

# Author's response regarding the second round of revisions of paper egusphere-2025-2314

## Relating extratropical atmospheric heat transport to cyclone life cycle characteristics and numbers in Southern Hemispheric winter

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February 24, 2026

We thank reviewers 1 and 3 for their second round of constructive comments and suggestions, which have allowed us to further improve the clarity of the manuscript. As described in detail below, in this second round of revisions we revised the transitions between the result sections, fine-tuned the cyclone track selection to make the result sections more consistent, comparable, and robust, and refined the introduction and conclusion sections to make our arguments more understandable. While one result becomes more nuanced, switching to more robust intensity and intensification metrics (that had previously been included only in the scope of sensitivity analyses) generally induces few qualitative changes. We adopted a more consistent use of wording to refer to heat transport terms and simplified methodological and result-oriented explanations within several paragraphs. Moreover, we decided that repeating these analyses for the NH is proposed as a future research direction because it is beyond the scope of our study.

The specific comments of the two referees are addressed below. These are listed in **blue** whereas our replies are shown in **black**. The line numbers of and figure references within the revised sentences refer to the revised version (not the track change file).

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### Reviewer 1

The authors revised the manuscript to address the concerns from the reviewers. My greatest concern was the readability of the manuscript, and it has been improved. The scientific results are understandable. However, the presentation and readability could still be improved, and the analyses could be better organized compared to other papers in this journal. Specifically, the connection between the three result sections can be enhanced, and readability can be improved by removing some details that may hurt the flow. Below I list some remaining concerns and suggestions for revision.

### Major comments

**Reviewer Comment 1.1** — There are three large sections in the paper. These three sections can be better connected.

1. For example, the analyses in Sect. 3 can be used to justify the flux attribution method, which is used in sections 4 and 5. Where does the flux threshold roughly locate, if put on Figures 4 and 5? This will demonstrate that the flux attribution method is better than simply using cyclone masks.

**Reply:** Thank you for your comment. In the revised version, we further improved the connection between Sects. 3 and 4 by motivating the following section with the results above:

L331: “Having found that the intensification rate strongly influences the local MSE flux, we further aim to quantify to what extent the different life cycle characteristics [...] modulate the zonally integrated MSE flux. As the previous composites are only snapshots during the life cycle, the guiding question is whether the intensification rate also influences the zonally integrated MSE flux integrated over the entire life cycle [...]”.

We considered your suggestion of adding an indication of the flux threshold to the composites, but this is not as simple as it might seem because the flux threshold depends on latitude and extratropical cyclones of all latitudes in the SH are included in the composite. Moreover, the composites in Sect. 3 do not show the vertically integrated MSE fluxes used for the flux attribution. Instead, at this point we again motivate the use of the attribution method with the previous composite results: L336: “As suggested by Figs. 4–6, MSE flux frequently occurs outside of the SLP-based cyclone masks [...]. Thus, in this section we make use of the newly introduced MSE flux attribution method.” The fact that the flux attribution method performs better than the SLP-based approach is then demonstrated further below in the text.

2. In Figures 3 and 4, cyclones with the deepest minimum and strongest intensification are defined as the top 10%. In Figure 8, the authors investigate the strongest and weakest 200 cyclones. Then in Figure 10, ‘strong’ refers to the top 25%. The definition varies from section to section, making it difficult to perceive the analysis as systematic. Having consistency throughout sections will improve the flow of the analyses. This is something I pointed out earlier, but has not been addressed.

**Reply:** To improve the connectedness and clarity of the three result sections, the selection of cyclone tracks was revised and made consistent across all sections. Following your suggestion, the results across the different sections were made more relatable to each other by adopting the consistent definition of strong vs. weak cyclones as above the 75th or below the 25th climatological percentile (previously chosen only in Sect. 5), respectively. Further in alignment with reviewer comment 3.9, we chose to change the metric to determine cyclone intensity and intensification rate from absolute SLP and temporal changes therein to SLP anomalies from the climatology and Bergeron units, respectively. These are more robust metrics (e.g., Sanders and Gyakum, 1980; Corn er et al., 2025) when comparing cyclones across a wide range of latitudes. These adaptations imply the changes listed below:

- Sect. 3: The MSE flux evolution along the life cycle in Fig. 3 is practically unaffected by these methodological changes because of compensating effects.
- Sect. 4: The switch to the more robust metrics renders most of the discussion paragraph in the previous L367-375 obsolete. In Fig. 8, the samples related to the most intense and most strongly intensifying cyclones become more alike but our key conclusion remains valid. A more nuanced framing based on the revised results is adopted in the transition to the Sect. 5, the summary, and the abstract.
- Sect. 5: Consistent with the changes in Sect. 4, the change to the more robust metrics leads to a more similar seasonal MSE flux contribution by the strongly intensifying and intense cyclones (both 47 % instead of the previous 43 and 47 %, respectively). The change in the intensity metric leads to changes in the seasonal relationships, in particular for the overall transient MSE flux. The corresponding description of Fig. 10 is adapted accordingly. As these results remain sensitive to the decomposition method, this does not qualitatively affect our key conclusion that the most intense and strongly intensifying cyclones do not modulate the seasonal mean MSE flux. Finally, the correlation between the number of all cyclones and total heat transport is not qualitatively affected.

3. In sections 3 and 4, analyses of strongly intensifying and strong cyclones are emphasized. However, in Sect. 5, categorizations by intensification and intensity result in no robust relationships between heat transport and cyclone numbers (lines 442-444). The paper starts as if strongly intensifying and strong cyclones would be important, but in the end, they are not. It would be better if this disconnect could be reconciled. More broadly, the robust conclusion from Sect. 5 is that cyclone numbers and total atmospheric heat transport are correlated (Fig. 11). This conclusion does not rely on flux attribution or categorization by intensity or intensification rate, which makes it not synergistic with the conclusions from the previous sections.

**Reply:** It is our intention to show that cyclone intensity and intensification rate are important for the (zonally integrated) MSE flux instantaneously but their contributions are masked on the seasonal scale. This twist in the storyline conveys an important message. To improve the transition between Sects. 4 and 5, we clarified that

L396: “Given that large transient MSE fluxes can be attributed to strongly intensifying and intense cyclones, in this section we test the hypothesis that the seasonal number of these storms relates to the seasonal mean zonally integrated transient MSE flux.”

We also revised this transition to motivate the examination of the total MSE transport:

L398: “To ultimately relate cyclone numbers to the zonally integrated heat budget, we also inspect the relationship between cyclone numbers and the total (i.e., not just the transient) MSE flux. ”

Very importantly, the final analysis does indeed rely on the flux attribution method for counting the cyclones at latitudes where they transport heat (cf. L453).

**Reviewer Comment 1.2** — There are details and discussions that can be neglected, or don't seem to be too important.

1. Is Equation 1 used in the main analyses? If not, it can be removed.

**Reply:** Thanks for your question on the relevance of Earth’s heat budget for our study. We considered excluding this equation but chose to keep it in the methods section to remind the reader that the MSE flux we are inspecting is one of the three main terms in the atmospheric heat budget. Moreover, references to the heat budget throughout the manuscript are less abstract when the corresponding equation is provided for clarification.

2. Lines 35 to 55 is a long single paragraph containing different key ideas. Lines 35-39 can be a different paragraph from the rest or be removed.

**Reply:** We revised the introduction and avoided lengthy paragraphs.

3. Lines 270 to 275. Is this detail necessary for the whole paper?

**Reply:** In our opinion, it is absolutely worth discussing the negative fluxes along the cyclone life cycle as they may be of interest for dynamicists. To clearly disclaim that this section goes into a detail that is not part of the main storyline, we rephrased the first sentence to L286: “To briefly shed some light on the negative flux anomalies, consideration is given to composite mean meridional winds [...]”.

4. Lines 288-289. This seems like an unnecessary detail.

**Reply:** This sentence was removed.

5. Lines 472-474. This seems like an unnecessary detail.

**Reply:** This sentence contrasts one of our key results with the main finding of a previous study.

6. Lines 521-526. The authors discuss in one sentence to remove stratospheric contributions, but in the next sentence discuss that stratospheric contributions are already small. Please revise.

**Reply:** Thanks for pointing out that this paragraph was confusing. We revised this section, which involved re-ordering these arguments.

**Reviewer Comment 1.3** — Some unclear presentations and discussions can be improved.

1. Lines 291-293. How can we say that the cold sector is broader than the warm sector from the figures?

**Reply:** This paragraph was revised to improve clarity on this aspect.

2. Descriptions around lines 345-354 (Figs. 8c and d) are difficult to follow.

**Reply:** We overhauled this paragraph by making the discussion of the figure panels easier to follow while still keeping it concise.

3. Lines 301-302. This seems like an overstatement according to Fig. 6c.

**Reply:** We agree and replaced “almost vanishes” with “is strongly reduced”.

## Minor comments

**Reviewer Comment 1.4** — Line 62: Please clarify what are ‘signals’.

**Reply 1.4:** We clarified in L66 that “[...] it was found that local flux extremes arise from a constructive interference of synoptic and planetary waves in MSE and the meridional wind (Messori and Czaja, 2014).”

**Reviewer Comment 1.5** — Line 266: hPa is missing after 850.

**Reply 1.5:** Fixed.

**Reviewer Comment 1.6** — Line 319: Is this correlation calculated by taking each year as an independent sample?

**Reply 1.6:** We added in L344 that “The correlation is calculated assuming that the winter seasons are independent of each other.”

**Reviewer Comment 1.7** — Line 380: ‘extent’?

**Reply 1.7:** Fixed.

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## Reviewer 3

### Summary

I appreciate the authors' efforts in addressing my previous comments and revising the manuscript accordingly. The current version represents a significant improvement over the original submission. However, I regret to say that I am still unable to recommend this manuscript for publication. While the presentation of results has improved, the overall clarity and readability of the manuscript remain critically insufficient. Below, I outline several major issues that, in my view, require careful attention before the manuscript can be considered for acceptance. I continue to believe that this study is of interest to readers of WCD, particularly as it seeks to bridge the gap between synoptic time scales and climate. Nevertheless, substantial revisions are necessary to ensure that the work is understandable and properly contextualised within the existing body of research on this topic.

### Major Issues

**Reviewer Comment 3.1** — In the first research question, you mention MSE flux; in the second, you refer to “heat transport”; and in the third, you return to MSE. How are these two terms related? This connection should be explained in the introduction; otherwise, the second research question appears isolated from the others.

**Reply 3.1:** We regret that the different wording for MSE flux caused confusion. Indeed, heat transport refers to MSE flux. We adopted a more consistent use of scientific terms and stuck to the use of ‘MSE flux’ to improve precision and understandability.

**Reviewer Comment 3.2** — The paper focuses on the Southern Hemisphere, and all references are to the Southern Hemisphere. Why include the Northern Hemisphere in Figure 1? It is not discussed in the main text, which creates confusion, especially since you state that NH data are used. Consider removing the Northern Hemisphere from the figure and from line 108 where you introduce the data.

**Reply 3.2:** We excluded the Northern Hemisphere from Fig. 1 and adapted the description of the dataset accordingly in L112: “[...] using six-hourly data of the years 1981 to 2021 during the winter season (i.e., JJA in the SH)”.

**Reviewer Comment 3.3** — While there is no perfect way to define eddies, introducing three different definitions seems excessive. It would be better to select the definition most appropriate for this context and justify that choice. If your results depend heavily on the chosen definition, this raises concerns about robustness.

**Reply 3.3:** In our opinion, there is no most appropriate definition of eddy MSE flux and comparing the three methods is a key feature of this study, showing that there is no single most robust definition. Past studies often relied on a single decomposition, so we believe it is important to

highlight the sensitivity of this choice in our study. Qualitatively, the results of Sects. 3 and 4 do not depend on the decomposition method, while there is higher sensitivity in Sect. 5. This is explicitly mentioned, for instance, in L325: “[...] it is worth pointing out that the findings regarding the horizontal structure of transient MSE fluxes also apply to vertically integrated fluxes (and fluxes calculated using different decompositions [...])”, or L355, L389, and L514.

It is a key finding of our study, however, that the relationship between the number of intense cyclones and MSE flux on a seasonal scale (Sect. 5) depends on the eddy-mean decomposition method. This is discussed in L437: “As indicated, these relationships are specific to the MA framework. It is important to point out that the correlations vary substantially with flux attribution percentile and flux decomposition method” or L16, L444, L461, and L514.

Furthermore, L462: “In contrast to the transient flux, the seasonal mean of the total zonally integrated MSE flux at 50° S exhibits a more robust correlation of 0.39 – 0.68 with the number of all cyclones passing that latitude.”

In sum, using three different decomposition methods enhances the robustness of the results in Sects. 3 and 4 and avoids an over-interpretation of correlations explored in Sect. 5.

**Reviewer Comment 3.4** — Are five appendices necessary? Much of the content seems more suitable for supplementary material, as it is not essential for understanding the main results. Additionally, Table A1 (list of acronyms) should be removed, as it is uncommon in journal articles and more appropriate for theses or long review papers.

**Reply 3.4:** All appendices were moved to the supplementary file.

**Reviewer Comment 3.5** — Italicised text is overused. Important points should stand out naturally through clear writing, not through font styling, which does not improve readability.

**Reply 3.5:** Italicization was abandoned throughout the manuscript and several sentences were reformulated accordingly.

**Reviewer Comment 3.6** — Merge the three subsections in the conclusion into one cohesive section. Summarise results, contextualise them within previous literature, and highlight methodological caveats and remaining/new research questions.

**Reply 3.6:** We refined the subsection titles to provide clearer guidance through this section. In line with another reviewer comment, we adapted the final section such that limitations of our findings are more naturally followed by future research opportunities. We think that having three subsections makes it easier to navigate through this part of the manuscript.

**Reviewer Comment 3.7** — Up to line 65, the meaning of “life cycle characteristics” is unclear. Does it refer to duration, travel distance, latitudinal migration, maximum intensity, etc.? Since this phrase appears in the title, clarify its meaning early — ideally before line 95.

**Reply 3.7:** Explaining this important term more clearly and earlier is a good idea. In the revised version, we introduce the cyclone life cycle and one of its key characteristics in the first two sentences of the manuscript.

**Reviewer Comment 3.8** — The description of how tracks are filtered remains unclear. If you exclude cyclones whose maximum pressure drop over 24h is less than 0, then none would be excluded. Please rephrase this passage clearly, as it is crucial for understanding your results.

**Reply 3.8:** Thank you for pointing this out. We improved the description of the intensification metric and wrote a clearer description of which tracks we aim to discard.

**Reviewer Comment 3.9** — Did you test this and decide not to include it (lines 367–370)? If so, state “not shown.” If it is based on cited references, specify that it is speculative. Otherwise, the robustness of your results is questionable. Ideally, test this explicitly.

**Reply 3.9:** Thanks for your comment. This paragraph has become obsolete as we chose to switch to different intensity and intensification metrics (see Reply 1.1.2). We had performed these tests ourselves before the initial submission.

**Minor Issues (list is not exhaustive, consider a deeper read-through before re-submitting)**

**Reviewer Comment 3.10** — Line 18: “details of the eddy-mean decomposition” is vague; clarify that it refers to the method of decomposing eddy and mean contributions.

**Reply 3.10:** We clarified in L17 that “[...] the relationship between their seasonal number and the seasonal mean poleward transient eddy MSE flux is sensitive to the choice of the eddy-mean decomposition method.”.

**Reviewer Comment 3.11** — Line 19 could be phrased more clearly.

**Reply 3.11:** We specified that cyclone characteristics refer to the cyclones intensities and intensification rates.

**Reviewer Comment 3.12** — Line 25: “near a land-sea contrast” sounds awkward; consider rephrasing.

**Reply 3.12:** Rephrased to L28: “[...] for instance in the Gulf Stream region which exhibits a land-sea temperature contrast[...]”.

**Reviewer Comment 3.13** — Line 32: The verb “explain” is overused; vary your wording.

**Reply 3.13:** Thank you for spotting this word repetition.

**Reviewer Comment 3.14** — Line 37: Change “This... fluxes make” to “These...” or make “fluxes” singular.

**Reply 3.14:** The corresponding paragraph was overhauled.

**Reviewer Comment 3.15** — Line 69: Replace “cyclone number” with “number of cyclones.”

**Reply 3.15:** Done. In general, when different numbers (e.g., the intense or strongly intensifying ones) are discussed we stick to the use of “cyclone number”.

**Reviewer Comment 3.16** — Line 83: Remove reference to “41 years of reanalysis data”; details belong in Methods.

**Reply 3.16:** Adapted as suggested.

**Reviewer Comment 3.17** — Line 86: Replace “a cyclone” with “each individual cyclone”; remove comma after “integrated.”

**Reply 3.17:** Thank you for this comment. Done as suggested.

**Reviewer Comment 3.18** — Lines 99–100: This sentence repeats the third research question; consider removing it.

**Reply 3.18:** The sentence was removed as the nuanced information that it added was considered irrelevant for the storyline.

**Reviewer Comment 3.19** — Line 115: Clarify that 7500 km is a chosen threshold; current phrasing suggests it is the only valid threshold.

**Reply 3.19:** We clarified in L118 that “Following Sprenger et al., 2017, the upper bound for the outermost closed contour is chosen to be 7500 km.”

**Reviewer Comment 3.20** — Lines 120–121: Revise for typos and clarity.

**Reply 3.20:** Thanks for spotting these typos. The explanation of the track post-processing was revised.

**Reviewer Comment 3.21** — Line 171: Sentence reads awkwardly, I guess you meant “the corresponding subscript”? Please rephrase.

**Reply 3.21:** Indeed. We rephrased this sentence.

**Reviewer Comment 3.22** — Line 289: Put “Messori et al. (2017)” in brackets.

**Reply 3.22:** Thanks for spotting this. Fixed.

**Reviewer Comment 3.23** — Line 380: Use “spatial extent” instead of “spatial extend.”

**Reply 3.23:** Fixed.

**Reviewer Comment 3.24** — Lines 409–411: Is this shown somewhere? Please clarify.

**Reply 3.24:** As indicated at the end of the sentence, this can be seen in Fig. 10a and d.

**Reviewer Comment 3.25** — Line 547: “applying there here developed” is unclear; please rephrase.

**Reply 3.25:** Rephrased to L577: “A follow-up study is planned of which the aim is to apply the newly introduced flux attribution method to idealized and fully-coupled climate simulations.”

## References

- Cornér, J., C. Bouvier, B. Doiteau, F. Pantillon, and V. A. Sinclair, 2025: Classification of North Atlantic and European extratropical cyclones using multiple measures of intensity. *Natural Hazards and Earth System Sciences*, **25** (1), 207–229, DOI: 10.5194/NHESS-25-207-2025.
- Sanders, F. and J. R. Gyakum, 1980: Synoptic-Dynamic Climatology of the “Bomb”. *Mon. Weather Rev.*, **108** (10), 1589–1606, DOI: 10.1175/1520-0493(1980)108<1589:SDCOT>2.0.CO;2.