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Interactive comment on "Potential-Vorticity Dynamics of Troughs and Ridges within Rossby Wave Packets during a 40-year reanalysis period" *by* Franziska Teubler and Michael Riemer

Anonymous Referee #1

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General Comments

The authors present a detailed study into the composite evolution of ridges and troughs within Rossby wave packets (RWPs), utilising a quantitative PV framework developed in previous publications. This is a well-studied problem but applying these diagnostics to it is certainly novel and has shed new light on some aspects of the dynamics involved, particularly with respect to the role of latent heating. They incorporate a large amount of data, by considering RWPs throughout the whole of ERA5, and consider the problem from several different complementary angles.

The manuscript is well written, and all figures are clear, and the results will certainly be

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Discussion paper

of interest to the wider community. I therefore recommend this paper is accepted for publication, subject to the following minor comments being addressed.

Specific Comments

L25: I'm not sure what you mean by the last sentence of the abstract. 'the most relevant aspect' in what respect?

L141: You call the first term on the RHS of Eq. 2 the 'adiabatic advection' of PV. This term is vague since, as you know, the wind field v is clearly modified by diabatic heating. I wonder if 'isentropic advection' provides a more accurate description? The term represents the advection of PV along isentropic surfaces (which makes a lot of sense when thinking about diabatic effects, see e.g. Harvey et al. (2020, QJRMS)), rather than the full 3-d material derivative following fluid parcels that many people are more used to thinking about, and 'isentropic advection' emphasizes this point. Also, I couldn't see where you defined v.

Sec. 3.2: It's commendable that you include all the details of the quality control you apply to your identified RWPs, and it's surely a complex task to filter out the events with 'questionable representativeness'. However, I was left wondering how you arrived at these thresholds. Have you tested the sensitivity of your results to any of these choices? In other words, how confident are you that you have succeeded?

Fig. 2 caption: Which axis is observed, and which is diagnosed? I may have misunderstood, but I wonder if 'amplitude tendency' is a better description of what is shown than 'amplitude evolution'? Also, what do you mean by '2d-fit', is it a least-squares regression? Finally, the symbol 'r' is often used for correlation, is there another symbol you can use for the slope here?

L276: 'weakening of ridges and an amplification of troughs' is confusing here because of the signs involved. Could you clarify whether you mean weakening of ridges or more negative PV tendencies, and how that relates to the offset from the origin in Figure 2.

L318: Just a comment. You note that the LHR is substantially stronger in winter than in summer, but that the divergent tendencies are similar. Are you able to tell why from your diagnostics? Is this because the divergent flow is similar in the two seasons, or because the PV gradients are weaker in summer than winter (or some other reason)? If the former, then is this just a result of having stronger static stability in winter?

L325: Could you expand on the methodology here. I think the composite time for each ridge/trough is based on the max/min values of the terms in Equation 6? Is that correct? Having just seen the spatial composites, I was not sure if it was that or some local maxima of the fields shown in Figure 3.

Figs 4, 5 and 6 captions: Using the words 'strongest' and 'weakest' could cause confusion here, due to anomalies taking both signs. Do you mean max and min? It might also help clarity if you reminded the reader that these plots include data from all seasons (in contrast to the Figure 3 which split into summer and winter), perhaps in the text at the start of section 4.2.

L385: I missed whether this section just uses the RWPs from the YOTC period, or all ERA5 RWPs with non-conservative tendencies only computed from the YOTC cases. Please could you clarify.

L436: I agree that the divergent flow has a detrimental impact on this measure of trough amplitude, based on area-integrated PV, but the mechanism is presumably much more adiabatic than the corresponding amplification of ridges, where mass is injection into the isentropic layer by the latent heating. I wonder if the depth-integrated mass-weighted PV [a more dynamically relevant measure of wave activity] also exhibits this effect?

L505: Again, just a comment. Is it obvious that divergence associated with the barotropic component does not also contribute to ridge building in the case of RWPs?

Technical Corrections

L74: ‘occurrenc’ -> ‘occurrence’

L92: Should this read ‘One prominent direct nonconservative impact’?

L139: This definition of \zeta_\theta is imprecise. Is it v_x - u_y with the derivatives evaluated along isentropic surfaces?

L324: ‘at that the’ -> ‘at which the’

L356: ‘baorclinic’ -> ‘baroclinic’

Fig 6 caption: You don’t say what the arrows show, presumably the composite divergent wind?

L527: ‘efficiency by that latent heat’ -> ‘efficiency by which that latent heat’

L563: ‘the the’ -> ‘the’

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