Reply to Reviewer 2

General Comments:

The authors have significantly reworked the manuscript based on the feedback they received. There are still some remarks that should be clarified.

We are glad to see that our revisions clarified many of the concerns of the first review of this reviewer. We appreciate the reviewer's additional comments that will help to further clarify the presentation of aspects of the methods applied in our study.

A summary of the specific changes made in the revised manuscript are given at the end of this response document.

Regarding my comment on the partitioning of PV anomalies, which the authors regard as "out-of-the-box" thinking, their response requires some clarification. In general, one uses interior PV and boundary conditions for potential temperature on the upper and lower boundaries as well as azimuthal wind conditions in the lateral direction to perform a complete PV inversion, assuming a geostrophic and hydrostatic state. These conditions can either be set to trivial values or be prescribed making certain assumptions. In my original comment, I wondered about the implications of the assumption that the PV anomaly in the respective domain would not be associated with a potential temperature anomaly on both vertical boundaries, which it appears is what the authors are assuming. While it might be common practice, it has also been shown that this can bear significant caveats (e.g., Egger and Spengler, 2018, DOI: 10.1175/JAS-D-17-0039.1).

In their response, the authors then blend arguments of regular PV inversion, including interior PV and boundary conditions as outlined above, with PV thinking of delta sheets introduced by Bretherton (1966), where all information is condensed into delta perturbations of potential temperature, i.e., static stability anomalies, at the boundaries, which can be extended to a given amount of these discrete delta layers of these type of PV anomalies (e.g., Heifetz et al., 2004, de Vries et al., 2009). Based on my reading of the manuscript, however, the PV inversion outlined by the authors deals with interior PV in given volumes and potential temperature anomalies on the vertical boundaries, which is also what my comment and question was based on. As indicated previously, the exact inversion procedure including the assumptions should be clearly outlined and a justification of the assumptions, especially regarding the boundary conditions, would be highly appreciated.

We agree with the reviewer that inversion of interior PV in a limited domain is (in a mathematical sense) not a well-posed problem because the (lateral and vertical) boundary conditions that are associated with the balanced state, i.e., the solution of the inversion, are not known a priori. Further issues arise when performing piecewise inversion using nonlinear balance because of the nonlinearity of the balance condition. We had appreciated the mathematical inexactness of PV inversion in the original version of our manuscript at the end of section 2.2. We agree with the reviewer that our list of issues mentioned at that point is incomplete and the list in the revised version now includes imperfect knowledge of the vertical boundaries.

We further agree that the associated discussion was too brief. The revised version now includes a much-extended discussion on the vertical boundaries and a quantitative estimate of uncertainty associated with piecewise PV inversion (see below).

My comment was not about the respective PV anomaly projecting on static stability anomalies, but on the used potential temperature anomalies in the inversion, as it is these anomalies, based on my understanding of the authors' method, that are used for the inversion. Regarding my comment, it would be correct that these potential temperature anomalies are then part of the respective PV anomaly. In their response, the authors then use an argument that would refute their own assumption, where they argue that vertical velocity is negligible at the upper and lower boundary, thereby suppressing an influence of the distant PV anomaly on this surface. However, the same argument would then also apply to the PV in proximity of the boundary if it is seen as more or less rigid. Therefore, no potential temperature anomalies along the boundaries would be attributable to any PV anomalies, though one appears to happily make certain a priori choices.

If the vertical boundaries were indeed rigid, i.e., w=0 at these boundaries, interior and boundary anomalies could be clearly distinguished. (Anomalies on the boundaries can of course still exist due to advection of (boundary) potential temperature across an existing background gradient, as in the Eady model.) The distinction starts to blur when the boundaries are moved into the interior and w=0 no longer holds. In our current study, however, we do not distinguish between the lower boundary theta anomalies and the low-level interior PV anomalies (analogously for the upper levels). In our previous response to this reviewer, we had thus argued only why it is a very good assumption that the impact of the distant anomalies on boundary theta anomalies is small.

Defining the low-level PV anomaly (in the generalized sense of Bretherton (1966)) as the combination of the lower boundary theta anomalies and the low-level interior PV anomalies makes physical sense because inversion of these anomalies considers, e.g., for a cyclone the impact of the thermal wave and the diabatically generated interior anomalies in combination. Other studies may seek to distinguish between these anomalies, but this is not the goal of the current study. Due to the combination of both anomalies, there is no need to identify the theta anomaly at the lower boundary that is associated with the low-level interior PV anomaly.

The lower boundary is physically motivated, due to the existence of the Earth's surface. Moving this boundary into the interior, i.e., at the top of the boundary layer, is physically motivated, too, because one attempts to avoid boundary layer effects, e.g., the diurnal cycle of theta anomalies.

The situation is different at upper levels because there is no boundary in a physical sense. The vertical boundary there needs to be introduced when seeking a numerical solution to the inversion problem with available data. The upper-level boundary is situated in the lower-most stratosphere and thus intersects the PV anomalies associated with RWPs. To a large extent, theta anomalies at this boundary can thus be assumed to be due to the stability anomaly associated with such PV anomalies in the lower-most stratosphere. PV anomalies higher up in the stratosphere that are unrelated to RWPs (as well as those at low levels; see response to first review) may contribute to theta anomalies at that boundary by their far field effect (''action-at-a-distance''). This effect, however, depends on static stability, which is very high in the stratosphere, and the effect can thus be expected to be negligibly small. Overall, we consider our interpretation of the boundary theta anomalies as lower and upper-level PV anomalies, respectively, to be an excellent approximation (for the reasons given here and in our reply to the first review of this reviewer).

The revised version of the manuscript reflects this discussion when introducing the definition of PV anomalies in section 2.2.

A thought experiment: if one wants to infer the anomalous flow field in the lower domain base on the PV in the upper domain, actually no information of the PV in the upper domain is needed if one knows the potential temperature at the interface between the upper and lower domain as well as the potential temperature at the surface, given that one inverts zero PV in the lower domain. Thus, the main question would be how the PV of the upper domain would project on the potential temperature at these interfaces. If a priori prescribing half of that information, what is one really assessing?

We fully agree with the reviewer's reservation about the boundary condition in this thought experiment. The thought experiment nicely illustrates that the boundaries of an elliptic problem are an integral part and need to be chosen in accordance with the physical problem at hand. The thought experiment is applicable to a boundary that is introduced at say 500 hPa. However, as described above, the situation is qualitatively very different in our study.

To further substantiate the claims of the authors, sensitivity experiments could be performed by moving the vertical boundaries of the chosen pressure levels as well as varying the choices of the boundary conditions. If the interior PV really is the most significant detail, then the respective results should not depend significantly on these choices if the volumes are chosen as such that the PV that is regarded as significant is always contained in the chosen volumes. In addition, a completeness test could be performed to further substantiate the assumptions of the authors with respect to their choices, where one would check if the sum of the anomalies yields the total field, or how closely it would resemble it. If the total field cannot be satisfactorily reconstructed, this would also point to deficiencies of the method and/or choices for the inversion.

In our previous studies using piecewise PV inversion we had performed sensitivity tests and found little sensitivity of the results to the specific height of the vertical boundaries (within a physically reasonable range, namely 175hPa and 75 hPa at upper levels and 825 hPa at lower levels). We thus feel very comfortable using the setup from previous studies here. We had performed also extensive tests of the reconstruction of the upper-level winds. Errors relative to the total wind are on average very small (below 5 %). This test is somewhat incomplete, though, because the ''baroclinic'' wind component at upper levels is relatively small, too, such that small relative errors of the total wind may still affect the baroclinic interaction. In fact, we thus did include a completeness test graphically in the original manuscript with respect to the contribution of the baroclinic and quasi-barotropic wind components to amplitude evolution (shading in Fig. 7). Thanks to the reviewer, we now realize our oversight that we failed to describe this assessment of the uncertainty associated with piecewise PV inversion in the main text; we had mentioned it in passing only in the caption. On average, the relative uncertainty is small. For troughs during extended winter (Fig. 7b), however, there is consistently a relative uncertainty associated with piecewise PV inversion of 25% - 30% in the baroclinic component, if the uncertainty is split equally between the baroclinic and the quasi-barotropic contribution. Even if the uncertainty were attributed completely to the baroclinic component, however, none of the results of our study would be affected. The revised version of the manuscript now includes this description of the relative uncertainty associated with piecewise PV inversion.

There was a conflict in the response of the authors, where they in response to my comment 7. clearly state that they are arguing in an isentropic framework, though in response to my comment on line 560 they refer to a calculation and argument in pressure coordinates. The

implications of this shift of framework in argumentation should be clarified. As indicated, adiabatic displacements would be invisible in an isentropic framework, except for implied divergence between isentropic surfaces. As the authors now include a more detailed discussion of the divergence, this appears to be more directly linked, but the authors are encouraged to double check for potential inconsistencies.
We realize that our presentation of the proxy for latent heat release leaves room for misinterpretation. We do not use this proxy to calculate associated diabatic PV tendencies. Rather, we use the proxy to explore the relation of upper-tropospheric divergence with latent heat release below, i.e., in the lower to mid-troposphere. For the definition of ‘’lower to mid-troposphere” we use the pressure levels given in the text (1000 hPa - 500 hPa). The revised version now emphasizes more clearly the purpose of our proxy for latent heat release.

Regarding the authors’ response to my point 7., they claim that my comment contained an incorrect statement. This is not true and in a way the reviewers more or less merely paraphrased my comment. To further clarify, with “ensuing”, an effect, which can be referred to as indirect, of the heating was implied in my comment, i.e., the heating yields a secondary circulation redistributing mass and vorticity and thereby PV. It appears the authors are aware of that. Furthermore, the more detailed discussion of the role of the divergence relates to my previous comment and thus addresses some of my concerns, in particular the relation of the divergence to the diabatic heating.
We are glad to see that the extended discussion of the role of divergence is helpful to address this previous comment of the reviewer.

Specific changes to the manuscript:

Instead of simply writing in Sect. 2.2.: “PV anomalies are partitioned into upper-level PV anomalies including the upper-boundary θ-anomalies and into lower-level PV anomalies including the lower-boundary θ-anomalies.” we have now added a new paragraph that discusses the interpretation of theta anomalies in some depth: “The upper and lower boundary θ anomalies are included in our definition of the upper- and lower-level PV anomalies, respectively. The lower boundary θ anomalies include the thermal anomalies of baroclinic waves, e.g., warm and cold sectors of cyclones, and a contribution from the static stability anomalies associated with low-level (interior) PV anomalies. Our upper boundary is situated in the lower-most stratosphere intersecting PV anomalies that are associated with RWPs. The upper boundary θ anomalies thus include the static stability anomalies associated with these PV anomalies. In addition, the boundary θ anomalies may include contributions from distant PV anomalies. Namely, the lower boundary θ anomaly may include contributions from PV anomalies above the separation level and the upper boundary θ anomaly contributions from PV anomalies below the separation level and from stratospheric PV anomalies outside of our domain that are unrelated to RWPs. These contributions are ultimately due to vertical motion associated with the evolution of the distant anomalies (cf.\, the concept of a "very gentle 'vacuum cleaner'" in Sect.\, 4 of \citet{HoskinsEtAl1985}). Vertical motion at the upper and lower boundary, however, is strongly limited by the large static stability in the stratosphere and the closeness of the boundary to the rigid boundary of the Earth's surface, respectively. Contributions of distant PV anomalies can thus be expected to be negligibly small, and we consider our interpretation of the boundary θ anomalies as upper-and lower-level PV anomalies to be an excellent approximation."

Having given the vertical boundaries much attention, we have simply deleted "lateral" before "boundaries" when we list the sources of inexactness at the end of Sect. 2.2.

At the end of Sect. 2.2, we have added the following discussion of uncertainty in the results of our piecewise PV inversion: "Figure 7 shows how these inherent and numerical inaccuracies of our piecewise PV inversion affect the results. On average, the relative uncertainty is small. For troughs during extended winter (Figure7b), however, there is a persistent relative uncertainty of 25% - 30% in the baroclinic component, when the uncertainty is split equally between the baroclinic and the quasi-barotropic tendency. Despite this relatively large uncertainty, and even if all of the uncertainty were attributed to the baroclinic component, none of the results of our study would be qualitatively affected."

Introducing the proxy for latent heat release when discussing Fig. 3, we now write: "… consistent with the invigoration of ascent and thus upper-tropospheric divergence by latent heat release below*, i.e., in the lower to mid-troposphere*. To examine the role of *lower to mid-tropospheric* latent heat release, we consider as a proxy …" (additions in italics)
Furthermore, we have added "in the lower to mid-troposphere" to the caption of Fig. 3.