Reply to the Reviewers

Reviewer #1

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

We thank the reviewer for her/ his insightful comments that helped to further improve our manuscript. Our responses to the comments are given below.

Specific Comments

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

Upon reflection, we agree that this last sentence is hard to understand, in particular before having read the manuscript. The sentence refers to the role of dry dynamics in the divergent amplification of ridges considered in some detail in our Sect. 5.3. Based on the comment of another reviewer, we will revise that section to some extend and will then reconsider (and clarify) the wording of the last sentence in the abstract.

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.

We agree and have renamed the term to isentropic advection. In addition, v is now defined.

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?

We have tested the sensitivity to the threshold for the differences between the observed and the diagnosed tendencies, for which we have eventually applied the 1.5-IQR rule, in some detail. The results of these tests are described in Sect. 3.3. Based on your comment, we note that this description is not sufficiently clear. What our sensitivity test showed is that the bias in the slope of the diagnosed tendencies in Fig. 2 stems from missing amplification by merging and weakening by splitting events: Changing the threshold for the IQR rule mostly changed the distribution shown in Fig. 2 for large absolute values of observed values. The mean values of diagnosed tendencies lie very close the diagonal for small and moderate absolute values, i.e., for the vast majority of data, irrespective of the choice of threshold. We are thus confident that the mean-picture presented in this study is not contaminated by data of questionable representativeness. Understanding that the bias observed in Fig. 2 is arguably due to missing partial merging and splitting events, we chose to be not particularly restrictive with the threshold for the IQR rule.

We have modified the manuscript to reflect this information.

Using the 3-IQR rule in the cases described in Sect. 3.2 affects few data and, according to literature, 3-IQR is a standard value for eliminating outliers. We thus spent less time testing sensitivities with respect to the choice of this threshold. Using values of 2 and 4 did not change the results shown in Fig. 7 in any notable way.

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?

Thanks for pointing this out. We have added the axis labels. In addition, we agree that ‚amplitude tendency‘ is a more precise term in this context and have changed the text accordingly. With 2d-fit we wanted to point out that we use total least squares instead of ordinary least squares to perform linear regression. As you might recall ordinary least squares only try to minimize the residual between the y-axis variable and the fit and do not account for uncertainties in the x-axis variable. Since our x-axis variable contains also uncertainties, we try to minimize the residual of both variables with the fit. We now clarified this in the caption of Fig.2 and changed the symbol r.

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.

We agree. This description needs clarification. More confusing, however, is the fact that we accidentally had omitted the important information that the tendencies for the ridges in Fig. 2 had been multiplied by (-1) such that positive tendencies would denote amplification for both, ridges and troughs. We apologize for this omission, which has most likely contributed to the confusion. We now prefer to show the figure without this rather confusing modification. In addition, we have modified the text and now write for increased clarity “… weakening of ridge amplitude and an amplification of trough amplitude.” in the last paragraph of Sect. 3.3.

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?

Before submission, we had thought about this rather curious observation, too. Unfortunately, we could not find a non-speculative explanation. Differences in static stability could be one explanation but our diagnostic does not provide a straight-forward means to test the idea. With a weaker PV gradient (in summer) we would expect weaker PV tendencies from the same upper-tropospheric divergence so this is likely not an explanation. A further potential explanation is that LHR in summer is more often associated with convection, which is potentially not sufficiently resolved by our proxy of LHR, whereas in winter LHR is mostly "on the grid scale". Again, we did not find a straight-forward way to test this potential explanation. In the manuscript, we prefer to refrain from speculative explanations.

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.

Yes, you are right. It is based on the terms in Equation 6. We now made this point clear at the beginning of Section 4.2.

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.

Thank you, we have added a reminder at the start of Sect. 4.2.

For clarity, we have changed the caption to "... for ridges (a,c) and troughs (b,d) at the times when the quasi-barotropic PV tendencies yield maximum amplification (a,b) and maximum weakening (c,d) of the respective amplitude."

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.

The latter. We clarified this point in the text.

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?

We thank the reviewer in particular for this comment. In our framework, the impact of the divergent wind on the area-integrated PV anomaly is two-fold (Eq. 6 in the manuscript): i) advection of background PV and ii) change in the area of the PV anomaly. Considering the second effect in isolation, the horizontal boundaries of an anomaly constitute material surfaces. Considering the anomaly between two isentropic levels results in a material volume for adiabatic motion. According to the impermeability theorem (Haynes and

McIntyre 1987,1990), the density-weighted PV (or "PV substance", to which we believe that the reviewer refers) will not change for adiabatic motion. An amplitude metric defined based on such a volume integral of PV substance should thus not yield an amplitude change due to this second effect of the divergent wind. As noted by the reviewer, the situation is different if diabatic transport effectively changes the mass of a PV anomaly sandwiched between two isentropic levels (as in the case of ridge building as indicated by our results).

We appreciate that the reviewer points to this intriguing between PV-based amplitude metrics. Because there are no other references to PV substance or wave activity in our manuscript, we prefer not to point out this interesting point in this study.

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?

Thank you for noting this unclarity. Our premise here is that the dry component of upper-tropospheric divergence varies to lowest order with the stage of the baroclinic life cycle. With the available PV tendencies as proxies, the stage of the baroclinic life cycle is arguably more closely related to BC than to the barotropic component. Indeed, this reasoning is currently not expressed sufficiently clear and we have clarified it in the revised version.

Technical Corrections

Thank you for the careful reading. We have corrected the manuscript accordingly.

L74: ‘occurrenc’ -> ‚occurrence’ thanks

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

Thank you, our wording here was indeed somewhat unclear. We meant to say: "One prominent indirect nonconservative impact are advective tendencies by the winds associated with low-level PV anomalies generated by latent heat release, in particular their role in enhancing baroclinic growth." We have changed the manuscript accordingly.

L139: This definition of \zeta_\theta is imprecise. Is it v_x - u_y with the derivatives evaluated along isentropic surfaces? Yes. For clarification we now write "... the component of relative vorticity perpendicular to an isentropic surface."

L324: ‘at that the‘ <- ’at which the’ thanks

L356: ‘baorclinic‘ <- ’baroclinic’ thanks

Fig 6 caption: You don’t say what the arrows show, presumably the composite divergent wind? Thanks, yes, we have added this information to the caption of Fig. 6.

L527: ‘efficiency by that latent heat‘ <- ’efficiency by which that latent heat’ thanks

L563: ‘the the‘ <- ’the’ thanks

Reviewer #2

We thank the reviewer for her/ his careful reading of our manuscript and the thought-provoking comments. The comments help to further improve our manuscript. Our responses to the comments are given below.

General Comments:

1. Equations (1), (2) and (3) were first presented in this context by Davies and Didone (2013). However, there appear to be some sign mismatches to the derivation of Teubler and Riemer. In particular, the first term in (3) should have a minus in front and the same applies to the third and fourth term on the right-hand side. Even though the authors do not use these terms explicitly in large parts of their study, as they mainly evaluate the adiabatic terms, these errors should be corrected if the equation is to be maintained in the manuscript.

Thank you for carefully checking the equations. Indeed, there is a typo and the first term on the right-hand side of equation (3) should have the opposite sign. We have double checked our code and confirm that our implementation is correct.

Regarding the third term, we agree that splitting the diabatic term in a “stretching” (second term) and “tilting” (third term) contribution is non-standard and that we have adopted this formulation from DD13. We now choose to present the “standard” form of the nonconservative impact on PV in isentropic coordinates, i.e., we omit the splitting because we do not consider the individual contributions of diabatic heating in this manuscript.

The signs of the third and the fourth term in our original manuscript are correct. For the fourth term It is clear that relative vorticity and thus PV increases if the curl of the accelerations (v dot) is positive. The sign of the third term can be verified rather easily by explicit calculation of the splitting of the diabatic term. Note that there are incorrect signs in Eq. 3a, 3b, and 4 in Davies and Didone (2013)) and in Eq. 72 and 73 in Hoskins et al. (1985)!

2. The PV partitioning is not clear. Do the authors assign the entire potential temperature anomaly at the upper and lower boundary exclusively as boundary condition to the upper and lower PV anomaly, respectively, and for inversion a zero potential temperature anomaly is assumed at the other boundary? What is the justification for such an assignment? Or in other words, why should the upper level PV anomaly not significantly project onto the lower boundary and vice versus? Can the exclusion of such an influence be justified?

Our piecewise PV inversion inverts i) the low-level PV anomalies between 850 and 650hPa) together with the temperature anomalies at 875hPa and ii) the upper-level PV anomalies between 600hPa and 150hPa together with the temperature anomalies at 125hPa. For the upper-level inversion we thus do not assume any anomalies (PV and temperature) below 600hPa, and vice versa for the inversion of low-level PV anomalies. The idea to consider boundary theta anomalies as distinct anomalies traces back to the Eady model. The role of theta anomalies as (delta-distributed) PV anomalies has been made explicit by Bretherton (1966). This is at the heart of PV partitioning and the idea of counter-propagating Rossby waves (e.g., references in manuscript: Hoskins et al. 1985, Emanuel et a. 1987, Heifetz et al. 2004b, de Vries et al. 2009). Such a partitioning was hence used in many previous studies employing piecewise PV diagnostic (e.g., Davis and Emanuel 1991, Davis et al. 1996, Nielsen-Gammon and Lefevre 1996, and our own previous work). Due to this standard use of the partitioning, we do not agree that the partitioning is unclear, at least as long as one accepts the standard paradigm of PV partitioning.

If we understand correctly, the reviewer challenges this paradigm and is concerned that static stability anomalies associated with the upper-level PV anomalies could penetrate down to the low-level boundary and imprint on the boundary theta distribution. We commend the reviewer on this out-of-the-box thinking and appreciate this thought-provoking question. In principle the reviewer is correct that the upper-level anomalies may impact the low-level theta distribution by associated stability anomalies. Stability anomalies arise because of a vertical deflection of theta surfaces by adiabatic vertical motion, which, in turn arises as part of secondary circulations during an adjustment-to-balance process. If the lower boundary were defined as a rigid boundary, i.e., the Earth's surface, then there could not be any theta anomaly associated with the upper-level PV distribution because vertical motion vanishes at the rigid lower boundary. In "standard" PV thinking, however, the lower boundary is defined at the top of the planetary boundary layer to avoid "contamination" of the balanced dynamics by boundary-layer processes. Vertical motion thus does not need to vanish at a such-defined lower boundary and, in principle, the boundary theta distribution could be modified by upper-level PV anomalies. Vertical motion, and thus theta anomalies associated with upper-level PV anomalies, however, can be expected to be small at the top of the planetary boundary layer due to the closeness to the rigid bottom where vertical motion needs to vanish (in more technical terms: vertical motion associated with upper-level balanced (PV) dynamics can be solved for by a variant of an omega-equation, i.e., by inverting an elliptic partial differential equation. The boundary condition vertical motion = 0 will ensure that vertical motion approaches zero when approaching the boundary). In addition, there is a density effect, which dictates that vertical motion needs to decrease with increasing density, i.e., height to fulfill continuity (in the absence of horizontal motion). In summary, there are sound theoretical arguments why low-level theta anomalies associated with upper-level PV can expected to be small. Synoptic experience supports the theoretical considerations. In practice, the "standard" separation of PV anomalies is thus well justified, although in principle there may be a non-zero imprint of the upper-level PV anomalies on theta at the top of the boundary, i.e., the lower boundary used for piecewise PV inversion.

3. In section 4.2, the authors talk about “amplification”, though it is not clear what they mean by that. If the advective tendencies that the authors discuss in this section should have an amplifying effect, there should be an alignment of the tendency with the actual anomaly. However, the phase shift between the tendency and the anomalies is more or less 90 degrees, which implies, as the authors pointed out in a previous section, a propagating response, not an amplifying response. This also renders the discussion about the relative weight of up- and downstream PV anomalies in this context questionable. In a way, the amplitude of the tendency is larger where it is located between larger PV anomalies, which is not surprising if this pattern is supposed to be propagated by advection. Based on these arguments, the conclusion of the authors in the second paragraph of this section about amplification associated with the quasi-barotropic advection is misleading.

Our definition of the tendencies that govern "amplitude" evolution are given in some detail in Sect. 2.3. Our amplitude metric is the PV anomaly spatially integrated over the region of the anomaly. Based on the reviewer's comment, we realize that this definition may not be explicit enough and have modified the text to clarify our use of the term amplitude. The reviewer is correct that one prominent signal in the quasi-barotropic tendencies is a 90 degree phase shift. A further signal, which we explicitly describe in the second paragraph of Sect. 4.2, is that amplifying tendencies dominate over the weakening tendencies. The reviewer's comment makes it clear to us that we should have noted here explicitly that this distribution of tendencies leads to amplification in the spatially integrated sense that is considered in our study. We now do so in the revised manuscript. The discussion of the relative weight of anomalies and our conclusions are thus not in question.

4. Similar to the arguments in the previous general comment, the amplification associated with the baroclinic tendencies also needs some revision. As there is now significant alignment of the tendency with the PV anomaly, it is correct to refer to an amplification. However, the fact that the largest PV anomaly has the largest tendency does not mean that it is growing the fastest in a relative sense. For example, if a pattern would be growing exponentially, such as in the Eady model of baroclinic instability, and if there would be a smaller and larger anomaly, they might feature the identical exponential growth, but the absolute tendency of amplification is different due to the different amplitudes. Therefore, the meaning of amplification needs to be clarified. For example, do the arguments of the authors hold if one diagnoses relative tendencies, i.e., normalized by the amplitude of the respective PV anomaly. It appears from Fig. 5 that this might be the case for some parts of the RWP but not in a general sense.

We agree with the reviewer that a note may be helpful to avoid confusion of readers that may expect an analysis of growth rates at this point. In general, a different choice of the definition of "growth" may yield somewhat different results. We deliberately choose to examine absolute growth and not relative growth of anomalies. One reason for this choice is that small anomalies during the initial stage of their development are hard to reliably identify, at least with our identification and tracking method. To avoid introducing an associated bias to relative growth rates, we prefer to study the absolute growth of anomalies.

5. In section 5.1, the authors present a comparison between the dynamic and thermodynamic terms, where only radiation plays an appreciable role in the overall development, while latent heating and other terms are rather minor. However, when looking at Fig. 9, it appears that in the evolution of the packet, there is almost no variation in the contribution from radiation, as indicated by the authors in section 5.2. In general, it would be good if the authors could expand the discussion around these terms and put the dynamic and thermodynamic contributions in better context. Furthermore, if my reading of the methodology is correct, the upper level PV anomaly and therefore its tendency, is defined between 600 and 150 hPa. Thus, the PV anomaly is defined across the tropopause interface between the troposphere and stratosphere, where a very strong vertical PV gradient and densely spaced potential temperature surfaces are present. In such a setup, the slightest heating will result in a strong response in PV, also from radiation. However, the relevance of these PV anomalies if they are across the tropopause is maybe not significant. Can the authors expand on where the respective heating occurs with respect to the PV gradients and the tropopause?

Thank you for this comment. Originally, we believed that a further discussion of the radiative tendencies is beyond the scope of our study. Based on this comment, however, we realize that some more information on the radiative tendencies is of interest here and will improve clarity. We will extend our manuscript in this regard and will consider spatial (horizontal and vertical) composites of the physical PV tendencies and the associated heating rates to discuss this point further. We will put these composites in the context of previous studies that considered radiative PV tendencies in the tropopause region (e.g., Zierl and Wirth 1997, Chagnon et al 2013, Chagnon and Gray 2015, Oertel et al. 2020).

Rossby waves constitute undulations of the tropopause, i.e., northward and southward extensions of tropospheric and stratospheric air masses, respectively. By their very nature, Rossby waves thus comprise tropospheric and stratospheric PV anomalies. The reviewer is correct that relatively small heating rates in the stratosphere may yield relatively large cross isentropic transport of PV due to the large gradients in theta and PV. To the extent that the associated PV tendencies impact PV anomalies associated with Rossby waves, these tendencies are significant for the evolution of Rossby waves.

6. Another comment on the radiation results would be that there has been significant focus on the effects of radiation, in particular related to cloud tops, on storm tracks (e.g., work by Aiko Voigt and the Cookie experiment community), and thus implicitly on cyclones on RWPs. The results of the authors indicate that the claimed impact by the aforementioned community might not be as relevant on a feature-based view and mainly reflect itself in a climatological background, which would be worthwhile to put in context.

This is another very good suggestion by the reviewer that will help to clarify the presentation of our resutls. Indeed, our results indicate that radiative PV tendencies have - on average – a small impact on the life cycle of *individual* anomalies but that these tendencies project much more substantially on the background state. The revised version of the manuscript will put our results in context as suggested by the reviewer.

7. Related to the comment above, most likely, most heating associated with cloud and rain processes occurs below 600 hPa and therefore the tendencies of upper level PV are not directly affected by the diabatic terms. However, the displacement of the theta surfaces will be felt aloft, which will manifest itself in divergence at these levels. For example, heating at levels below 600 hPa yields a mass transport above potential temperature surfaces that can be located below 600 hPa initially, but the ensuing mass redistribution will reach higher altitudes in the hydrostatic and geostrophic adjustment. The divergent signal is thus potentially largely associated with the atmosphere trying to attain balance after experiencing diabatic heating. The method employed by the authors cannot disentangle between these causes and effects. The authors should expand the discussion about these potential caveats and what they imply for the interpretation of the results.

We are not sure if we understand the reviewer correctly. In particular, we do not see the different causes and effects that need to be disentangled. It might be helpful to re-state upfront that we evaluate our PV budget on isentropic levels, not on pressure levels. Pressure levels are used only in the technical sense of performing piecewise PV inversion.

The impact of diabatic heating due to mass transport to the upper-troposphere is fully accounted for in our analysis by the divergent term. We fully agree - and emphasize in the manuscript at several instances - that this is the major impact of latent heat release on RWPs. The divergent flow contains both, the “balanced” secondary circulation associated with latent heat release and unbalanced (gravity-bore like) motion. These two components are not disentangled by our diagnostic, but we believe that it is not these two components to that the reviewer refers. In addition, as discussed in some detail in the manuscript, it is difficult to disentangle the role of diabatic and adiabatic secondary circulations with our diagnostic.

Latent heat release certainly generates PV anomalies below the maximum of heating. The balanced state associated with these anomalies, however, does not directly(!) impact the PV distribution on isentropic levels above. If the reviewer implied this impact by the wording "the ensuing mass redistribution will reach higher altitudes in the hydrostatic and geostrophic adjustment." the reviewer would not be correct. The PV distribution aloft is impacted by the diabatically generated lower-level PV anomalies indirectly(!) due to advection by the associated wind field. In L92 we explicitly state that we do not attempt to disentangle this impact, which has been the focus on many previous studies.

8. In general, I found it sometimes difficult to follow the reasoning of the authors and they sometimes also indicate that they will contradict themselves, e.g., paragraph 508-519, especially line 518. As a reader, I would appreciate a more stringent guidance through the material avoiding potential confusions and suspense to wait (at the cost that I might have forgotten until then) until further clarification in later sections.

Based on the reviewer's next comment, we will revise subsection 5.3 and thus the paragraph noted by the reviewer here explicitly. Otherwise, however, it is difficult for us to identify other parts in the manuscript that may require revisions based on this comment.

9. I find the argumentation in the ensuing paragraph also confusing (top of page 25). The authors make it sound like as if the moist baroclinic paradigm does not require a beneficial phasing of the latent heating with the overall baroclinic structure. This would be incorrect and indeed the arguments presented by the authors at the end of the paragraph are consistent with the moist-baroclinic instability paradigm, i.e., the phase relation between heating and the perturbations is closely tied to the overall baroclinic structure. Furthermore, most of what is then argued in the last paragraph before the conclusions is straight forward moist-baroclinic instability reasoning, i.e., once there is a baroclinic growth, i.e., once a westward vertical tilt of the anomalies is established in a baroclinic environment, ascent and associated latent heating occurs at a favorable location for growth. The authors thus do not present something new in this context, even though they appear to make it seem like new. Instead of presenting these findings as something new, they should rather put their findings in context with existing literature on moist baroclinic instability and relate their findings to the overall three-dimensional structure of the RWP.

This is another good point of the reviewer. Moist-baroclinic instability theory links latent heat release to the region of dry ascent in a baroclinic wave. A beneficial phasing of latent heat release is thus inherent in the theory. Our original manuscript should have noted this relation more explicitly and have put our results in this context. The mere existence of beneficial phasing during moist baroclinic growth is certainly not a new result and our original manuscript was not sufficiently clear about the new aspects of our analysis. New aspects include considering data of a large number of real cases and going well beyond the modal structure of moist-baroclinic instability theory: we focus on the ridge amplification, which is much more pronounced than that of the remainder of the (putative) moist-baroclinic mode (e.g., the trough); we consider the variability of heating and its impact at similar stages of the baroclinic life cycle; and we investigate spatial patterns of (a proxy of) latent heating and its relation to PV anomalies that may evolve in time. The revised version of the manuscript will refer to previous literature of moist-baroclinic instability and will use this context to make clearer the new aspects of our study.

Specific Comments:

163: Has this approximate equality been checked with data? I am wondering how close this relation really holds.

Yes, it was tested with data. In the first authors PhD thesis, the advection of the background by the background flow has been investigated for different time mean durations and integrated over the whole inversion domain. It was shown that this term is negligible (2-3 orders of magnitude smaller) compared to the advection of background PV by the anomaly flow.

170-171: The authors state that the aforementioned occurrences are exceptional. Why do the authors not identify and quantify the potential influence of these occurrences and the effect mid-level PV anomalies might have on their results?

The impact of these occurrences might be interesting, but their analysis is beyond the scope of this manuscript. The focus of this study is the mean perspective of wave dynamics, not the impact of exceptional occurrences.

259: There is a grammar issue with the sentence, in particular with the run-on part "and

mainly due to small-scale. . .". thanks

297-298: How can one infer the group velocity from the blue contours? Strictly speaking, given the 90 degrees shift, one can only identify the phase propagation.

This question is related to your third comment. We will make clearer in the revised version that amplification and decay of anomalies is defined by spatially integrated tendencies and will note explicitly that amplification and decay of anomalies occurs in this spatially integrated sense. At the leading edge the quasi-barotropic tendencies generate and amplify new anomalies. At the trailing edge the quasi-barotropic tendencies lead to decay of anomalies. In that sense the quasi-barotropic PV tendencies relate to the group propagation of RWPs.

377: "save"? What does that mean in this context?

„save" in this context is used as a preposition (as a synonym for except)

399: The value is not "random", as the authors state it is reflecting a more "climatological" value.

Thank you, we agree that the term random is not a good choice and will rather use the term climatological in the revised version.

560: How can adiabatic subsidence be of significance in an isentropic framework? It would basically be invisible, except if it is associated with horizontal divergence.

We here refer to adiabatic subsidence in the context of our proxy for latent heat release. This proxy is calculated in pressure coordinates and thus reference to adiabatic subsidence does make sense.

614: "tropospheric" thanks

637: Please clarify what is meant by “differences in phasing” in this context. Phasing of what? Furthermore, what aspects of the secondary circulation are meant?

“Differences in phasing” meant to refer to the relative position of latent heat release to upper-tropospheric PV anomalies. With “secondary circulations” we meant to refer to upper-tropospheric divergence associated with dry balanced dynamics. We will revise this statement, taking into further account revisions that we will implement based on your last general comment above.