growth rate". While they are related, they are not the same. It would be better just to say Eady growth rate in the text as that is what is shown in the figures.**

Yes, we have modified the manuscript accordingly to address your comment and use Eady growth rate instead of available potential energy.

**L457 - Decrease -> weakening**

Thank you for the suggestion. We changed it as you suggested.

**L480 - I think this paragraph could be split into multiple paragraphs. It's quite long and does discuss different things.**

Thank you for pointing this out, we have modified the manuscript accordingly to address your comment.

**The code/data availability needs improvement. You could just write something about where the model data is stored, but I think the tracks you have generated should be made openly available which should be easy enough to do with zenodo. Similarly the code for the data analysis and figures could be uploaded to a zenodo repository.**

Upon acceptance, we can provide the TC tracks and the code and data for most figures on a zenodo repository.