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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 #2

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Review of WCD-2020-52:

"Potential-Vorticity Dynamics of Troughs and Ridges within Rossby Wave Packets during a 40-year reanalysis period" by F. Teubler and M. Riemer

General Comments:

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

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Discussion paper

adiabatic terms, these errors should be corrected if the equation is to be maintained in the manuscript.

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?

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.

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.

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?

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.

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.

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.

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.

Specific Comments: Reference to line numbers in the manuscript.

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

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?

259: There is a grammar issue with the sentence, in particular with the run-on part “and mainly due to small-scale…”.

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.

377: “save”? What does that mean in this context?

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

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.

614: “tropospheric”

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?

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