Spatio–temporal averaging of jets obscure the reinforcement of baroclinicity by latent heating - reply #2

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General comment to the reviewers

We would like to thank both reviewers for a comprehensive assessment of the manuscript in both rounds of review. We think that your comments have significantly improved the manuscript and provoked interesting ideas and discussions. Please find an answer to all of your comments below. For most comments, we have made changes to the manuscript. In the cases we did not change the manuscript, we have explained why and hope that you find the explanations satisfactory.

The line numbering in our reply refers to the version with tracked changes.

Reviewer 1

Summary

The manuscript has been revised based on comments from two reviewers including myself. I am satisfied with most of the revision. I think the results are very interesting and add insight to the role of latent heating in the midlatitude circulation.

Besides a few minor comments, I still have two major suggestions. One suggestion is for an additional figure that could aid the interpretation of the results (see major comment 1). The second suggestion (major comment 2) is regarding the physical interpretation of the difference the authors find between the effect of latent heating on baroclinicity for the averaged and instantaneous flows. I don't disagree with the interpretation given in the manuscript, but I think that using other well-known concepts from the literature could add to the contribution of the paper to the theoretical understanding of the role of latent heating in the midlatitude circulation.

Major comments

1) As explained in lines 124-127, the results presented in this paper are calcu-

lated by binning the data and averaging over all members in each bin. It could be that the sample size varies greatly between bins. I think that adding a figure showing the sample size for each bin (for the 3h-Z, 1d-Z, 10d-Z, 3h-2D, 10d-2D and for the 3h-2D sorted by jet direction) would help to interpret the results better. It is important to see which cases are common and which are rare. For example, in lines 140-141 the authors mention that the patterns found at high latitudes in the 10d-Z analysis might not be robust due to the small sample size.

Thank you for pointing this out. We have added the requested figure in Appendix A and changed line 143.

2) As implied in the title and in many places in the manuscript (e.g. lines 209-212, 256-262 and 272-278), the authors argue that latent heating reinforces baroclinicity and that averaging over longitude and time obscures this picture. I agree that this statement is consistent with the results, but I think it gives a misleading impression for the role of latent heating. Looking for example at the lower panel of figure 1b, it is clear that latent heating occurs inside the warm conveyor belt of a frontal cyclone. The "jet" in this case is the upper level northward wind inside the cyclone, which is maximal slightly westward of the latent heating that occurs in the mid-troposphere. It is true that strictly speaking this reinforces the baroclinicity inside the cyclone. However, the term "baroclinicity" usually refers to the background (averaged) flow, which may become baroclinically unstable and allow for further growth of eddies (mostly cyclones). Here I think this local baroclinicity represents the available potential energy of the cyclone and not the background flow. One can think of it in the context of the Lorenz energy cycle: mean available potential energy (MAPE) is converted to eddy available potential energy (EAPE) which is then converted to eddy kinetic energy (EKE). I would argue that the results of this paper demonstrate that latent heating reduces MAPE and increases EAPE. Therefore it plays a role in the energy transfer from the mean flow to the eddies. This interpretation is based on the following results:

(a) Latent heating is located eastward of the upper level northward instantaneous jet (Figures 5g, 7c, 9, and the example in Figure 1).

(b) The regions of strong latent heating are characterized by upward wind, large-scale precipitation, positive temperature anomaly and high specific humidity (Figures 5-7).

(c) Latent heating is concentrated on the poleward side of the zonally- and temporally-averaged jet (Figure 3j,k,l).

Results a and b fit well with the characteristics of frontal cyclones and warm conveyor belts. Inside the cyclone there is a positive correlation between temperature and upward wind ($w'T'>0$) and temperature and northward wind ($v'T>0$), as well as specific humidity and upward or northward wind ($w'q'>0$, $v'q'>0$). The positive correlation between temperature and northward wind ($v'T'>0$) is necessary for the conversion of MAPE to EAPE and the positive correlation between temperature upward wind ($w'T'>0$) is necessary for the conversion of EAPE to EKE (see Lorenz, 1955; and a recent example in Okajima et al. 2022). Latent heating then contributes to this temperature anomaly and to the conversion of MAPE to EAPE. Note that in this paper the "jet" is measured at a higher level than the latent heating so that if the flow inside the cyclone is tilted westward with height (as in a characteristic baroclinically unstable eddy), then the upper level northward wind would be slightly westward of the mid-tropospheric northward wind and the positive temperature (and latent heating) anomaly that is positively correlated with it. Also note that the opposite patterns of upward/downward motion between the instantaneous and averaged jets (lines 182-184) are consistent with the differences between the Ferrel circulation and the Lagrangian-mean circulation (as approximated by the Transformed Eulerian Mean). The fact that latent heating is concentrated on the poleward side of the zonally averaged jet (figure 3) demonstrates that it acts to decrease MAPE. Therefore, I don't think that saying that latent heating reinforces the baroclinicity delivers the right impression for those thinking in terms of wave-mean flow interaction. My suggestion is to use the well-known concepts of the Lorenz energy cycle to explain the role of latent heating in the midlatitude circulation. The authors can consider whether to accept this suggestion or not.

Thank you for bringing up this important point. Reviewer 2 raised a similar concern in the first round, which was also discussed among the authors before first submitting the paper.

Explaining results (a), (b), and (c) using MAPE and EAPE requires separating eddies from a background (or mean) state and choosing a reference state. Based on our findings, the subjectivity in these actions potentially obscures the link between APE, baroclinicity, and latent heating.

Furthermore, the representation of baroclinicity in the APE framework is highly sensitive to the choice of reference state. While a reference state computed from a channel model scaled for baroclinic instability gives an APE distribution that represents lower tropospheric baroclinicity (Federer et al. 2024), APE relative to a global reference state does not (Novak and Tailleux 2018).

Hence, we find it difficult to reconcile the APE framework in our discussion on the latent heating effect on baroclinicity and have therefore opted to not delve further into this analysis. However, as this discussion is likely of interest to the readers of WCD, we have included a paragraph on lines 239-246 and removed the paragraph on lines 218-220.

Minor comments

1) Line 48: two comments: (a) "explore the extent to which" instead of "explore to the extent which". (b) "... occurs on the cold or warm flank of each jet definition" instead of "... occurs on the poleward or equatorward flank of

each jet definition". For the instantaneous jet its really mostly the westward vs eastward flank.

(a) Thanks and (b) great point, thanks a lot - this has been corrected. Lines 48 and 49.

2) Line 74: "only allow for one jet latitude for each time step" (add "for each").

Thanks - done! Line 75.

3) Line 79: since you removed the hat above n in equation (1), it needs to be removed here as well.

Yes, that was a slip. Done. Line 80.

4) The paragraph in lines 83-91: The parameter K is not defined. Shouldn't it have units? It is written here as if it is a dimensionless parameter, but it should have units of $\sec^{-2}$ if n has units of length. Also there is some confusion with the sign. If you use it to detect a maximum point then K should be negative and the inequality in line 86 should be opposite (the second derivative is lower than some negative threshold). In line 96 it has a positive value, which seems inconsistent.

You are absolutely right, thanks a lot for spotting this! Line 95.

5) Line 91: "de tails" – change to "details" (delete the space).

Done, thanks. Line 90.

6) Lines 99-100: "Fig. 1a shows…" (add "a") and remove "(Fig. 1a)" at the end of the sentence.

Done. Line 99.

7) Lines 118-119: Here it should be "left side" and "right side" instead of "cold flanks" and "warm flanks". It can be explained that the left side is cold and the right side is warm, due to thermal wind balance, and then the terms "cold flanks" and "warm flanks" can be used for the rest of the paper, but since this sentence describes the geometry of the analysis I think the terms "right" and "left" should be used here.

Great suggestion, this has been updated. Lines 118-120.

8) Lines 185-186 and 188-189: "ascent appears to be mainly a function of latitude" and "the specific humidity is… mostly a function of latitude" – it should be "mostly a function of the jet latitude", because if it were a function of lati-

tude, it would have had a diagonal structure, as in figure 3.

Nicely spotted, has been fixed. Lines 192, 195.

9) Caption of figure 5: I think the last sentence should be moved to the methods section.

Thanks, done. Figures 3,5,7 all had similar sentences at the end of their captions, which have all now been moved to the methods. Lines 111-112,120-121,126-129

10) Line 223: "Figure 7 only shows" instead of "only show".

Thanks, done. Line 228.

11) Line 237: "Appendix A" instead of "Sect. A".

Thanks, done. Line 250 (and 143).

12) The caption of figure B1 is not clear. I couldn't understand what it means that the circles correspond to a certain number of cross sections. How is the center and radius of each circle calculated? How is the red contour calculated

Thanks. We have updated Figure B1 and the caption. Hopefully, it is better explained now.

Reviewer 2

This is an intriguing piece of work. The authors address the issue of where the stormtrack latent heating occurs relative to the jet, and hence whether this heating reinforces or depletes the extratropical baroclinicity. The analysis helps reconcile two seemingly conflicting views: the climatologist view that the latent heat transport helps decrease the meridional temperature gradient, and the synoptician view that the latent heating actually occurs on the warm sector of extratropical cyclones. The authors convincingly show that both are true. The heating occurs on the poleward side of the Eulerian mean temperature gradient (and hence helps decrease that mean gradient) but on the warm side of the distorted isotherms, consistent with common synoptic experience. One could say that the 2D averages considered in this paper have a bit of a cuasi-Lagrangian flavor.

I feel that the analysis presented in this paper has broader implications than the annular mode persistence problem that seems to be its primary motivation. While also relevant for that problem, the relation between baroclinicity forcing and eddy momentum flux memory is somewhat speculative and still under debate. I suspect the motivation of the authors to emphasize the persistence implications is novelty, as the idea that the stormtracks are self-maintained by their associated latent heating is certainly not new. But even if the notion is not entirely new, I think the analyses in this and previous papers by the same authors provide novel support and physical intuitition for the self-maintenance theory.

The link between self-maintenance (depletion) and persistence as suggested by Xia and Chang (2014) motivates studying the latent heating effect in context of jets that shift meridionally and not only in the climatological storm track. Indeed, the link between baroclinic forcing and eddy momentum flux forcing is debated. We are currently working on quantifying the effect of latent heating on the flow on time-scales beyond the life time of a cyclone, but that has to be for a future publication.

I already provided a first review during the open discussion period in the egusphere. The authors have satisfactorily addressed the most important issues raised in that review. Reading the revised version has raised additional thoughts, most minor. The only comment that may require significanty work is the first one but I leave that at the discretion of the authors as I submit a recommendation of minor revision.

1) I believe most of your diagnostics employ full fields (as opposed to anomalies). Have you actually tried using anomalies instead? That should make the main results cleaner, I think. In particular, the latitudinal-dependent background present in some of the figures might be removed if you subtracted the climatology before compositing. The connection with Xia and Chang (2014) would also be clearer using anomalies because these authors use regressions. Though not

required for acceptance, I urge you to give this some consideration.

Thanks for this suggestion! We have not tried using anomalies, but we agree that the connection to Xia and Chang (2014) would be clearer. Using anomalies, one could e.g. see whether: given the presence of a jet, do we have more or less latent heating than normal, and on which flank. However, if one where to give graphical illustration of the eddy correlation between air temperature and latent heating, we would somehow need to define an anomalous 2D jet axis - which is not straight forward and requires choosing a reference state. Although it would be interesting, redoing the analysis using anomalies would require quite a bit of work and given that our results are already quite clear, we think that this is something that could maybe be left for future work.

2) Fig.3 caption. Please specify which latitudes the diagonal lines correspond to

Thanks, this has been updated in the figure caption.

3) 173. My understanding of the results of Lachmy and collaborators (I'm not one of them) is that *heating* displaces the eddy driven jet equatorward of the eddy heat flux maximum. To shift the eddy driven jet poleward of the heat flux maximum one would need cooling. When the heating/cooling is weak, the surface westerlies and heat flux maximum should be collocated, I think.

This is also our understanding. We have exchanged "poleward/equatorward shifts" with "latitudinal shifts relative to the eddy heat flux" in our reference to this paper to avoid any confusion. Thanks! Lines 178-181.

4) 180. As I said in my previous review, I don't think the 10d-2D results mean much. It is of course your call whether to include them or not but if you want to investigate the sensitivity to the averaging period, I would rather suggest trying some intermediate value like the 1d averaging considered for the sector means. It seems strange that you only consider this averaging in that case. Do you get mixed signals wth the 2D jets perhaps?

We never calculated a 1d-2D version. Time-averaging 40 years of reanalysis data, applying the T84 truncation, detecting the jets, creating a random sample of jet axes, and interpolating all variables to the jet cross-section is quite time-consuming and costly. When choosing how to spend our resources, we choose to compute the "ends of the spectrum" of the 2D jet definitions. It would indeed be interesting to try to see whether a 2D version of daily data is sufficient to capture the 3h-2D signal. We agree that the 10d-2D results do not mean much, but that is also the point. However, we think that the current set of results is sufficient and leave a 1d-2D version for future work. Thanks for the suggestion, though!

5) 190. Could the cold side latent heating occur because the detected jet is subtropical and not associated to cyclones?. Is this heating associated to eastward oriented jet perhaps? I also wonder if the convective precipitation found in this case (line 199) could be associated to tropical/subtropical cyclones rather than extratropical cyclones.

Interesting suggestion. Since we're detecting jets on the 2PVU surface, we doubt that the African easterly jets are well represented, if this is what you are thinking of. Figure 1 in this document shows subtropical 3h-2D jets, identified using the potential temperature threshold of Spensberger (2024). The strongest signal is in the large scale precipitation on the warm flank, but note that the sample size is smaller here.

6) 234. Sect A should be *Appendix* A.

Thanks, done. Line 250.

References

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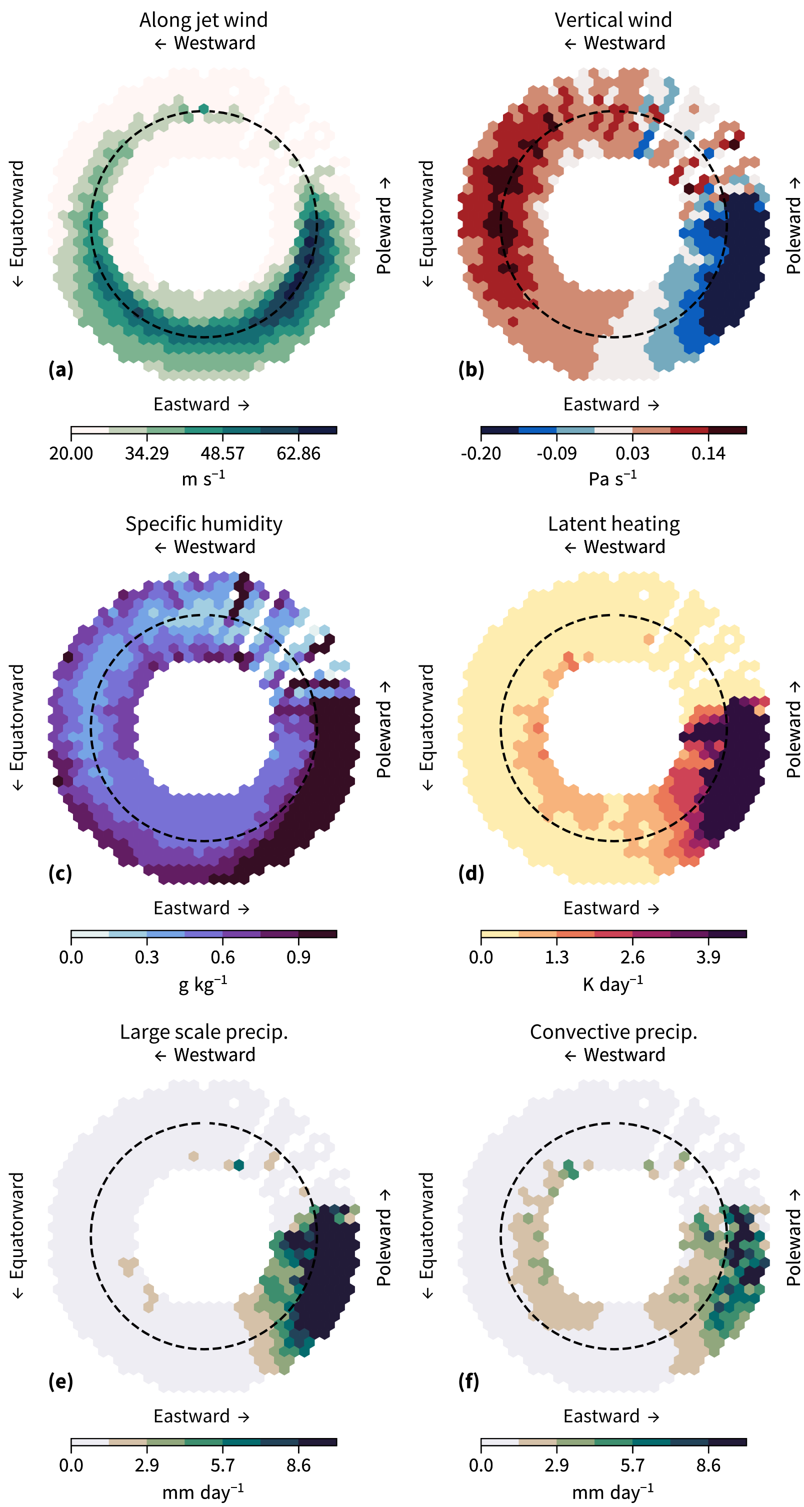

Figure 1: Like Fig. B2 in the manuscript but only considering jets with potential temperature in their core on the 2PVU surface equal to or larger than 335 K.