“Towards a holistic understanding of blocked regime dynamics through a combination of complementary diagnostic perspectives”

We are grateful to the reviewers for the helpful suggestions that further strengthen the results of this study and improve the quality of the manuscript. Below, we comment on the questions and comments raised by the two reviewers and document the adjustments to the manuscript (in green). The line numbers in our comments refer to the new manuscript.

We would like to inform the editor and reviewers that after an in-depth check of the data and methods, we have found slight inconsistencies in the data which have now been corrected. We adjusted the figures and the numbers slightly changed, but overall the main message of the study, that divergent PV tendencies govern a main part in the amplification and maintenance of the PVA amplitude, remains unchanged.

Comments to Reviewer 1

I thank the authors for their revisions, and I think that the additional notes and clarifications have largely satisfied my comments.

Thank you.

- I would just like to query a bit more on the baroclinic aspect. I can see that baroclinic effects would indeed show up strongly in the v_low dot grad q_0 term, but it is not clear how exact this equivalence is - eg do barotropic processes also project on this term, and are baroclinic effects associated with the upper level PV anomalies neglected? A stronger argument here would help.

  From the PV perspective, baroclinic instability is described by interaction and the mutual amplification of upper- and lower-level PV anomalies (e.g., Hoskins et al. 1985). Our quantitative analysis is performed for the evolution of upper-tropospheric anomalies. In this sense the term v_low dot grad q_0 fully describes the baroclinic amplification of the upper-level anomaly. The reviewer is correct that we do not consider explicitly the impact on the upper-level anomaly on the low-level anomaly, i.e., the baroclinic growth of the low-level anomaly. Implicitly, this impact may be seen at later time steps as increased baroclinic growth at upper levels, but our study does not aim at diagnosing this two-way interaction process.

  In the PV framework, barotropic processes do not project onto the baroclinic term. This may be different if “baroclinic” would be defined as (w’T’)_bar (as in an energy framework) or in terms of nontrivial vertical shear. It is important to

note that different frameworks provide somewhat different definitions of processes. At this point in the manuscript, however, we refrain from discussing such differences. In response to your next comment, elsewhere in the manuscript, we will add a reference to the literature where such a discussion can be (partly) found.

The revisions aim at clarifying that the analysis is performed for upper-level anomalies and that the terms quasi-barotropic and baroclinic are considered in the context of PV thinking. In addition, we have removed potentially confusing illustrative statements about the representation of, e.g., cyclones and low-level dynamics. We have revised a paragraph when describing the PV framework in Sect. 2.2.: “It is important to note that our quantitative analysis is performed for the evolution of upper-tropospheric anomalies. From the PV perspective of midlatitude dynamics we may thus consider the impact of the upper-tropospheric anomalies on themselves (mediated by v_{up}) as quasi-barotropic dynamics, and the impact of the lower-tropospheric anomalies on the upper-tropospheric anomalies (mediated by v_{low}) as baroclinic dynamics.” (Section 2.2., Line 149ff)

In addition, after Eq. 4 in Sect. 2.2, we have added, as a reminder, the term “upper-tropospheric” when we talk about linear, barotropic Rossby waves (L. 171).

- Also I’d find it very useful to have a brief comparison (sentence or two) on how similar the baroclinic effects identified here are to those in Martineau et al.

  We thank the reviewer for this comment and agree that a brief comparison is of interest. We have added the following paragraph in our discussion section (Section 5), after having discussed the processes during onset and maintenance:

  “Baroclinic PV tendencies play a positive, but subordinate role during regime onset but become more important for maintenance. In an energy framework, and on average over many winter cases, Martineau et al. (2022) found that baroclinic energy conversion makes a leading-order contribution to the energetics of Greenland blocks. Individual tendency terms in an energy framework and a PV framework are not directly comparable (e.g., see discussion in Section 3f of Wirth et al., 2018) and a single case study cannot be compared to the mean results of many cases. It is worth pointing out, however, that our analysis focuses on the upper troposphere, whereas Martineau et al. (2022) find the strongest signal in the low to middle troposphere. In addition, we stress that our analysis demonstrates the importance of moist-baroclinic growth for the amplification of the negative PV anomaly before and during onset. It is the divergent outflow associated with moist processes below, however, that is most effective in amplifying the upper-level anomaly in our case study. We thus consider our results to be consistent with those of Martineau et al. (2022).” (L. 659ff)

- l664: eddy-straining

  Corrected the typo from ‘eddy-training’ to ‘eddy-straining’.

Comments to Reviewer 2

General comments

The paper constitutes an impressive piece of work tackling one important outstanding problem in dynamical meteorology, namely the dynamics of atmospheric blocking. Part of the problem is the lack of agreement between definitions and approaches to the problem. In this paper the authors present an attempt to integrate several of these approaches in a step towards linking the different aspects of atmospheric blocking. From this point of view the importance of the contribution is very clear as is the fit of this work into the scope of Weather and Climate Dynamics.

I give below a list of specific and technical comments that I believe can improve the paper by making it more understandable. Some of the most important comments are related to the projection onto weather regimes. In particular I find puzzling the meaning of the projection of tendencies although I suggest a way to present this that makes sense. My intention is not to impose this view, but I certainly think it would be worthwhile for the authors to expand on the explanation of their approach. Some other important comments are related to the interpretation of the results. I got confused at times and this will probably be reflected in my specific comments. To reduce confusion it might be worth being more rigorous in the labeling of the stages and substages that occur during the evolution of the case study.

Once these comments are considered I will be able to fully recommend the article for publication in this journal.

We thank the reviewer for the feedback on the manuscript and provide answers/comments to all concerns below.

Specific comments

- L83-84: I can see that the projection (correlation) of PVAs with a mean regime PV pattern will increase as a PVA co-locates over the location of the regime pattern, but it’s difficult to see that the same will occur in the case of PV tendencies. For these the regime patterns will need to form some sort of complete set, akin to Fourier modes or spherical harmonics or even EOFs, but I don’t think this is the case here, is it? Could you explain a bit more on how this approach is the right one? (This comment is related to those to Eq. 6 and L226 below).

  From the Eulerian perspective, we project the individual PV tendency terms onto the regime pattern which yields in sum the full tendency of the projection ($\partial q'/\partial t$). This approach is equivalent to that by Feldstein (2003) or Michel and Riviere (2011) who use projections of vorticity tendencies to study the evolution of streamfunction patterns. When taking the time derivative of the projection operator, the time derivative can be pulled inside of the integration (summation) and the scalar product, because these are all linear operations and the regime pattern has no time dependency.

In the introduction section, we added: “The projection of the anomaly's tendency is equivalent to the tendency of the projection of the anomaly and thus enables…” (L. 89f).

In addition, in Sect. 2.4, after Eq. 6 we added: “Note that projecting the tendencies yields the tendency of the projection P_qL because q_WR^L is constant with time and the summation and $\partial/\partial t$ can be commuted. The normalized projection is performed…” (L. 231ff)

- L114: Is the PV inversion part of the Teubler and Riemer method? If so, add a reference to their work, or make this clear when you introduce the method in Section 1.

  Thank you for this comment and indeed, the PV inversion is part of Teubler and Riemer (2021). We added the following sentence to Section 1: “A piecewise PV inversion is performed in Teubler and Riemer (2021) in order to separately consider the influence of baroclinic interaction and quasi-baroclinic propagation. The consequent distinction between quasi-barotropic, baroclinic, divergent, and non-conservative PV tendencies allows the quantification of the effect associated with dry and moist dynamics on the amplitude evolution of troughs and ridges.” (L. 69ff)

- Equation 3: No definition has been provided for v_0. Is it the result of the inversion of q_0? This needs to be explicitly stated.

  Thank you for pointing out the absence of a clear definition of v_0. The background wind field v_0 is determined similar to the background PV (q_0): Climatology for each time step (Δt = 3h) based on the time period 1980-2019 and subsequent 30-day running mean climatology centered on the respective time (+/- 15 days). Note that the wind field v_0 is balanced to a good approximation. We added the following sentences to Section 2.2: “The background wind field v_0 is obtained in the same way as the background PV field q_0 as a 30-day running mean climatology (1980–2019) centered on the respective calendar day, and is in good approximation balanced.” (L. 154f)

- Equation 6: I find it puzzling that both q^L but especially its tendencies can be projected onto the same weather patterns. I find it similar to projecting q^L onto weather patterns defined in terms of a completely different variable, e.g. original weather patterns defined in terms of Z500. Why does it work? Can you discuss more on this?

  The answer to these questions are essentially included above. The projection of the anomaly's tendency is equivalent to the tendency of the projection of the anomaly. As we are interested in how similar a given PV distribution is to a regime pattern, we use the regime patterns as a composite of PV. However, projecting onto the geopotential pattern of EuBL would give qualitative similar results as the regime pattern looks quite similar (due to PV inversion: a smoothed version of the PV pattern with reversed sign). Please see our

response to your first specific comment about how we have modified the text.

- L226: ‘The observed temporal evolution of P_{q^L} agrees very well with the diagnosed evolution of [the sum of the terms in Eq. 7’. I agree with the sentences as it is, as indeed the time derivative of Eq. 6a (for P_{q^L}) is Eq 6b, i.e. dP_{q^L}/dt = P_{\partial q^L/\partial t}. Perhaps this is the approach to take to explain why the projection of the tendencies work. Having said that, I think there is something missing between this sentence and the next. Between which two variables is the difference of 0.07 d^{-1} mentioned in the text?

  We apologize for the missing part of the sentence. The correct sentence now reads “There is a near-constant difference of $0.07d^{-1}$ between the diagnosed and observed evolution (a relative difference of 12% at onset time), which increases during the decay stage of the regime.” in the manuscript. (L. 249f)

- L244-245: I got confused here. Earlier in L240 it is said that a weaker threshold is used to increase the PVAs’ spatial extent, but then it says in L244-245 that it is advantageous to not group together individual anomalies in a single large anomaly. I find these two sentences contradictory, but I have the suspicion that I’m missing something.

  We thank the carefully reading reviewer for noticing that. In general, setting thresholds means a small subjective assessment. In the end, some information is always lost. We try to find a balance here: On the one hand we want to capture the full anomaly bounded by a threshold of 0.0 PVU so that the Boundary term (BND) stays relatively small compared to the PV tendency terms that describe the integrated PVA amplitude evolution. In the case of the Rossby Wave Packet study by Teubler and Riemer (2021), this was done with individual troughs/ridges. However, troughs and ridge areas were then truncated vertically in the longitude direction because the threshold of 0.0 PVU did not always separate two negative anomalies upstream and downstream of a trough. So we here set the threshold higher in absolute terms to avoid these long elongated PV anomaly bands that can in some cases span the full hemisphere. Visual inspection was necessary to get a feeling for a suitable threshold that fulfills both conditions (not group together anomalies vs. capture most of the anomalies’ area) and the climatological assessment of negative PV anomalies has confirmed our subjective choice objectively.

  In the revised version of the manuscript, we have restructured the paragraph accordingly: “In this context, the threshold value fulfills both conditions required: (1) It is close enough to $q' = 0$ such that the budget of the integrated PVA amplitude of Teubler and Riemer (2016) can be closed as good as possible, and (2) it is far enough away from $q' = 0$ such that the grouping of PVAs over the entire northern hemisphere by single thin filaments is avoided, which enables the investigation of single distinct PVAs.” (L. 265)

- L261-264: This paragraph needs rewriting. The term Bnd is suddenly introduced without indicating that this will appear later in Eq. 9. A similar issue occurs with the term RES, which again does not appear until Eq. 9. Perhaps the only thing required is to move this paragraph to after that equation.

  We thank the reviewer for this comment as this motivated us to introduce the equation for the total change in PVA amplitude more clearly. In addition, we decided to avoid the RES subcategory as this confuses with the term RES ($-\mathbf{v}_{res} \nabla q_0$). We additionally clearly state all terms that are involved in the total amplitude evolution. Note, that the term RES in the new version of the manuscript exclusively describes the advection of background PV by the residual wind field ($-\mathbf{v}_{res} \nabla q_0$). The boundary term (BND) should be much more clearly defined in the new version (see L.296-305).

- L474: I'm not sure I agree with the statement on the baroclinic and divergent tendencies. In the second part of the active life cycle the baroclinic tendency has an important positive contribution, whereas the divergent tendency has a small, negative effect.

  With focus on the net effect of PV tendencies within the second half of the active EuBL life cycle, we agree with the reviewer that the baroclinic tendency has more important contributions than the divergent PV tendency. However, the divergent PV tendency still shows up with a net amplifying effect on the PVA amplitude. In Lines 524ff, we have stated this now clearly.

- L489: I agree that the non-radiative diabatic tendencies are mostly positive, but I think it is worth explicitly noting that they are also very small.

  We added a sentence in the caption of Figure 10 to indicate that the y-axis range differs between the subplots (b) and (c). However, after checking the manuscript again, we think that it is not necessary to include another statement as it was mentioned clearly enough that the non-radiative diabatic tendencies are negligible compared to the advective tendencies in the beginning (L. 546ff).

- L499: The divergent PV tendency is put as the leading contribution and the quasi-baroclinic one as the secondary. However, going by the bars in Fig. 11b I would consider them the other way around, or perhaps both as the leading terms.

  Thank you for spotting this. We have corrected this in the revised version and now point to the important role of the divergent and quasi-barotropic term (L. 563-569).

- L517: '... all prominent peaks […] are associated with an increase…' This is really difficult to judge. Perhaps be more explicit on which ones you consider

the most prominent.

We have added a grey horizontal line in Figure 11a to refer to in the main text "Most importantly, all prominent peaks of the divergent PV tendency (> $3 \cdot 10^7$ PVU m^2 s^{-1}, grey horizontal line in Fig. 11a) are associated with an WCB outflow fraction of at least 20%, and vice versa." (L. 587f)

- L547: It would be interesting to expand the discussion on the case study. The authors describe that the PVAs are advected into Europe and do not build up locally. How common, and therefore, how representative such a case is?

  Recall that we investigate exclusively a single case study here. Hence, we do not plan to extend the discussion regarding this point as this is outside of the scope of the manuscript. In follow-up work, we will perform a climatological analysis of blocked regime dynamics from this quasi-Lagrangian perspective and investigate the pathways of negative PV anomalies in detail.

- Figure B1 caption: I'm not sure the description of the lines in the caption corresponds to the legend in the figure. The observed line is described as black, dashed, but there is no black, dashed line. There is also a pink line mentioned which I can't see.

  We apologize for this. Please find a revised version of this figure. Note that we have extended the figure such that it now consists of two subpanels where we show in (a) the boundary term (BND), the observed PVA amplitude change (OBS) and the diagnosed amplitude change (DIAG), and in (b) the PV tendency terms of Equation (9) that are of less importance and would have made Figure 10b too busy.

Technical comments

- L26-27: 'The weather regimes…' Move this sentence to later, possibly to current L78 once the set of patterns to be used in this work has been introduced. Otherwise, it looks a little out of place. Alternatively, you could expand on the general discussion on weather regimes to explain the differences between them, but I don't think this is really necessary here.

  The structure of the introduction is to to start with atmospheric blocking but then switch to a weather regime perspective quickly. We decided to not shift this sentence where the term 'blocked' regime is introduced to L.78, as this part constitutes the bridge between weather regimes and blocking and is necessary for the information that follows afterwards. Still we agree with the reviewer that the actual location of the sentence is not ideal and rearranged some sentences in the first bit of the introduction for smoother transitions.

- L90: When you say 'now generally' do you actually mean 'here' as in 'in this

work'?

Yes! We changed it from "now generally" to "here".

- L98: I suggest putting 'by combining the three perspectives' between commas.

  Done.

- L105: Add 'the' between 'is' and 'content'. You could consider 'constitutes' instead of 'is'.

  Thank you, we have changed the sentence from "... is content of Sect. 5" to "... constitutes the content of Section 5".

- L118: Just for clarification, are the data valid at 2, 3, and 4 UTC taken from the forecast starting at the previous analysis time, i.e. in the example from the 0 UTC forecast?

  The temperature and wind tendencies from ERA5 on model levels are available hourly data and are just available from ERA5 forecasts (see here: https://confluence.ecmwf.int/display/CKB/ERA5%3A+data+documentation#heading-Meanratesfluxesandaccumulations). For these mean tendencies, accumulations are performed over the hour (the accumulation/processing period) ending at the validity date/time and are expressed as temporal means, over the same processing periods, and so have units of "per second". For example, data valid at 2 UTC includes the accumulation from 1 to 2 UTC, which finally is expressed as a temporal mean then. We decided to not make changes regarding this in the manuscript.

- L126: Should it say 'isentropic wind vector'?

  Yes, we changed it from 'wind vector' to 'horizontal isentropic wind vector'.

- L129: What are the sources and sinks of non-conservative momentum apart from friction?

  Gravity wave drag can be another source/sink. We have added this information: "...and **v (dot)** the sources and sinks of non-conservative momentum (e.g., friction or gravity wave drag)." in Line 137.

- L133: Delete 'mathematical expression used here:'

  Removed.

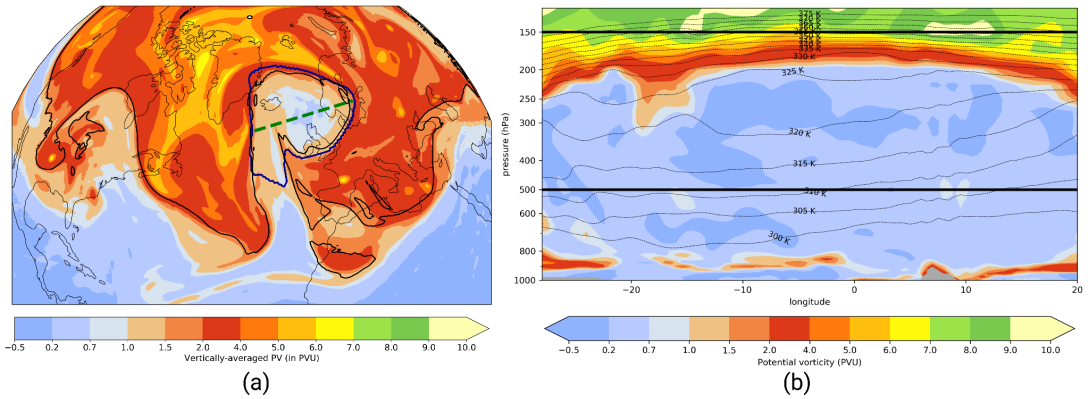
- L240: I'm not sure what 'respectively' indicates here? What corresponds to what? I think it could be deleted without affecting the meaning of the text.

  Deleted.

- L259: What isentropic levels are those mentioned in this line? Are those contained within the 150-500 hPa layer? Please clarify.

  We follow the recommendation of Röthlisberger et al. (2018), where the isentropes with maximum mean PV gradients in the midlatitudes are identified separately for all months in ERA Interim (1979-2016). For our case study in March, we selected the isentropic levels 315, 320 and 325K.

  Regarding the question if those isentropic levels are contained within the 150-500 hPa layer Figure R1b showing a cross section through the PV anomaly investigated in our study (shown in Fig. R1a) confirms the location within the layer.



(a) (b)

Figure R1. (a) Vertically-averaged PV (500-150hPa, shading, in PVU) for 13 March 2016 18 UTC (maximum stage for European Blocking regime life cycle) Thick black line is the 2 PVU contour at 325 K. The identified PVA_qL associated with the European Blocking regime life cycle is shown by the dark blue line. (b) Vertical cross section for the location shown in (a) as green dashed line. PV is displayed in shading (in PVU), contours show potential temperature every 5K. Grey shading points to orography. The two horizontal lines at 500 and 150hPa demonstrate the region used for the vertically-averaged PV calculation.

- L262: Change 'residuum' to 'residual'.

  Changed.

- L274: Should it say 'v . nabla q_0' instead of 'v . q_0'?

  Yes, thank you for this! We changed it to v' nabla q_0.

- Figure 5: The purple lines appear as red/magenta in my screen and hard copy. If possible change perhaps to a darker purple or a different more contrasting colour. The WCB trajectories partially obscure the fields underneath due to the vast amount of them. Perhaps plotting less of them could make the figure clearer? Just to clarify, these trajectory intersection points do not all belong to the same WCB, do they?

  We changed the colors in this Figure to better illustrate the difference between negative and positive divergent PV tendencies. We also reduced the number of WCB intersection points (marked as black crosses). Multiple trajectory intersection points can belong to the same WCB 'feature' but are here shown as intersection points of the trajectory at 320K that fulfills the WCB conditions (ascent of at least 600hPa within 48 hours).

- L322-323: How is the amplitude measured in PVA? Should it say 'measured by the maximum value within the PVA'?

  Yes, the amplitude is measured in terms of the maximum value within the PVA area. We changed the sentence from "The troughs upstream and downstream of the ridge also increase in amplitude (measured in PVA)." to "The troughs upstream and downstream of the ridge also strengthen as manifested by the increasing PV anomaly magnitude within these areas." (L. 350ff)

- L365: 'We already anticipated' gives the idea that something was predicted. I'd suggest changing it for 'We have already shown'.

  Changed.

- L372: I think it should be Fig. 6b.

  Thanks for spotting this typo that we have corrected.

- L375: Change 'an' for 'a'.

  Changed.

- Figure 6 caption: Define DIAG.

  Done. We included the sentence "The full diagnosed tendency (DIAG) is displayed as the sum of all the terms included in Eq. 7 (black curve)." in the figure caption.

- Figure 8 caption: Two queries here: (1) In what sense is there a centre of mass? Should the tracks be defined in terms of centroids, or is some atmosphere mass or density involved in the location of the tracks? (2) How is the 'mean frequency' defined? I understand it is some sort of measure of

persistence but I'm not quite sure what it is? (See also comment to L413 below).

(1) For each time step (3-hourly), we determine the center of mass position (lat/lon point) of the PV anomaly. The track displayed shows the straightforward connection of these individual center of mass positions. An alternative would have been to determine the centroids as geometric centers, but we decided to determine the center of mass where the PV anomaly field is used as 'mass'/weight. (2) The mean frequency is defined as the number of time steps per grid point when the traced PV anomaly is located at this grid point divided by the total number of time steps. A value of 0.5 indicates the spatial presence of the PV anomaly for 50% of the time.

We have slightly adjusted the manuscript by adding more information in the figure caption and also in the introduction text of Section 4.2 (L. 440f)

- Figure 8b: It would be useful to add dates, or just the day number, to selected events mentioned in the main text.

  Done. We have labeled the splitting/merging events, that are explicitly mentioned in the text, in Figure 8b with capital letters (and numbers).

- L413: What is the 'frequency maximum'? What is it that it's being counted?

  The frequency shown in shading in Figure 8a shows the footprint of the PV anomalies lifetime. Thereby we calculate the frequency of this PV anomaly at each grid point based on its full lifetime from March 4 to March 25. For example a frequency of 60% at a grid point means that the PV anomaly overlapped this grid point in 60% of its lifetime. We have adjusted the figure caption: "The mean frequency of the PVA^{-}_{qL} during the time of the track (= percentage of the PVA^{-}_{qL} lifetime on which the PVA^{-}_{qL} overlaps a grid point) is shown in gray shading (in \%)."

- L421: There are two jumps in the track, one northwards and one southwards. I assume the one discussed here is the one southwards…?

  Absolutely true. First, a northward jump of the track occurs due to a merging event (see green dot in Fig. 9a near the southern tip of Greenland). The more prominent event takes place shortly after that where the northern part of the PV anomaly splits off. What remains is the southern part of the anomaly that we follow towards Europe. The southward jump is associated with this splitting event. We added the word 'southward' to the sentence in the manuscript to indicate which event we talk about (L. 458)

- L450: Do you mean 'upstream'?

  The sentence was expressed a little inaccurately and may therefore cause confusion. We have decided to change the sentence to "Quasi-barotropic PV

tendencies govern the very early amplification of the PVA^-_{qL} (Fig. 10b), suggesting that the trough sitting over the eastern part of the U.S. makes an important contribution to the formation of the incipient PVA^-_{qL} through northward advection of low PV air into the downstream region." (L. 493f)

- L455 and L457: There is slight repetition between these two lines on the amplification of the PVA.

  Thank you for spotting this, we have removed this repetition.

- Figure 10: The PVA_qL^- phase could be indicated in the figure with vertical lines.

  Done for Figure 10 and Figure B1.

- L468: Is the change in the character of the baroclinic tendency from negligible to persistently positive associated with a particular synoptic system or event?

  That the baroclinic term changes from almost negligibly small to consistently stable positive was already captured in the climatological composite study of Rossby Wave Packets by Teubler and Riemer (2021). The sequence of first a peak in the quasi-barotropic PV tendency term and then an increase of the baroclinic term is called 'downstream moist-baroclinic development' and investigated in Teubler and Riemer (2021)'s Section 5.1 and 5.2. It is difficult to say if this is related to a specific synoptic system/event, but it is becoming clear that with increasing positive baroclinic PV tendencies, some shift from temperature wave at the ground to PV distribution in the upper troposphere is becoming apparent, leading to amplification of the negative PV anomaly at an ideal phase shift.

  With focus on March 18, 00 UTC in this study (Fig. R2), we see positive temperature anomalies (shading in b) near the surface beneath the upper-tropospheric PV anomaly associated with the EuBL regime life cycle. This warm anomaly is associated with a cyclonic wind field near the surface. Associated with anomalous southerlies on the eastern flank of the PV anomaly and temperature anomaly, baroclinic PV tendencies (Fig. R2a) are negative (yellow lines) over most of the PV anomaly's area and therefore lead to an overall amplification.

  We have added the following sentence to the manuscript (L. 527ff): "This positive contribution of baroclinic PV tendencies leading to the amplification of PVA^-_{qL} indicates incipient downstream moist-baroclinic development (more on this in Sections 5.1 and 5.2 in Teubler and Riemer, 2021)."

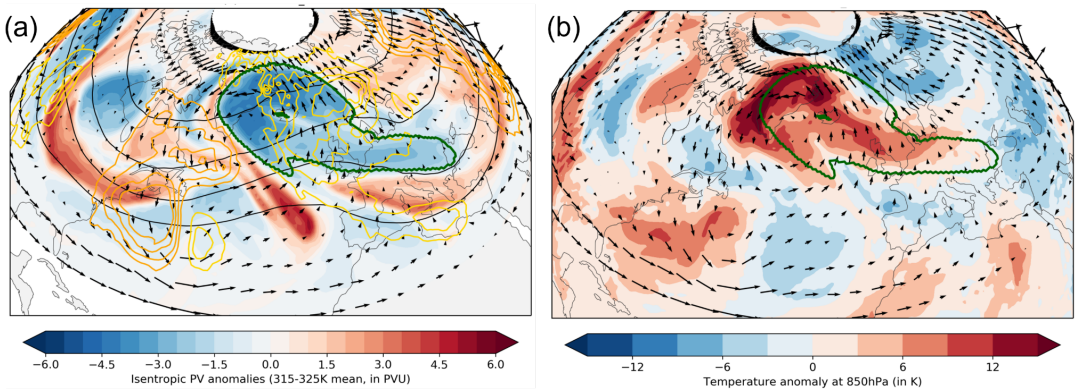


Figure R2: (a) Isentropic PV anomalies (315-325K mean, PVU, shading), PV contours for same isentropic level selection (black solid, levels = [2,3,4,5] PVU), baroclinic PV tendencies (- $\mathbf{v}_{low}$ ∇q_0) as contour lines (yellow: negative, orange: positive) and the quasi-Lagrangian PV anomaly associated with the EuBL regime life cycle (green contour line. Wind vectors show the wind field $\mathbf{v}_{low}$. (b) Temperature anomalies at 850hPa based on 30-day running mean climatology (1979-2021, shading in K). Wind vectors and green contour line: see description of (a).

- L470: Looking at Fig. 11c, I think it should say 'with only a small negative net impact'.

  As mentioned in the beginning of this document, a correction for the data was necessary which led to small changes in the analysis. As a result, the net effect of divergent PV tendencies in the second part of the active EuBL regime life cycle (Fig. 11c) is now positive. Still, the divergent PV tendency is not the main contributor in the second part of the active EuBL regime life cycle so that the corrections do not change the key messages of the manuscript.

- Figure 12: It would be worth noting that the red curve is the same as that in Fig 10b. The authors could also note that the NON-CONS curve in Fig. 10c is the same as that in Fig. 10b, although of course this is more obvious.

  We already had this information included in the caption of Figure 12: "(a) Evolution of divergent PV tendencies from Fig. 10b (red line)...". We additionally mention now that the NON-CONS curve in Fig. 10c is the same as in Fig. 10b.

- L586: I think it should say 'However, a few [...] events…' rather than 'However, few [...] events…' As it is written, the idea conveyed in my opinion is that the splitting events are not important at all.

  Thank you. We added the 'a' in front of 'few'.

- L604: Add 'the gap' between 'bridge' and 'between'.

  Added.

- L645: Delete ‘also’.

  Deleted.

References

All references labeled here are included in the reference list in the manuscript.