Author's response for paper egusphere-2025-2314

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

by Jan Zibell, Alejandro Hermoso, Aaron Donohoe, and Sebastian Schemm

November 10, 2025

We thank the three reviewers for their detailed and constructive comments, which helped us to majorly improve the readability of our study and deliver our key messages more clearly.

We start our response with a summary of the main changes we implemented in the revised version: In the introduction, the main research gaps were emphasized more clearly to introduce the goals of the manuscript more effectively. All descriptions of methods were moved to the methods section, described more clearly, and the transitions between sections were reworked. We reduced redundancy and superfluous repetitions, rephrased explanations more clearly, streamlined the description of results in all sections, most notably in Section 3, and rewrote the discussion of sensitivity analyses more concisely to not distract from the main points (especially in Section 4). We condensed a discussion of the method sensitivities that extended from Section 4 to 6 within two paragraphs in Section 6 to improve the flow of the manuscript.

We further added short discussions related to reviewer and editor comments within the proper sections. Overall, the main text was shortened by more than 15 % and only brief clarifications within the appendix were added to address specific comments by the reviewers. Restructuring the manuscript also included removing a few figures from the main body of the manuscript and the appendix, and revising the figure captions. During the revision of the figures in the Appendix, it was noticed that a wrong version of Fig. E2 was included in the previous version which corresponded to a sensitivity test. As had been mentioned in the previous version, "Randomly selecting between the overlapping cyclones was also tested, but had no qualitative effect on the results.". No further analyses were added and it is important to mention that despite these substantial structural revisions the conclusions of the study remain unchanged.

In the following, we address in detail the comments of each referee. 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).

Reviewer 1

The authors investigate the relationship between extratropical cyclone properties and atmospheric heat transport in the Southern Hemisphere winter. To study this relationship, the authors utilize many different methodologies, including cyclone tracking, cyclone-centered composites, and quantifying the atmospheric heat transport. The authors find that a large fraction of cyclone-related eddy atmospheric heat transport is done by intense cyclones. The authors also find that the relationship between cyclone number and atmospheric heat transport is sensitive to the definition of the variables.

While I think this manuscript attempts to answer very interesting questions as proposed in the introduction, from the current version of the manuscript, it is difficult to evaluate whether those are sufficiently addressed. The amount of analyses included in the paper, including the various methods used here, is beyond what is typically presented in a single paper. Hence, delivering the key messages to the readers while not making them lost in the technical details and discussions is important. In the current version of the manuscript, there are many places that needs to be shortened or removed, even with five appendix sections. I am familiar with the methodological details and the Southern Hemisphere storm tracks, but it was not easy going through the manuscript. Considering that the paper will be read by a broader audience, I recommend that the authors majorly restructure the manuscript. Moreover, I have some comments on the interpretation of the scientific results.

Reply: We thank the reviewer for their thoughtful review. We appreciate that the research questions are considered interesting and acknowledge the justified concerns regarding the presentation of our analyses. The reviewer is right to question the length of this study which included many arguments written in a convoluted style. To facilitate the navigation across this manuscript, we streamlined the introduction towards the main research questions, revised the presentation of the methods which are no longer scattered across different sections, reduced unnecessary side tracks in the results sections, restructured and condensed the discussion of the results, and revised the key findings in Sect. 6 to present them in an even clearer way. The descriptions of technical details arising from sensitivity analyses have been considerably shortened and labeled more clearly as such, for instance,

- L332: “Qualitatively, the results are robust relative to the choice of the percentile and flux decomposition method (see Appendix D, Fig. D1).”
- L367: “A comprehensive sensitivity analysis ensures the robustness of the findings presented above. The results are neither sensitive to [...]”

Reviewer Comment 1.1 — There are details in the results section that are not new (introduction materials) and involve in-depth discussions (discussion materials). These are helpful, but they obscure the key messages the authors are trying to deliver. In the current manuscript, all the details of the figures are explained. I appreciate the author’s thoroughness, but some details can sometimes be neglected, for the sake of a bigger key message. Below, I will list parts of the result section that had particularly dense details to read.

1. Lines 253-260: The results are largely consistent with theoretical models of baroclinic instability. These descriptions can be shorter.
2. Lines 260-301: Please shorten these paragraphs so that the key message becomes clearer.
3. Lines 311-315: It seems that the underlying physics is the Clasius-Clapeyron equation. I also think these could be shorter.

4. Lines 316-323: The precipitation pattern is generally consistent with Lq flux. It was confusing to see the moisture budget brought in to explain the simple result.
5. Lines 353-370: The attribution methods were explained in the method section. It was clear why the authors chose to use the 'nearby flux' method over 'cyclone mask' method. If not comparing the two methods is not the main objective of this study, I suggest moving these (and Fig. 6) to the appendix.

Reply 1.1: Thank you for your suggestions where to shorten the text. Overall, we reduced the length of Section 3 from 126 to 77 lines. We thereby reduced the amount of details considerably while keeping the descriptions of dynamically interesting features and the main points relevant to the storyline.

1. We shortened the descriptions in this section and emphasized that these findings match our expectations from theory, for instance:
   - L239: "This evolution is broadly consistent with the baroclinic life cycle, in which transient eddy heat flux peaks as baroclinicity is eroded, before intensification terminates (Novak et al., 2015)"
   - L245: "This highlights the close relationship between transient MSE flux and the intensity of baroclinic growth"
   - L271: "Overall, the wind anomalies tilt westward with height during intensification and become more barotropic at mature stage[2] which agrees with baroclinic theory (Eady, 1949, Thorncroft et al., 1993)"
2. We majorly reduced the discussion of the asymmetry between warm and cold sectors. We only kept one point that we think is not obvious and worth mentioning to better understand methodological biases, namely (L289) "In the composites, the warm sector flux stands out more strongly, which could be related to the fact that compositing by the cyclone center implies a tendency to centering next to the warm sector. Since the warm sector narrows during the life cycle as the cold front catches up with the warm front, the fluxes in the warm sector are spatially more constrained than in the broader cold sector (Shapiro and Keyser, 1990)."
3. We shortened this paragraph as well.
4. Thank you for your suggestion on this paragraph. Indeed, the small offset between the maxima in precipitation and poleward moisture flux is not relevant for the main conclusions of our study. We shortened this paragraph.
5. Thank you for your feedback. In accordance with a suggestion by reviewer 3, the second research question stated in the introduction has been adapted to:

   L95: "How much of the zonally integrated heat transport can be attributed to cyclones, and how do their contributions differ according to their key life cycle characteristics such as intensification rate, intensity, and lifetime?"

In our opinion, the increased fraction of the cyclone-attributed flux (former Fig. 6) is an important result that may be overlooked if placed in the methods section. Thus, Section 4 starts by addressing the second research question directly:

L317: "The zonal integral of the seasonal mean cyclone-attributed transient MSE flux, denoted as $\langle v' m'^{cycl}_{MA} \rangle$, amounts to 1.65 PW at around 50° S (orange solid line in Fig. 7a)[3]. Thus, around 30 % of the overall transient MSE flux is attributed to cyclones (Fig. 7b), while the rest is attributed as non-cyclone related MSE flux."

We restructured the method section:

L176: "In this section it is described how local MSE fluxes are attributed to individual cyclones. Having computed cyclone masks from the SLP fields, the simplest approach is to spatially integrate the MSE fluxes over the cyclone mask at each timestep. Note, however, that climatologically the maximum transient MSE flux is located equatorward of the maximum in surface cyclone frequencies (Fig. 1a). [...] Thus, most of the transient MSE fluxes are expected to be excluded when spatially integrating fluxes only within the (SLP-based) cyclone masks."

The remaining paragraph in Section 4 connnects to both Fig. 1 and the results of the cyclone-centered composites:

L324: "Note that the transient MSE flux located within the SLP-based cyclone masks is less than 1 PW (black dashed line in Fig. 7a). This low fraction ($\approx$ 10 % at 50° S, Fig. 5b) is consistent with the fact that transient MSE flux peaks equatorward of the cyclone center along the trailing fronts. Accordingly, a low to negative correlation is found between the seasonally averaged total and transient MSE fluxes attributed to cyclones using this simpler approach (black line in Fig. 7c). Based on the overlap method, we find the peak of the cyclone-attributed MSE fluxes at around 50° S, and therefore much closer to the MSE flux maximum near 42° S, while the fluxes within the SLP-based cyclone masks much further poleward (62° S)."

Reviewer Comment 1.2 — One of the major results emphasized in section 5 is that strongly-intensifying cyclones account for a disproportionate amount of cyclone-related eddy AHT. I'm not sure if this is surprising as the authors describe it. The reasoning in the manuscript is that the strongly-intensifying cyclones account for 43%, similar to the moderate cyclones' contribution, although the former is half in number. But I'm not sure if the expectation here (i.e., a proportionate result) for the strongly-intensifying cyclones is to contribute 25%. This would mean that there is no relation between cyclone intensity (or intensification rate) and AHT. If one assumes that AHT and intensity (or intensification rate) are linearly correlated (a simple y=x relationship), the top 25% of cyclones would contribute 44% to AHT, very similar to the result in the paper. Since the number is expected from a linear relationship, from some perspective, this could just be a proportionate contribution. I would recommend that authors reassess their point. Additionally, the definition of strongly-intensifying or strong cyclones change from section 3 to section 5, please provide more justification for this change.

Reply 1.2: Thank you for your valuable comment. Indeed, we used the word 'disproportionately' to compare the accumulated transport to the frequency of cyclones. You are of course right to point out that a contribution well above 25 % should not be surprising. The percentage to expect, however, not only depends on the relationship between cyclone characteristic (e.g., intensity) and heat transport but also on the distribution of that characteristic. In the example you brought up, you implicitly assumed that the intensification rate or intensity of cyclones is equally distributed. This is certainly not the case, as intensities are somewhat normally distributed (whereas the distribution of intensification rates is strongly skewed). Let's write the atmospheric heat transport (AHT) accumulated by the bottom (weakest) 75% cyclones as $AHT_{accum}(I_{75}) = \int_0^{I_{75}} f_{AHT}(I) \cdot \rho(I) dI$, where I_{75} is the 75th percentile of intensity (or intensification) I, f_{AHT} is the function describing the relationship between AHT and the characteristic I, and $\rho(I)$ the distribution of I over a time span. For $\rho(I) \propto I$, we obtain the result you suggested. Approximating $\rho(I) \propto \sin^2(\pi I)$[1], which approximates a normal distribution while keeping the integration simple, we obtain $\frac{AHT_{accum}(I_{75})}{AHT_{accum}(I_{100})} \approx 0.63$. Thus, the 25% strongest cyclones contribute around 37% of the heat transport. This is below the 44% you suggest. It was assumed that $\rho(I) \propto I$. As shown in Fig. R1, this is not the case in the dataset. The median flux (orange lines) increases somewhat exponentially with cyclone intensity and is approximately linear with intensification, although it saturates for very high intensification rates. Thus, the fact that one obtains close to 44 % in the case of the intensification rate is due to compensating effects. A last assumption that goes into the above estimate is that the constant offset of the linear relationship is zero, although it is not a priori clear if cyclones of intensity close to zero should have vanishing fluxes.

Acknowledging your input that we should not expect a contribution of 25 %, we explicitly state that (L402) "cyclones contribute disproportionally compared to their frequency", see below.

To avoid introducing more figures and explanations (in accordance with feedback by reviewer 3), we decided not to add these relationships in detail but rather in more general terms. The complete paragraph reads:

L397: "The above results may suggest an almost linear relationship between intensification rate and transient MSE flux: In the case of a linear relationship, one would expect the cyclones above the 75th percentile to contribute approximately 44 % to the budget.[5] The flux indeed increases with cyclone intensification rate and intensity. However, for cyclones intensifying very rapidly the relationship is found to be non-linear, while it is rather exponential for intensity (not shown). Further, the intensities are not equally distributed but follow a (skewed) normal distribution for both characteristics. In sum, the identified fractions mean that the strongest cyclones contribute disproportionally compared to their frequency, which agrees with the conclusions in Messori and Czaja (2015)."

Regarding the definition of strong cyclones, in the method section we highlighted that (L218) "Choosing the 75th percentile (instead of the 90th, for instance) is found to be an adequate compromise between selecting only the strongest cyclones and including a large enough number of cyclones to counteract the event-to-event variability (see discussion in Appendix D)."

Reviewer Comment 1.3 — 3. Figure 11. Please provide the p-values of the linear correlation. If authors are using simple linear regression, R^2=rho^2, and additional information will be helpful to understand the significance of the relations.

Reply 1.3: Thank you for your good idea. We added the p-values to the panels, removed the explained variance, and briefly refer to them in the text. Both of the relationships that we discuss are statistically significant. Note, however, that these remain method-dependent as discussed in

[1]The π in the $\sin^2$ applies to integrating over I normalized from 0 to 1.

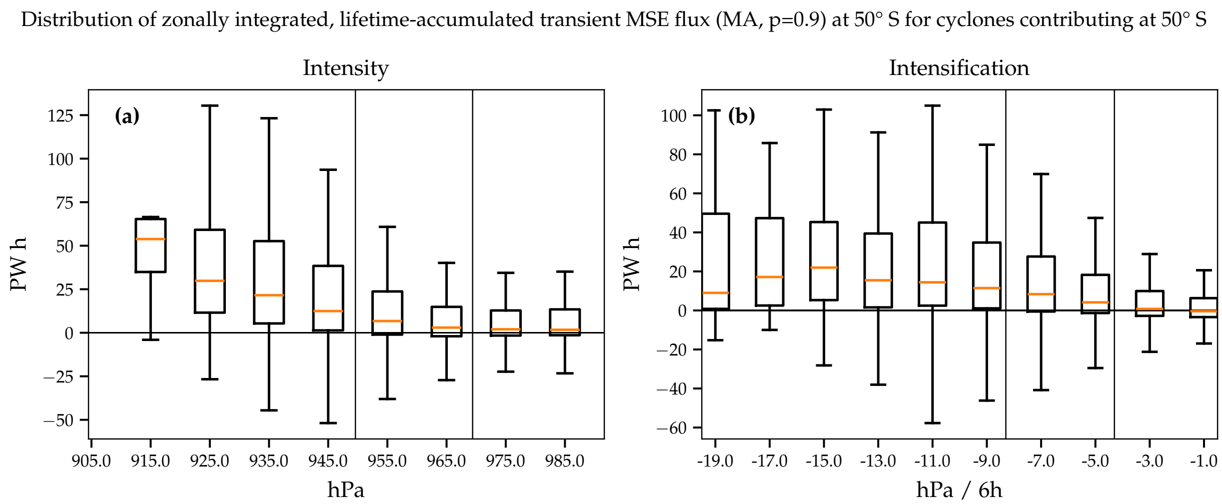

Figure R1: Lifetime-accumulated cyclone-attributed fluxes at 50° S based on all cyclones that contribute to flux at this latitude (units PW h). **(a)** The cyclones are sorted by their intensity as measured in absolute hPa. **(b)** As **(a)** but for intensification measured as the 6-hourly difference in minimum SLP (hPa $(6h)^{-1}$). Boxes indicate the inter-quartile range (IQR), whiskers 1.5 times the IQR, and yellow lines the medians. Fliers are not shown. Vertical black lines indicate the IQR of the life cycle characteristic on the x-axis.

detail in the text, such that we do not want put too much emphasis on statistical significance here. We added in L420:

"In both cases, the slope of the linear fit is significantly different from zero as indicated by the p-values on the panels (using a two-tailed test, Fig. 10a,f)"

Reviewer Comment 1.4 — 4. Figure 8. I suggest that Fig. 8 be turned into a bar chart or something similar. The text does not describe latitudinal structure, and it is hard to distinguish the lines.

Reply 1.4: Thank you for this suggestion. We agree that it was not very easy to distinguish the lines in the initial version. A bar chart for the attributed fluxes at a given latitude is a good suggestion for an alternative visualization. However, it is important to us to showcase the latitudinal distribution of the attributed fluxes and its dependence on the life cycle characteristics. We emphasized that

L343: "The fluxes of the most intense cyclones are located further poleward compared to the weakest cyclones (dotted lines in Fig. 8a). [...] The corresponding ratio in Fig. 8b (dotted line) thus increases towards the pole."

To facilitate distinguishing the individual lines, we increased the size of the figure to span the entire page. As we removed the former follow-up Fig. 9 (which was a repetition for a different cyclone subset) this does not increase the length of the manuscript.

Reviewer 2

Summary

This manuscript tries to evaluate and characterize the contribution of extratropical cyclones to the total poleward atmospheric heat transport, focusing on the Southern Hemisphere winter. This is done using ERA5 reanalysis data and identifying cyclones based on local minima of sea level pressure associated with eddies. The amount of poleward heat transport associated with the cyclone is evaluated using an attribution method, based on the overlap in longitude and latitude between the surface cyclone and the lower tropospheric poleward heat transport. The main findings are: (1) Poleward heat transport by cyclones maximizes during the intensification period. (2) Poleward heat transport is much stronger on the eastward side (warm sector) of the cyclone, compared to the westward side (cold sector). (3) Latent heat transport makes a higher portion of the total transport by the cyclone as the latitude decreases and exhibits a strong asymmetry between the warm and cold sectors. (4) Intense cyclones and strongly intensifying cyclones transport much more heat poleward during their life cycle compared to weak cyclones. (5) The total poleward eddy heat flux is not correlated with the number of intense cyclones in the interannual variability, when eddies are defined as deviations from the monthly mean, yet there is a positive correlation when eddies are defined using a high-pass filter.

Each of the results is examined carefully, while considering the sensitivity to the method of identifying eddies, to the attribution method and the percentile threshold chosen for that, to the choices of latitudes and pressure levels, to the focus on intense or intensifying cyclones and to the eddy lifetime.

The manuscript is well-written and the presentation is clear and pleasant to look at, while serving the message well. The motivation for each analysis presented is explained and the connection between the results and conclusions is clear.

I therefore recommend on publication of this manuscript in Weather and Climate dynamics. I think it would be of interest to the community studying Earth's energy budget and the role of extratropical cyclones in that budget.

I have a few minor comments elaborated below, relating mostly to a few sentences where I found it difficult to understand the intention. Also, I think it may be possible to shorten the manuscript a bit, without losing the important messages.

Reply: We thank the reviewer for their positive and encouraging feedback on our manuscript. As suggested, we shortened the manuscript by getting rid of side tracks and rephrasing without losing the important messages. We further addressed the other helpful comments and questions as outlined below.

Comments

Reviewer Comment 2.1 — Line 49: MSE includes also potential energy.

Reply 2.1: Thank you for spotting this inaccuracy, we modified this statement accordingly.

Reviewer Comment 2.2 — Lines 123-124: "tracks that are... weaker than 990 hPa... are discarded" – do you mean that the SLP is higher than 990 hPa?

Reply 2.2: Exactly. We rephrased the sentence to add more clarity (L120).

Reviewer Comment 2.3 — Line 161: delete "for" ("for to explore").

Reply 2.3: Thank you for pointing out this mistake.

Reviewer Comment 2.4 — Line 211: delete "4 and 5" (maybe this was supposed to refer to a certain section?).

Reply 2.4: Thank you for spotting this, we removed this typo.

Reviewer Comment 2.5 — Lines 216, 220 and figure 2: The use of different terms to describe the stages of the life cycle is confusing: mature stage (line 216), min. SLP (figure 2), intensification phase and deepening phase (line 220). It is not clear if the mature stage and min. SLP are the same and what is the difference between the intensification and deepening stages.

Reply 2.5: Thank you for bringing this up. We replaced 'min. SLP' with 'Mature stage' and added 'Intensification -' and 'Weakening phase' to Fig. 2 for more clarity. Although it should be clear that 'deepening phase' and 'intensification phase' are identical when using sea level pressure as an intensity metric, we changed most occurrences of 'deepening' to 'intensification' to avoid confusion.

Reviewer Comment 2.6 — Figure 3: The discussion of the figure in lines 240-252 mentions regions of negative MSE flux. I see there are blue regions in the figure, but it is difficult to identify them exactly and connect them to the description in the text. I suggest to add a zero contour to the figure, to highlight these regions.

Reply 2.6: Thank you for your suggestion. We agree and added a white zero contour in all composites.

Reviewer Comment 2.7 — Discussion of figure 8a,b in lines 381-385: The figure shows that sorting the cyclones by intensity makes the greatest difference for the MSE flux poleward of 50S, yet the authors say that this differentiator is equally important to the intensification. They argue that the high ratio between MSE flux by the largest and lowest intensity cyclones in high latitudes is due to SLP climatologically decreasing towards the pole. I don't how that explains this ratio. The MSE flux is by transient eddies, meaning that the climatology is subtracted from the total fields. What am I missing?

Reply 2.7: Thank you for your question. Indeed, the phrasing of being an 'equally important differentiator' is not precise and misleading. We generalized this statement to that intensity (L342) "also emerges to be a clear differentiator for accumulated transport".

Regarding the climatological decrease of SLP to the pole: transient eddy MSE fluxes are themselves not affected by SLP decreasing towards the pole (apart from changes in the vertical lower integration bound that are not relevant for this discussion). Moreover, you are right to say that the climatology of the velocity and MSE fields are effectively subtracted.

However, as we define cyclone intensity using SLP, by selecting the deepest (most intense) cyclones, the most intense cyclones tend to be located poleward of the weakest cyclones.

This motivated the analysis of cyclones within a narrower latitude band, for which the results are very similar. Thus, the climatological decrease towards the pole may increase the ratio between strong and weak cyclones but is not the main reason that it is high. Although we shortened this paragraph in the revised version, we kept this information in the text.

As indicated in the text, we also repeated this analysis by defining cyclone intensity using SLP anomalies from the climatological mean (as done in Cornér et al., 2025) and do not find qualitative differences that affect the conclusions in this study.

Reviewer Comment 2.8 — It would help to mention how the MSE flux in figure 8 is related to the MSE flux in figure 6. To me it was confusing that the total MSE flux by transient eddies in figure 6 is almost 2 PW, whereas in figure 8c it is around 0.1 PW. Is that because the results in figure 8 should be multiplied by the number of cyclones to get the result in figure 6? What is the average number of cyclones per season?

Reply 2.8: Thanks for raising this interesting question. The average seasonal number of cyclones is 341 in SH JJA. The average lifetime of a cyclone is 3.4 days or 13.5 six-hourly timesteps. Thus, at each timestep there are 12.5 (already postprocessed) cyclone tracks within the entire SH. At 50°S, the lifetime averaged flux of all cyclones (black long dashed line) is 0.122 PW. Multiplying these means (1.53 PW) approximately recovers the climatological median of the seasonal mean flux in the cyclones (1.65 PW, orange line). The distributions of lifetimes and cyclone-attributed fluxes are skewed, explaining the small discrepancy between the seasonal mean and the back-of-the-envelope estimate above.

In the manuscript, we added L354: "The average cyclone-attributed MSE flux of all SH cyclones, calculated at 50° S, is 0.12 PW (black dashed line in Fig. 8c). During each winter season, around 340 unique cyclones are identified. These have an average lifetime of 3.4 days, such that 12.5 cyclones are present on average[4]. The estimated 1.5 PW that they contribute altogether matches the seasonal mean flux shown in Fig. 7a."

With footnote 4: "340 cyclones / 92 d × 3.4 d"

Reviewer Comment 2.9 — Discussion of figure 8c,d in lines 386-394: there is no discussion of the differentiating by intensity (dotted lines).

Reply 2.9: Thank you for bringing this to our attention. We added a brief description of the ratio between the most and least intense cyclones:

L343: "The fluxes of the most intense cyclones are located further poleward compared to the weakest cyclones (dotted lines in Fig. 8a). [...] The corresponding ratio in Fig. 8b (dotted line) thus increases towards the pole."

Reviewer Comment 2.10 — Lines 395-396: It is not clear to me why using this subset makes the comparison fairer (relating to comment 7).

Reply 2.10: Thank you for your feedback. One aim of this paragraph, which we now labelled as "comprehensive sensitivity analysis" (L367), is to ensure that the climatological SLP decrease does not affect the conclusions. We found (L370) "that the latitudinal offset between the peaks of the attributed fluxes of the deepest and weakest cyclones is reduced when using SLP anomalies". Alternatively, we get to the same conclusion when not taking into account the large number of cyclones of all latitude bands but instead similar ones as measured by their geographical occurrence (L371): "this offset is also reduced when sub-selecting cyclones intensifying only within 50–60° S". Furthermore, we clarified that (L373) "Excluding tracks shorter than 3 d further ensures that weak, short-lived tracks do not distort the picture".

Reviewer Comment 2.11 — Lines 413-414: I find it difficult to understand the arguments in this sentence. It is not straightforward to see the connection to the correlation between poleward propagation of eddies and MSE flux.

Reply 2.11: Thank you for this feedback. We shortened this paragraph significantly. Looking at poleward propagation speed was motivated by previous research which found that this is the characteristic for which the poleward moisture fluxes attributed to cyclones are the largest (Sinclair and Dacre, 2019). To compare their findings with our MSE fluxes, we repeated the analysis. The point of this statement, which we by now moved to the last section, is that for poleward propagation speed the attributed MSE fluxes are not larger than for intensity.

L472: "Previous research has found that the poleward propagation speed (and not necessarily intensity) is the characteristic for which the poleward moisture fluxes within a fixed radius around the center are the largest (Sinclair and Dacre, 2019). For MSE fluxes in this study, the cyclones with fastest poleward propagation speed show comparable lifetime-averaged fluxes to those of the most intense cyclones at some latitudes but not consistently larger ones (not shown)."

Reviewer Comment 2.12 — Lines 502-503: I find it difficult to understand the explanation for the low correlation in figure 11d and the comparison between figures 11c and f.

Reply 2.12: Thanks again for this feedback. We revised the description of the results as follows:

L409: "It was found that when omitting the flux attribution and simply correlating the total number of cyclones with the mean transient MSE flux ($\langle[\overline{v'm'}_{\mathrm{MA}}]\rangle$), the correlation remains positive albeit weaker than the previous result based on the cyclone-attributed flux (cf. Fig. 10a and d). $\langle[\overline{v'm'}_{\mathrm{MA}}]\rangle$ is not correlated with the number of most rapidly intensifying cyclones (Fig.10e) but is correlated with the number of the most intense cyclones (Fig. 10f). The latter is larger compared to the case with flux attribution (c.f. Figs. 10c and f). Under the assumption that the most intense cyclones dominate the overall transient MSE flux, the decrease of the correlation from Fig. 10f to Fig. 10c could be an indication that the attribution method does not perfectly attribute MSE fluxes to cyclones. One possible explanation is that the attribution method attributes some fluxes of the most intense cyclones to neighboring weaker ones. This would be in line with the fact that the correlation of the cyclone-attributed fluxes with the overall cyclone number (Fig. 10a) is higher than with the intense ones (Fig. 10c). Over a season, more cyclones with MSE fluxes attributed independently of intensity would be expected to have more attributed MSE flux overall."

Reviewer Comment 2.13 — Line 505: should be Fig.E1b-h instead of Fig.E1b,h.

Reply 2.13: Thank you for your suggestion, but we think that this would not be the most precise figure referencing. The previous sentence relates to the correlations $\rho\left(\langle[\overline{v'm'}_{\mathrm{TE}}^{\mathrm{cycl}}]\rangle, n_{\mathrm{all}}\right)$ and $\rho\left(\langle[\overline{v'm'}_{\mathrm{TE}}]\rangle, n_{\mathrm{intense}}\right)$, which are depicted in Fig. E1, panels b and h, respectively. The different columns show the correlations for different n and the rows for different atmospheric heat transport (by eddies). In principle, referring to Fig. E1b-h instead of Fig. E1b,h would not be a false statement, but it would be less precise.

Reviewer Comment 2.14 — General comment to section 5: I appreciate the authors effort in examining the sensitivity of the results to various methods and parameters. However, this makes the paper a bit too long and heavy. I think it would not reduce the quality and completeness of the paper if some of the content is removed. To me figure D2 was useful, but other figures in the appendices (D1, D3-D6, E2) and some of the discussion in section 5 were too much loaded with details, which really make it difficult to follow. In general, I think that the effects of the method (defining eddies as deviation from the monthly mean, using a high-pass filter or considering zonal anomalies) and the choice of the percentile on the results can be explained qualitatively with less details. If the authors still prefer to keep the section as it is, that's also fine.

Reply 2.14: Thank you for your valuable input. We significantly revised Section 5 as illustrated in the comment above, but also the discussion on the method sensitivity was boiled down to what we think is essential for the analysis. The remaining discussion thereof was unified and shortened within Section 6 and we used fewer standalone mathematical symbols. Due to shortening of the main text, former Figs. D4 and D6 became obsolete and were removed. We also decided to remove former Fig. D3 since former Fig. D5 alone summarizes the method (in-)dependence quite satisfactorily. We acknowledge that Fig. E2 is loaded with a lot of information. As the important finding of the latitudinal dependence of the correlation between total heat transport and the number of all cyclones motivates exciting follow-up research, we decided to keep Fig. E2. Reducing the number of panels in this figure was considered an option but eventually deemed to not help clarify what is shown; as it is now, there is consistency throughout the paper in the arrangements of the panels.

Reviewer Comment 2.15 — Lines 619-620: I don't understand the argument in this sentence.

Reply 2.15: In accordance with feedback from the other reviewers, we chose to cut statements that derail from the main storyline. This statement was also among those.

Reviewer Comment 2.16 — Lines 641-643: I don't understand the argument in this sentence.

Reply 2.16: This argument was also cut out as it described a train of thought that is not relevant for this study.

Reviewer 3

Summary

This manuscript aims to assess the link between extratropical cyclones and moist static energy (MSE) transport in the Southern Hemisphere during winter. Using ERA5 reanalysis and methods that partition energy fluxes into transient and stationary components, the authors attribute deviations from the mean to cyclones and quantify their contribution to total eddy MSE flux. The study finds that local meridional eddy MSE fluxes peak during cyclone intensification, particularly in the warm sector, and that a significant portion of these fluxes lies outside traditional cyclone masks. By including these external fluxes, the cyclone-attributed share of poleward MSE transport increases to about 30%. The analysis shows that cyclone characteristics (e.g., intensity, intensification rate) strongly influence the magnitude of attributed fluxes, both locally and in zonal integrals. On a seasonal scale, the most intense 25% of cyclones contribute nearly 45% of the cyclone-attributed AHT, despite being less frequent, while cyclone count alone is not a reliable predictor of seasonal flux. These findings highlight the dominant role of strong, rapidly intensifying cyclones in shaping transient eddy heat transport, while also acknowledging contributions from non-cyclonic or broader atmospheric features.

Despite the relevance and potential impact of the study, the manuscript is rather difficult to read. The presentation suffers from an excessive number of figures and appendices, many of which are hard to compare and detract from the clarity of the main message. The language is at times inappropriate or imprecise, requiring multiple readings of several sentences to grasp their meaning. This significantly undermines the accessibility and effectiveness of the work. Additionally, the text often repeats itself, rephrasing the same concepts unnecessarily, while methods are sometimes embedded within the results and discussions occasionally stray into irrelevant territory. These structural and stylistic issues pose a major obstacle to understanding and appreciating the main findings of this study. While I do see the merits of this work, which could be of interest to readers of Weather and Climate Dynamics, I cannot recommend it for publication in its current form. A major revision and restructuring are necessary. Below, I provide some general comments, followed by specific line-by-line comments to help the authors address the issues outlined above.

Reply: We thank the reviewer for their constructive feedback and regret that the presentation in the initial version of the manuscript made reading it difficult. We greatly appreciate the detailed and extensive comments to restructure this manuscript and improve it. As suggested, we collected methodological descriptions in the methods section and reordered the Figures. In accordance with suggestions by reviewer 1, we reduced the length of the result sections, excluded one figure that added comparably little to the main storyline, and removed side thoughts or, if still included, labeled these as such, for instance: L358: "An intriguing side result is that the lifetime-averaged cyclone-related MSE flux is larger when computed using all cyclones than when using only the longest or shortest life cycles.". Moreover, we reduced repetitions, the use of expressions such as "In other words, ...", and revised the sentence structure in many instances.

The appendix was shortened by three figures, while we chose to keep 1) the table of abbreviations and mathematical notation which should facilitate reading the manuscript, 2) further details on the

MSE flux attribution to cyclones that enhance the reproducibility of our results, 3) an explanation of the statistical test adopted when studying cyclone composites and more details on the method sensitivity that would be very distracting in the methods section, 4) a note on the variability of single events and a (much shortened) description of the sensitivity of the results in Section 4 when using different methods established in the field, and 5) a complete description of the relationship between cyclone numbers and heat transport for different methods, which is the data basis for the discussion section 6.2 and potentially very useful for follow-up studies.

The general and specific comments are addressed below.

General comments

Reviewer Comment 3.1 — You make use of many abbreviations, which in some points makes it quite difficult to understand what one is reading. I understand that some of these abbreviations are commonly used and the difficulty encountered might be subjective, still it would help if some of these were avoided. For instance, AHT might be easy to drop in favour of heat transport. You only talk about the atmosphere, so there's no risk that it could get confused with ocean transport. Another example might be TE, which could be simply referred to as 'transient' as opposed to 'stationary' (SE).

Reply 3.1: We acknowledge that we use many abbreviations which may not always be the most intuitive. We appreciate your suggestions and got rid of 'AHT' which was replaced by 'heat transport' (specifying 'atmospheric' only at few instances). As we do not discuss a decomposition that features a transient overturning circulation, 'transient' MSE fluxes are indeed equivalent with 'transient eddy' MSE fluxes which we adopted in the revised version. We thereby got rid of 'TE' and 'SE'. Regarding the subscripts of the scientific terms, the use of 'TE' was misleading since it did not describe the decomposition method to define the transient fluxes. However, because the method dependency is important in our study, the subscript is inevitable to avoid ambiguity. We replaced the subscript $(\cdot)_{\mathrm{TE}}$ with $(\cdot)_{\mathrm{MA}}$ which better describes the definition of transient MSE fluxes from monthly anomalies (MA).

Reviewer Comment 3.2 — You also often directly use mathematical expressions (e.g., $\langle v'm'_{TE}\rangle$, $\langle v'm'^{cycl}_{TE}\rangle$, etc.) without giving them a descriptive name that would help the reader understand what they actually represent. For instance, $\langle v'm'_{TE}\rangle$ could be referred to as transient MSE flux, and $\langle v'm'^{cycl}_{TE}\rangle$ as the transient MSE flux attributed to cyclones, if I understood correctly.

Reply 3.2: We appreciate your input and, although the meaning of all terms is listed in the Appendix, reduced the use of those as much as possible. At some instances, the occurrence of a mathematical term is intended to retain precision of what is meant in a concise way, which some readers do appreciate. In your comment above, you understood correctly that $\langle v'm'^{cycl}_{TE}\rangle$ represents transient MSE flux attributed to cyclones, but to be precise, it is the vertically integrated flux. Having read the methods section, this should be clear, yet repeating these terms explicitly at a few instances can help some readers. Despite reducing their occurrence, if still referring to terms in the result sections in the remaining few instances, we explicitly state their meaning, for instance:

- in Sect. 3, L230: "First, it is investigated how the local transient MSE fluxes ($v'm'_{MA}$) evolve along the extratropical cyclone life cycle and where they maximize relative to the cyclone center."

- in Sect. 4, L317: “The zonal integral of the seasonal mean cyclone-attributed transient MSE flux, denoted as $\langle v' m'^{cycl}_{MA} \rangle$, amounts to [...]”
- or L391: “the cyclone-related transient MSE flux in a season, i.e., $\langle [\overline{v'm'}^{cycl}_{MA}] \rangle$” in Sect. 5 where it should be clear from the context that zonal integrals $[\cdot]$ are investigated.

Reviewer Comment 3.3 — Concerning Figures, some panels could be better explained, as it is not always clear what different lines represent (e.g., see comment below on Figure 6).

Reply 3.3: We revised the figure captions, in particular that of former Fig. 6 (now 7).

Reviewer Comment 3.4 — Figure B2 seems to me rather important, together with Figure 7, as they provide a visual example of your attribution method. As it is not straightforward to understand, please consider combining these figures and including the result in the method section. Furthermore, regarding Fig. B2, it is not entirely clear to me yet what the difference between light and dark colour shading is. Does dark shading represent MSE fluxes that are attributed to cyclones?

Reply 3.4: Thank you for your suggestion. We moved former Fig. 7 to the methods section where the attribution method is detailed. Within the schematic, we added black connectors between the cyclone masks and MSE flux features as present in Fig. B2. We chose a schematic instead of a snapshot because this allows us illustrating how cyclones are counted (purple bars) over their life-cycle, which would take many subplots for a real case example and likely be less clear to interpret. The purpose of Fig. B2 is illustrating the method dependency of the cyclone-attributed fluxes. The discussion thereof does not fit well into the methods section as it would deviate from the important descriptions. Regarding the color shading in this Figure, the caption reads: “The binary masks corresponding to the identified flux features shaded in reddish and bluish colors if MSE fluxes are poleward or equatorward, respectively, and in dark and light tones if fluxes correspond to and negative MSE anomalies, respectively.”

Reviewer Comment 3.5 — You measure the intensity of cyclones by looking at their minimum SLP, but at the same time acknowledge that in this way the analysis becomes biased towards high-latitude systems, given the climatological gradient in SLP (line 396). Why not considering a more objective measure of cyclone intensity, e.g., the maximum laplacian, which is not affected by the aforementioned bias?

Reply 3.5: Indeed, many different methods to quantify the cyclone intensity exist (e.g., Cornér et al., 2025). In our study, we chose the SLP-based tracking method because it also yields a cyclone area (defined by the outermost closed SLP contour). As the sensitivity analyses which used SLP anomalies from the climatological mean overall did not yield qualitatively different results, we chose to stick to absolute SLP as an intensity metric. Further testing the sensitivity of these results with regards to other cyclone tracking methods is beyond the scope of the study.

Note that the revised discussion of the methodological sensitivities is found in the paragraph starting with (L367) “A comprehensive sensitivity analysis ensures the robustness of the findings presented above.”

Reviewer Comment 3.6 — Regarding your attribution method, can you clarify how the MSE flux elements/patches are identified and partitioned? Is it using some kind of water-shedding algorithm?

Reply 3.6: In the initial version, we described the methodology briefly in line 192. The rephrased version reads:

L186: "Transient MSE flux features in this work are identified using a percentile threshold [...] A well-defined MSE flux feature mask is defined by adjacent grid points at which the percentile threshold is exceeded. The features are identified and labeled using TempestExtremes v2.1 (Ullrich et al., 2021)."

Specific comments

Reviewer Comment 3.7 — Lines 1-23: The abstract could be a bit more concise and clearer; at a first read, there are a few statements that are not so easy to interpret (e.g., lines 18-20).

Reply 3.7: Thank you for your feedback. We made the abstract more concise.

Reviewer Comment 3.8 — Line 22: the global warming statement is perhaps not so relevant here, I would consider removing it.

Reply 3.8: We removed this statement.

Reviewer Comment 3.9 — Line 40: why self-amplifying?

Reply 3.9: This sentence was removed when shortening the introduction.

Reviewer Comment 3.10 — Line 42: largest anomaly based on what? Monthly, seasonally, yearly?

Reply 3.10: We also excluded this reference to more quickly build up to the main research gaps and questions.

Reviewer Comment 3.11 — Line 46: Since "the" Earth

Reply 3.11: Thank you for pointing this out. This sentence was restructured as well.

Reviewer Comment 3.12 — Line 52: In "the" midlatitudes

Reply 3.12: This sentence got restructured.

Reviewer Comment 3.13 — Line 54: not sure why you use a semicolon here

Reply 3.13: Thank you for pointing this out, the sentence was rearranged for better readability.

Reviewer Comment 3.14 — Lines 55—59: I am not sure I can fully follow these two statements, at least not without actually reading those references; can you try and be more explicit here?

Reply 3.14: Thank you for your feedback. We elaborated on the framework and added a concrete example what can be learned from it:

L47: "[...] storm track activity is typically assessed in the zonal integral by defining the storm track latitude as the maximum in transient eddy MSE flux (Barpanda and Shaw, 2017; Shaw et al., 2018). This framework allows changes in the storm track latitude and intensity to be decomposed into changes in the hemispheric scale heat budget, i.e., the background heat transport, the heat storage, and energy input by radiative and turbulent fluxes. On the seasonal scale, for instance, the storm track response to radiative forcing in the NH is delayed by the stationary background circulation (Barpanda and Shaw, 2017)."

Reviewer Comment 3.15 — Lines 55—71: In general, I feel like the introduction could start with this paragraph, where both background and aim for this study emerge more clearly. The previous two paragraphs are nice and true, but one may struggle to follow until the aim is revealed... in the third paragraph.

Reply 3.15: Thank you for this comment on the structure of the introduction. We agree that stating the aims of this study only in the third paragraph is relatively late.

This study makes use of tools and analyses from both the 'Eulerian heat transport' and the 'Lagrangian weather system' community. Some readers thus might not be very familiar with moist static energy fluxes, while others do not think about cyclone life cycle characteristics everyday. The first two paragraphs aim to bring readers on the same page by introducing the concepts relevant for this study.

Acknowledging your concern, we shortened the first two paragraphs and removed parts that are only "nice and true" and unrelated to the aims of the study. In addition, for each of the three paragpraphs we added one clear research gap related to one of the research question:

L31: "However, while it helps to explain the growth rates of cyclones, it does not explain the exact number of storms in a season or how changes in cyclone number and intensity relate to each other. A small number of intense cyclones, for example, may erode as much baroclinicity as many weak cyclones (Sinclair and Catto, 2022)."

L53: "Although these existing approaches connect the zonally integrated atmospheric heat transport to local weather phenomena, the contributions of cyclones to the zonally integrated heat transport have not been systematically quantified."

L64: "Despite these advances, a systematic analysis of when during the cyclone life cycle MSE fluxes maximize has not been performed. Consequently, it remains unclear which life cycle characteristic most strongly modulates the instantaneous zonally integrated transient MSE flux and how much individual cyclones contribute to the seasonally integrated flux (Messori and Czaja, 2015) depending on their characteristics."

Reviewer Comment 3.16 — Line 72: I think you meant "concept", not "conception".

Reply 3.16: This paragraph was shortened and does no longer include this word.

Reviewer Comment 3.17 — Lines 73-74: What do you mean by "circulation in the eddy"? I found the whole sentence not so easy to follow.

Reply 3.17: We shortened this statement to simply state that under warming, (L73) "individual cyclones transport more latent heat because of the Clausius-Clapeyron relationship".

Reviewer Comment 3.18 — Line 81: what are these exceptions? Are there some data suggesting the number of cyclones is increasing?

Reply 3.18: Thanks for the question. We clarified that most studies agree on the change except from Chang et al. (2012) who do not find a robust circumpolar change.

L68: "Most studies that investigate cyclone numbers within coupled climate simulations agree that the cyclone number in SH winter decreases in a warmer climate (König et al., 1993; Geng and Sugi, 2003; Lambert and Fyfe, 2006; Grieger et al., 2014; Chang, 2017; Priestley and Catto, 2022) whereas Chang et al. (2012) identified no robust decrease apart from the Atlantic sector"

Reviewer Comment 3.19 — Line 85: How is the Rossby radius of deformation expected to respond? Should it increase as opposed to what is observed, i.e. diabatically-driven cyclones becoming smaller under warming?

Reply 3.19: It is interesting to think about the changes in the cyclone size under warming. As this study is about present-day cyclones, however, we shortened this part of the paragraph. We generalized this statement to

L76: "Moreover, the change in MSE transported by individual cyclones likely also depends on the cyclone characteristics such as the intensity or spatial extent, which are projected to change as well (Dai and Nie, 2022; Priestley and Catto, 2022)."

Here, we refer to the change (increase) in cyclone size found in Dai and Nie (2022).

Reviewer Comment 3.20 — Lines 87—89: I am struggling to see how this statement is related to what comes before. You mean that part of the latent heat fluxes may be modulated by ocean dynamics and thermodynamics?

Reply 3.20: We reduced the scope of the introduction at this point to that (L75) "the total heat transport does not remain constant" under warming, which is the general important point.

Reviewer Comment 3.21 — Lines 100-101: At a first read, the second research questions is not so clear. What do you mean by "cyclone characteristics [...] reflected [...] in the zonally integrated energy flux"? It might become clearer as one reads on, but so far I found it rather confusing.

Reply 3.21: Thank you for your feedback. In accordance with a comment on the overall contribution of cyclones to heat transport by reviewer 1, we rephrased the second research question to

L95: "How much of the zonally integrated heat transport can be attributed to cyclones, and how do their contributions differ according to their key life cycle characteristics such as intensification rate, intensity, and lifetime?"

Reviewer Comment 3.22 — Line 102: Why the focus on the seasonal scale? It comes a bit out of the blue.

Reply 3.22: We introduced the seasonal scale earlier in the introduction:

- L31: "However, while it helps to explain the growth rates of cyclones, it does not explain the exact number of storms in a season or how changes in cyclone number and intensity relate to each other"
- L50: " On the seasonal scale, for instance, the storm track response to radiative forcing in the NH is delayed by the stationary background circulation (Barpanda and Shaw, 2017)."
- L59: "In addition, days with enhanced zonally integrated transient MSE fluxes have a disproportionate impact on the seasonal integral (Messori and Czaja, 2015) suggesting that local extreme events may significantly contribute to climatological zonally integrated flux."

Reviewer Comment 3.23 — Line 114: What are these levels?

Reply 3.23: We added the levels we used as a footnote.

Reviewer Comment 3.24 — Line 115: This statement is out of place here, so I would remove and keep it in the referenced section.

Reply 3.24: Done as suggested. It is now placed together with the vertical levels.

Reviewer Comment 3.25 — Line 126: 1 Bergeron corresponds to a drop of 24hPa in 24 hours, not 1 hour. This also means that if a cyclone minimum pressure drops by 1hPa in one hour, it does not necessarily correspond to 1 Bergeron, as the pressure drop must be calculated over a 24-hour period. Given the confusion, can you please clarify the intensification criterium by which you exclude cyclones?

Reply 3.25: Thank you for spotting the typo in this line. As you correctly point out, it should read $24\,\mathrm{hPa\,d^{-1}}$ instead of $24\,\mathrm{hPa\,h^{-1}}$. We further elaborated that we used 24 h periods to calculate the pressure drop (L122).

Reviewer Comment 3.26 — Lines 128-129: I cannot understand this statement: how are leap days a problem? Also, don't you focus on Souther Hemisphere winter, i.e. June, July, and August? There are no leap days there.

Reply 3.26: Thanks for raising this question. For the tracks analyzed in this study, it is not relevant indeed and we removed this sentence.

Reviewer Comment 3.27 — Lines 131-150: The atmospheric energy balance could be presented in a better way. Also, it is not clear from this paragraph why vertical averaged of m and v are subtracted before computing the meridional overturning circulation term in Eq. 2. Please clarify.

Reply 3.27: Thank you for your comment. We revised the paragraph and added a reference that includes the derivations of the atmospheric energy balance in the form of Eq. 1. We do not believe that more elaborate derivations are well-placed in this paper.

L135: "To quantify atmospheric heat transport, we use the MSE framework. MSE is defined as $m = h + \Phi$ where the first contribution is the thermal energy of the atmosphere $h = c_pT + Lq$ where, c_p is the specific heat capacity, L is the latent heat of evaporation, T is temperature, and q denotes specific humidity and Φ is geopotential. In the vertical integral and zonal average, the local atmospheric heat budget (as derived, for instance, in Trenberth, 1991) is written as

$$\partial_t\langle[h]\rangle = [F_{\mathrm{TOA}} - F_{\mathrm{S}}] - \partial_y\langle[vm]\rangle, \tag{1}$$

where F_{TOA} and F_{S} represent the net energy flux at the TOA and surface, respectively, and v denotes meridional wind (Neelin and Held, 1987; Barpanda and Shaw, 2017) F_{TOA} is comprised of only radiative fluxes whereas F_{S} includes both radiative fluxes at the surface and turbulent fluxes. The temporal change in h is thus determined by the convergence of atmospheric heat flux and the net energy input at the surface and TOA."

Regarding the subtraction of vertical averages, we elaborated that this is necessary to ensure mass conservation. We references on this procedure for the interested reader:

L158: "To investigate the total MSE flux, we perform a correction to account for mass conservation which is not guaranteed in ERA5 (e.g., Mayer et al., 2021). In the zonal mean, mass-conservation corresponds to $\langle[v]\rangle = 0$ when calculating heat transport with respect to a time-independent atmospheric mass. We thus adopt the approach introduced by Marshall et al. (2014) and subtract165 vertical averages of $[m]$ and $[v]$ before computing the overturning circulation. Thereby, the MSE flux that is related to a net mass flux, which can be unrealistically large (Cox et al., 2023), is removed.

Reviewer Comment 3.28 — Line 157: Is 6PW referred to the maximum TE flux? Or is all at 45ºS? Why did you chose to highlight that latitude?

Reply 3.28: We adapted the description of the figure as:

L182: "In the SH, transient MSE flux peaks at 6 PW around 45° S, while the zonally integrated cyclone frequency reaches up to 12 % near 62° S."

Reviewer Comment 3.29 — Line 158: You mean the method in Barpanda and Shaw (2017)? It is not clear why there would be discontinuities at the turn of each month, unless each month's mean is removed rather than a centred 30-day mean. However, this is not specified anywhere. Please clarify.

Reply 3.29: Thank you for your comment, this was indeed not the clearest possible description. We specified that

L164: "Defining transient eddies as deviations from monthly mean data introduces discontinuities at the end of each month."

Reviewer Comment 3.30 — Line 160: This sentence is a bit out of context in this paragraph. What methodological biases are averaged out by using a large cyclone sample size? Also, I feel like the use of cyclone tracks has not been properly introduced yet, making this statement quite obscure to interpret.

Reply 3.30: Indeed, this argument was not placed in a good context. We moved this point to Sect. 6.2 where we state that

L494: “In the above summary, the conclusions are overall independent of the choice of eddy-mean decomposition method. A consequential method dependence was only found for the correlation between the seasonal cyclone number and the transient MSE flux (Sect. 5 and Appendix E). This points to a non-negligible influence of planetary, low-frequency waves to the local transient MSE flux attributed to individual systems as discussed below (Messori and Czaja, 2014; Stoll et al., 2023). Because some fraction of this background flux is attributed to an individual extratropical cyclone, the effect of its life cycle characteristics on the zonally integrated flux is partly masked. For large enough sample sizes, this method-dependency averages out (N $\gtrapprox$ 200 as in Sects. 3 and 4), but the effect influences the result when considering a seasonal set of intense cyclones (N $\approx$ 60, Sect. 5).”

Reviewer Comment 3.31 — Line 161: ‘for to explore’, remove the ‘for’

Reply 3.31: Thanks for pointing this out, fixed as suggested.

Reviewer Comment 3.32 — Line 165: do you really need an abbreviation for high-pass?

Reply 3.32: We agree that generally, high-pass does not need an abbreviation. Consistent with the comment above, we solely use ‘HP’ within the subscript to concisely indicate the flux decomposition method.

Reviewer Comment 3.33 — Lines 191-203: Please consider avoiding the percentage and just use values between 0 and 1, as it makes formulas easier to follow (e.g., 2 x (1- p))

Reply 3.33: Thank you for your comment. We switched from using percentages to values between 0 and 1 in the methods section and at multiple occasions throughout the remainder of the paper, including the appendix, for consistency.

Reviewer Comment 3.34 — Line 193: Any particular reason why this value was used?

Reply 3.34: Thank you for this question. Visual inspection revealed that, especially in the tropics and sub-tropics, many flux features only span 1–3 grid cells. In the extratropics, the features are more spatially extent as expected. Furthermore, excluding these small-scale subtropical features considerably reduces computation speed since fewer features have to be (uniquely) matched with cyclones. In sum, this value was chosen as an optimum to not discard too many slightly larger features.

Reviewer Comment 3.35 — Lines 205-211: This paragraph largely repeats what you say already in the methods section, consider removing it or highlight what new information is conveyed.

Reply 3.35: You are right. We rephrased the transition between the sections as:

L224: “These methods enable us to study the relationship between various extratropical cyclone life cycle characteristics and MSE fluxes. They also allow us to analyze the fractional contribution of cyclone-related MSE flux to the zonally integrated poleward heat transport and the relationship between cyclone number and intensity and zonally integrated atmospheric heat transport. By defining the number of cyclones as where they contribute to MSE flux we can study the linkage between cyclone numbers and heat transport in a consistent way.

3 Transient MSE fluxes along extratropical cyclones

First, it is investigated how the local transient MSE fluxes ($v'm'_{\mathrm{MA}}$) evolve along the extratropical cyclone life cycle and where the maximize relative to the cyclone center."

Reviewer Comment 3.36 — Line 211: What is "4 and 5"?

Reply 3.36: These section references are a typo — thank you for pointing it out.

Reviewer Comment 3.37 — Lines 285-286: The sentence inside brackets does not really add much, please remove.

Reply 3.37: We shortened the discussion of the composite results such that this part was removed.

Reviewer Comment 3.38 — Line 288: Don't you consider meridional fluxes by default when using v'? It is not clear to me what you mean to say with this sentence.

Reply 3.38: This sentence was cut out as well.

Reviewer Comment 3.39 — Line 312: Assuming 'that'

Reply 3.39: This paragraph was rephrased to

L299: "The relative contribution of $v'Lq'_{MA}$ reduces for cyclones propagating closer to the pole as expected due to the Clausius-Clapeyron relationship (black contours in Fig. 6a–c)."

Reviewer Comment 3.40 — Line 315: I don't understand this sentence, in particular the ending 'thereby the percentage to $v'm'_TE$'. Can you clarify?

Reply 3.40: This sentence was meant to repeat the argument related to the comment directly above. The adapted phrasing is more concise.

Reviewer Comment 3.41 — Lines 316-322: I am not sure this paragraph adds much to the discourse. Either I am missing something or I would just remove it.

Reply 3.41: We reduced this paragraph to stating the generally good spatial overlap between moisture flux and precipitation. While the paragraph discussed that this is not necessarily trivial, it did not relate to any major objectives of the study. The sentence replacing this paragraph reads:

L298: "The maximum of $v'Lq'_{MA}$, which is confined to the warm sector, largely overlaps with six-hourly accumulated precipitation (black contours in Fig. 6d-e)"

Reviewer Comment 3.42 — Line 323: What is 'this latitudinal separation'? It is not so clear to me.

Reply 3.42: Due to shortening and rephrasing, this statement is no longer used.

Reviewer Comment 3.43 — Lines 327-328: Maybe it is more a question of style, but I don't think you need to anticipate what the next section is about at the end of each section. Similarly below, you repeat the finding of the previous section at the beginning of the new section. It reads somewhat redundant and does not improve clarity.

Reply 3.43: Thank you for your comment. We reduced anticipating the upcoming results. Regarding the example you pointed out, we rephrased the statement to not read as much as an anticipation but as a disclaimer on the sensitivity of the previous results, which we decided is most naturally placed at the end of this section.

L304: "Overall, the cyclone-centered perspective confirms and expands on previous findings on (low-level) transient eddy MSE flux extremes (Messori and Czaja, 2015; Geen et al., 2016; Messori et al., 2017). As the following sections discuss vertically integrated MSE fluxes, it is worth pointing out that the findings regarding the horizontal structure of transient MSE fluxes also apply to vertically integrated fluxes"

Moreover, we removed the anticipation of Section 5 in Section 4. We further reduced redundancy between the paragraphs throughout the manuscript. The introductory paragraphs in Sections 3 and 4 were halved in length. Reducing redundancy also meant condensing the discussion of the role of the interplay of scales, which was distributed in Sections 4, 5, and 6 to two paragraphs in Section 6.

Reviewer Comment 3.44 — Line 332: The title is not so clear. What do you mean by 'different' cyclones? Is it based on intensity, duration, etc. In a way each cyclone is unique, so the title as it stands does not mean much.

Reply 3.44: We agree that the previous title was not very precise. We changed it to "How do cyclone life cycle characteristics shape the zonally integrated heat flux?"

Reviewer Comment 3.45 — Line 335: 'the' evolution

Reply 3.45: This sentence was cut (see comment above).

Reviewer Comment 3.46 — Line 338: I would remove the note '3'. A zonal mean is a zonal integral, so it does not need to be stated.

Reply 3.46: Thank you for your suggestion to shorten the text. Although Stoll et al. (2023) argue that this distinction is important to make (especially in polar latitudes where the cosine of latitude reduces any zonal average to zero), we removed this indicator because from the units in the text and plots it is clear that zonal integrals are shown.

Reviewer Comment 3.47 — Line 346: Remove 'than'

Reply 3.47: Thank you for spotting this typo. The sentence was further rephrased to

L324: "Note that the transient MSE flux located within the SLP-based cyclone masks is less than 1 PW [...]"

Reviewer Comment 3.48 — Figure 6: The way the caption is structured, it reads like the meaning of purple, black, and orange lines applies to panel (a) only. Please clearly state if the same holds for panels (b) and (c).

Reply 3.48: Thank you for pointing out that the caption lead to confusion. We clarified the use of the colors.

Reviewer Comment 3.49 — Line 354: No need to refer back to the methods section every time you use one of the methods, it is quite distracting.

Reply 3.49: We acknowledged your concern and kept the references to the methods section at a minimum. With restructuring the manuscript to list all methodological steps in the methods section (instead of the individual result sections), in each of Sects. 4 and 5 only one reference to the relevant method subsection is kept in the text for better orientation.

Reviewer Comment 3.50 — Line 355: Here you only briefly refer to Figure 7, which is not described at all. It would actually be more appropriate to include it in the methods section, where you describe the attribution analysis.

Reply 3.50: Thank you for your suggestion. We moved this Figure (formerly 7, now 2) to the methods section and slightly adapted it for even more clarity.

Reviewer Comment 3.51 — Line 359: You mean an absolute increase? Because compared to the black line, the orange line appears to capture more than double the amount of MSE fluxes across all latitudes.

Reply 3.51: Correct. Due to shortening of this subsection, we now use absolute numbers to describe the results:

L318: "[...] around 30% of the overall transient MSE flux is attributed to cyclones [...]"

Reviewer Comment 3.52 — Line 364: Please use absolute latitude, as it is confusing to read '20º poleward' when you actually are referring to a peak at 60ºS. Similarly below with '10º further equatorward'.

Reply 3.52: We rephrased the description according to your suggestion:

L327: "Based on the overlap method, we find the peak of the cyclone-attributed MSE fluxes at around 50° S, and therefore much closer to the MSE flux maximum near 42° S, while the fluxes within the SLP-based cyclone masks much further poleward (62° S)."

Reviewer Comment 3.53 — Line 367: Further equatorward than what latitude? In the subtropics there would not be any extra-tropical cyclones, which is what you focus on, so why would their absence in your tracks matter?

Reply 3.53: We removed this statement which deviated too much from the main points.

Reviewer Comment 3.54 — Line 376: Why are you citing those two papers here? Please remove as they are irrelevant here.

Reply 3.54: We clarified the use of these citations:

L336: "We compare the accumulated MSE flux of 200 strongest and weakest cyclones (as done in Catto et al., 2010; Sinclair et al., 2020)."

Reviewer Comment 3.55 — Line 397: Why 30h? I cannot understand this statement.

Reply 3.55: Thank you for your feedback. While restructuring this paragraph for more clarity and conciseness, this sentence was rephrased to

L373: "Excluding tracks shorter than 3 d further ensures that weak, short-lived tracks do not distort the picture"

Reviewer Comment 3.56 — Line 403: Note 5 is redundant, you have already stated that you focus on different latitudinal bands, so they must be two different sets.

Reply 3.56: We removed this footnote.

Reviewer Comment 3.57 — Line 409: The term 'unjustly' is not really appropriate, you mean systematically biased perhaps?

Reply 3.57: We shortened this section. The discussion about using a fixed radius vs. SLP-based masks has reduced to the note that (Sinclair and Dacre, 2019) used a constant radius in Sect. 6.

L472: "Previous research has found that the poleward propagation speed (and not necessarily intensity) is the characteristic for which the poleward moisture fluxes within a fixed radius around the center are the largest (Sinclair and Dacre, 2019)"

Reviewer Comment 3.58 — Line 413: Why is 'total moisture' italicised?

Reply 3.58: This sentence was restructured. Italicization was used to highlight the differences in the two studies. We investigate MSE fluxes, while (Sinclair and Dacre, 2019) investigated moisture fluxes. See also L472.

Reviewer Comment 3.59 — Lines 412-415: I struggle to understand the meaning of these two sentences.

Reply 3.59: The paragraph, including these sentences, has been rephrased, shortened, and moved to Sect. 6 as mentioned in the comment above.

Reviewer Comment 3.60 — Lines 416-421: This summarising paragraph is somewhat superfluous. Does it add anything to the previous ones? If not, either remove it, or integrate it in the conclusion section.

Reply 3.60: Thank you for your suggestion to reduce the repetitiveness within this section. We restructured this section such that the initial Section 4.3 was partly condensed into a paragraph in Sect. 4.2. The summarizing paragraph of the initial in Section 4.2 was shortened to half its original length. With the previous paragraphs having been adapted, it is now less superfluous but connects the important points in a slightly broader context.

L378: "In summary, against expectations based on the composite analysis presented in Sect. 3, the fluxes attributed to intense cyclones contribute more strongly to the zonal integral compared to those attributed to the most rapidly intensifying cyclones. Thus, not only the intensification rate but also the spatial extend of cyclones, which is typically large for the most intense cyclones, is an important determinant of the zonally integrated poleward energy transport associated with extratropical cyclones."

Reviewer Comment 3.61 — Line 425: Please check the grammar of this sentence, it reads a bit weird.

Reply 3.61: Thanks for your feedback. This paragraph was removed to make the discussion more concise.

Reviewer Comment 3.62 — Lines 425-428: What are the other complementary perspectives presented in the previous sentence then? It is not really clear to me what you are trying to say in this paragraph.

Reply 3.62: This paragraph was removed for conciseness as it distracted from the main points, see above comment.

Reviewer Comment 3.63 — Lines 430-431: Can be or are associated with? Also, what do you mean by 'a latitude band of the order of 10^{o}'?

Reply 3.63: This description of Fig. D2 was moved to Appendix D and rephrased to:

L590: "The averages depicted in Fig. 8 conceal the variability of the MSE fluxes attributed to individual cyclones. These are shown in Fig. D2 for the MA framework, in particular the cyclones that intensify least and most rapidly across the SH. While the means (solid lines) amount to around 20 PW, individual cyclones can be attributed more than 150 PW across a latitude band of more than 10° (Fig. D2a)."

Reviewer Comment 3.64 — Line 432: 'This prompts the above framing that the averages…' I really struggle to follow this sentence.

Reply 3.64: This sentence was cut when shortening the corresponding section.

Reviewer Comment 3.65 — Lines 469-482: These paragraphs essentially explain your attribution method, thus it would be more appropriate to move them to the methods section, together with Figure 7.

Reply 3.65: This paragraph was moved to the methods section as suggested and slightly adapted.

Reviewer Comment 3.66 — Line 627: There seems to be some typos in the latex command for Stoll et al. (2023).

Reply 3.66: Fixed.

Editor comment

Excerpt of the public justification in the scope of the co-editor decision:

[...]

I would recommend acknowledging the role of atmospheric fronts more explicitly. In the Southern Hemisphere, particularly across the lower midlatitudes near 40° S, long cold fronts are common and often extend well beyond the cyclone masks. This is consistent with Fig. B2, which indicates that the heat transports associated with cold fronts in this region exceed those captured by cyclone-only diagnostics. While adding an explicit analysis of frontal systems may not be feasible at this stage, noting this limitation would strengthen the interpretation of poleward heat transport mechanisms.

[...]

Reply: We thank the editor for their good suggestion. While the role of surface fronts was discussed at a few instances, we made this discussion more prominent and consistent from the introduction to the conclusion section. The corresponding discussions read:

- L57: "These MSE flux peaks typically form banded structures which, in some cases, follow the frontal zones near surface cyclones (Geen et al., 2016; Messori et al., 2017)."
- L254: "This is consistent with previous findings that heat transport is bound by the frontal zones (compare, for instance, with the 990 hPa lines in Fig. 4; Geen et al., 2016; Messori et al., 2017) which in the SH frequently extend to 20–40° S which is well equatorward of the cyclone centers (Schemm et al., 2015; Rudeva et al., 2019)."
- L327: "Based on the overlap method, we find the peak of the cyclone-attributed MSE fluxes at around 50° S, and therefore much closer to the MSE flux maximum near 42° S, while the fluxes within the SLP-based cyclone masks much further poleward (62° S). This is consistent with the fact that transient MSE flux peaks equatorward of the cyclone center along the trailing fronts."
- L461: "Crucially, a non-negligible fraction of warm-sector and cold-sector fluxes are located outside of the cyclone masks defined by closed SLP contours. Typically, warm-sector fluxes extend further equatorward and are spatially bound by the frontal zones."
- L490: "Other high-frequency eddy MSE fluxes not attributed to cyclones may occur far from the cyclone center (possibly along fronts), be linked to high pressure systems, or be unrelated to weather features with closed contours such as meandering zonal flows."
- L539: "A different approach to capture the heat transport along the fronts could be to attribute fluxes via identified surface fronts (e.g., as in Papritz et al., 2014), although it is unclear how well these capture fluxes throughout the sectors (Messori et al., 2017)."

References

Chang, E. K., Y. Guo, and X. Xia, 2012: CMIP5 multimodel ensemble projection of storm track change under global warming. *J. Geophys. Res.-Atmos.*, **117 (D23)**, 23118, DOI: 10.1029/2012JD018578.

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.

Dai, P. and J. Nie, 2022: Robust Expansion of Extreme Midlatitude Storms Under Global Warming. *Geophysical Research Letters*, **49 (10)**, e2022GL099007, DOI: 10.1029/2022GL099007.

Sinclair, V. A. and H. F. Dacre, 2019: Which Extratropical Cyclones Contribute Most to the Transport of Moisture in the Southern Hemisphere? *J. Geophys. Res.-Atmos.*, **124 (5)**, 2525–2545, DOI: 10.1029/2018JD028766.

Stoll, P. J., R. G. Graversen, and G. Messori, 2023: The global atmospheric energy transport analysed by a wavelength-based scale separation. *Weather Clim. Dynam.*, **4 (1)**, 1–17, DOI: 10.5194/WCD-4-1-2023.