

# A quasi-Lagrangian perspective on the role of dry and moist processes in the formation of blocked North Atlantic-European weather regimes

Hauser et al.

We thank the editor and the three anonymous reviewers for their constructive feedback on our manuscript. Please find our replies to the comments and questions raised below.

## Reviewer 1

### Overview:

This paper examines the roles of moist and dry processes associated with four types of ridging and blocking events affecting the Euro-Atlantic region. The manuscript is well organized and clearly demonstrates that moist processes drive the initial amplification of the blocks (particularly for European blocking and ridging), while dry processes appear to be responsible for “reconfiguring” the blocks into their termination point. The methodological approach builds directly on the authors’ previous work, and as such, I have no major questions regarding the methodology. Notably, the framework is expanded to a broader class of blocking events and is presented in a compelling way that highlights the growing importance of moist processes for blocking and their potential to address longstanding challenges in representing Euro-Atlantic blocks. The authors also do a really good job contextualizing the work regarding current modelling challenges.

We thank the reviewer for this overall positive feedback on the manuscript.

Overall, the results are novel and meaningfully extend upon prior work. The paper is a good read and very well organized. While I have no major concerns with the manuscript, I offer a few minor comments for consideration.

### Minor Comments:

1. Line 130: While the splitting and merging components of the algorithm are discussed in detail elsewhere. Please consider rephrasing handling “splitting and merging events appropriately” with at least a simple qualitative sentence or two summarizing how splitting and merging is defined.

In the new version of the manuscript, we added the following: *“All PVAs<sup>-</sup> are tracked using an overlap-based tracking algorithm that also accounts for feature splitting and merging. When a merge occurs, instead of assigning a new tracking ID, which would fragment a single life cycle into a disjointed “puzzle” of multiple IDs, the algorithm retains the tracking ID of the larger feature and archives the metadata of the smaller merging feature. A similar strategy is applied when features split. This approach enables the reconstruction of complete life cycles of PVAs<sup>-</sup>.” (L.132 ff).*

2. Line 161: I find the discussion of terms in the budget equation a bit difficult to grasp. LOW is mainly baroclinic (dry?), what do you mean by mainly?

We thank the reviewer for pointing out this ambiguity. The term "mainly" was used to acknowledge that while low-level PV anomalies are often enhanced by moist processes, the interaction itself is described by balanced, adiabatic dynamics. To clarify this, we have revised the manuscript to distinguish between the kinematic nature of the term and the physical drivers of the anomalies. We now define LOW as the "quasi-adiabatic, balanced interaction" between levels. This clarifies that while moisture indirectly strengthens the low-level PV anomaly, the LOW term specifically quantifies the resulting advection of background PV, keeping it conceptually distinct from the direct moist-divergent effects captured by the DIV terms.

In the modified version of the manuscript, we have adjusted the explanation of LOW: *"The term **LOW** captures baroclinic interaction, namely the amplitude modification of upper-level PV anomalies by the wind field associated with low-level PV anomalies (gold arrows in Figure 1b), thereby contributing to baroclinic growth when the upper- and lower-level anomalies are suitably phased (e.g., Hoskins et al., 1985, Section 6b). Moist processes may strengthen the low-level PV anomaly, but LOW represents the quasi-adiabatic, balanced response to that anomaly rather than its diabatic generation."* (L. 163 ff).

3. Line 170: This is perhaps my own misunderstanding, but any additional clarification in the text would be great. NONCONS is argued to not be a significantly important process in the overall budget. And I agree, given the budget is intended for large-scale dynamics, and the strongest diabatic tendencies are likely occurring within portions of block amplification associated with localized deep convection. But how does the budget account for rapid upscale (multi-scale) growth of locally generated NONCONS potential vorticity? (i.e., Oertel et al., 2021 showing vertical shear growing NONCONS potential vorticity onto large-scales). Would this process be tied into one of the other components of the budget? I would be careful to argue against the lack of importance of this process without citation/proof. I think my personal concern is whether the budget approach, by design, does not account for upscale interactions associated with the NONCONS term. I do not think this needs to be discussed much in the text, but at the very least consider rephrasing the terminology in the text that it is 'unimportant'?

We appreciate this insightful comment regarding the upscale impacts of diabatically generated PV anomalies. From the perspective of forecast-error growth it has been argued that the upscale impact of (convective-scale) latent heat release is dominated by divergent motion (Zhang et al. 2007, Baumgart et al. 2019, Selz et al. 2022). While the specific pathway to such upscale impacts are case dependent (Groot and Riemer 2026), this route to upscale impacts is included in the divergent terms of our PV budget. Certainly, however, the reviewer is correct that re-arrangement of diabatically generated PV-anomaly dipoles as investigated in detail in Oertel et al. (2021) is a further viable pathway. As demonstrated by Oertel et al., such PV re-arrangement can impact the meso-(alpha)-scale structure of a ridge and the adjacent jet. Recall, however, that our PV budget is designed to quantify the integrated amplitude of negative PV anomalies. The generation and re-arrangement of PV-anomaly dipoles *within* such a negative PV anomaly does not impact on this amplitude metric. Upscale impacts would be seen in (small) modifications of the PV budget terms that

include the upper-level PV anomaly and the associated winds, respectively. While the upscale impact described, e.g., in Oertel et al. (2021) is certainly of importance for forecast errors and model (and hence analysis) biases, the impact on the overall integrated-amplitude evolution of PV anomalies can be expected to be subordinate compared to the other mechanism that more directly captured by our PV diagnostic.

The reviewer is correct that “unimportant” is too strong of a qualifier for NONCONS. In the revised version, we adjusted our phrasing in a more understandable way: “*The term **NONCONS** represents direct diabatic modification of PV by non-conservative processes such as latent heating, radiative effects, friction, and turbulent mixing (e.g., Chagnon et al., 2013). While diabatic PV tendencies can be locally strong—particularly in association with deep convection—their spatially localized nature implies that their contribution to the area-integrated PV budget is typically small (cf. Teubler and Riemer, 2016; their Figures 9 and 10). Radiative PV tendencies, although more spatially coherent, exhibit little spatio-temporal variability over the life cycle of upper-level PV anomalies (Teubler and Riemer, 2021). Diabatically generated PV anomalies may nonetheless influence the large-scale flow indirectly through rapid upscale interactions, for example via divergent circulations, which are represented by other terms in the PV budget. NONCONS processes are therefore not expected to substantially affect the integrated amplitude of PVAs considered here. Following previous studies (e.g., Teubler and Riemer, 2021; Teubler et al., 2023; Hauser et al., 2024), NONCONS is not analyzed explicitly in the present study.*” (L. 178 ff).

4. Line 186: Maybe just to help with context, but what are the situations where the budget fails to match observations? Is it random or are there particular flow configurations?

The budget fails to match observations in situations when (i) a major splitting or merging events occurs, which leads to a strong reduction/increase in area, or (ii) the anomaly contour based on the vertically-averaged PV anomalies fails to encompass also isentropic-level based negative PV anomalies, bounded by the  $q' = 0$  line.

We added the following sentence to the manuscript: “*Such large DIAG–OBS deviations primarily occur during non-linear evolution phases, such as large-scale splitting or merging events, or during periods when the VAPV anomaly contour does not fully capture isentropic-level PV anomalies.*” (L. 205 ff).

5. Figure 5: Apologies, I find the table quite difficult to interpret. I would suggest the authors to provide a simple summary sentence introducing the table or perhaps adding an additional qualitative description to the caption.

In the revised version of the figure, we have added “less frequent” and “more frequent” to the two ends of the colorbar in panels (b) and (c) to make it clearer. We also included the following sentence in the respective text, when discussing these panels: “*Figures 5b and 5c show deviations in transition frequencies from the climatological mean for the upstream and retrogression pathways, respectively (positive values indicate higher-than-average frequency).*” (L. 305 ff). The figure caption has also been adjusted by adding this sentence: “*Each row represents a blocked regime, and each column shows the share of cases (%) with the respective preceding regime.*”.

6. Figure 11: More for your consideration. But the shaded WCB imprints are overlapping with each other. Have you considered shading negative and positive div (since they are quite separate from each other) and contouring the WCB imprints? Another idea is to have a single additional contour for WCB inflow, ascent and outflow to complement the shaded plots but overcome the issue of the shaded components of the WCB overlapping with each other. My concern with the current figure is that it misrepresents the 'inter-connected' nature of the WCB.

Thank you for your suggestions and for highlighting this issue. We have decided to retain shading for the WCB footprints and contour lines for  $DIV_{div}$  in Figure 11. To address the concern about overlapping WCB regions, we have added fine lines for different levels of the WCB footprint, making the overlaps more clearly visible while maintaining the overall representation of the WCB structure.

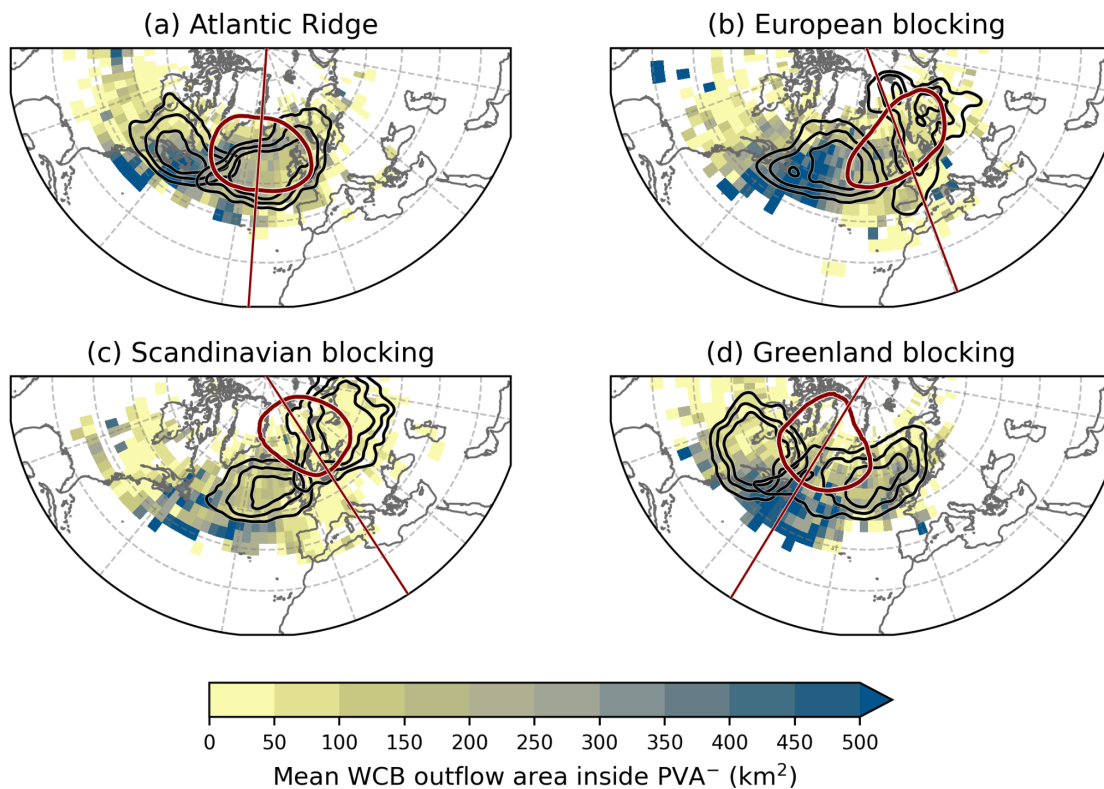
7. Figure 12: Apologies if I did not catch this, but the numbers in the legend of the figure, is this referring to the area size of WCB ascent and outflow? Perhaps mention this in the caption.

The numbers in the legend of the original Figure 12 referred to the area size of WCB ascent in panel (a) and WCB outflow in panel (b). However, upon further consideration, we decided to revise this figure because its original design made it difficult to convey the main message we intended to highlight—namely, the link between the pathways of onset PVAs<sup>-</sup> and the locations where onset PVAs<sup>-</sup> are co-located with WCB activity, particularly WCB outflow. In the original Figure 12, the WCB outflow/ascent area within onset PVAs<sup>-</sup> was shown for all blocked regime types and all pathways in a single panel. As a result, information about individual regime types and pathways was not clearly distinguishable. In addition, we found no strong justification for showing both WCB ascent and WCB outflow, as they yielded very similar results.

We therefore split the original Figure 12a into separate panels for the four blocked regime types and removed the original Figure 12b. In the revised figure (see new Figure 12 and Figure R1 in this document), we show the mean WCB outflow area within onset PVAs<sup>-</sup> separately for each regime type. The corresponding discussion paragraph has been adjusted accordingly (L. 485-504). Importantly, the key takeaways remain unchanged; rather, we believe they are now more clearly and directly conveyed with the revised figure.

8. Line 504: 'strong amplify' -> 'strongly amplify'

Thank you, we changed this accordingly.



**Figure R1** Replacement Figure for Figure 12 in the first submitted version of the manuscript: Mean WCB outflow area (km<sup>2</sup>) within the onset PVA<sup>-</sup>, calculated over 3° × 3° latitude–longitude grid boxes for all onset PVA<sup>-</sup> in the time window from –6 days to onset, shown separately for each blocked regime type: (a) Atlantic Ridge, (b) European blocking, (c) Scandinavian blocking, and (d) Greenland blocking. Each onset PVA<sup>-</sup> is assigned to a 3° × 3° grid box based on the location of its center of mass, and grid-box means are shown only where more than four PVA<sup>-</sup> are available. Black contour lines indicate the occurrence frequency of onset PVA<sup>-</sup> (0.2, 0.25, 0.3) along the pathways at a time lag of –3 days before onset (see Figure 4). The closed dark red contour denotes the regime mask (cf. Figure 2), and the red line marks the longitude used to separate the pathways.

## Reviewer 2

This study looks at blocking in the North Atlantic-European sector from two different perspectives: an eulerian one, based on a well established weather regimes approach, and a lagrangian one, by tracking anticyclonic potential vorticity anomalies. The work highlights that several paths exist to create blocked regimes and a PV budget approach highlights the contributions of a range of different processes. I enjoyed reading the manuscript and learned a lot from it - I think it would be of interest to Weather and Climate Dynamics readers and the community at large. The manuscript is well reasoned and well written. I only have a few minor suggestions for the authors to consider.

## General comment

The manuscript is well written and well reasoned. However, I found some of the diabatic process links somewhat tenuous at times. Figures 6 and 7 perhaps make this case nicely. However, it is then argued that diabetics are important for retrogressive regimes, when most figures show the reverse. For example, consider the critical role that moist processes play in retrogression pathways. However, the relevant figures do not always necessarily support that in my view (see 8 and Figure 11 for example). In addition, Figure 9 shows a different perspective from say Figures 6 and 7 on which diabatic processes are perhaps equally as important as boundary and baroclinicity (UP). Perhaps discussing this in some more detail will help to clarify for the reader.

Thank you for this overall positive feedback. We apologize for not making the link to diabatic processes and their relative importance clear enough in our original draft. The revised version states the contribution more clearly, while taking into account the differences to the contributions by other tendency terms.

Specific comment for consideration:

- Line 57-58: Confusing sentence, consider rewording.

Thank you for raising this. We changed the sentence to: *“Oceanic blocks are characterized by a consistently strong diabatic contribution, while continental blocks, especially over Asia, show marked seasonal variability.”* (L. 56 ff)

- Lines 127: PVAs already defined above?

Yes, we defined this already in the introduction. Thanks for making us aware of this.

- Line 263-265: I know what you mean here but it reads fairly clunkily. Consider rewording this paragraph.

Thank you for spotting this. We reworded it in a clearer way now: *“The location of onset PVAs- in the days preceding blocked regime onset depends on the regime type. For North Atlantic-centred blocked regimes (Figure 4a,d), both pathways are predominantly oceanic. In contrast, for blocked regimes over Europe and Scandinavia (EuBL and ScBL), retrogression pathways (orange shading) remain confined to continental regions, while upstream pathways (violet shading) extend over the ocean (Figure 4b,c).”* (L. 284 ff)

- Lines 318-320: Where do I see the onset PVAs amplifying to day t+1? I am not sure which result you are referring to here? All amplifications in Figure 6 occur before the onset day.

We apologise for the lack of clarity. In Figure 6a, the black line shows the diagnosed change in amplitude between consecutive time steps; positive values indicate amplification, while negative values indicate decay. The curve crosses zero at day +1 relative to blocked regime onset, indicating a transition from amplification to

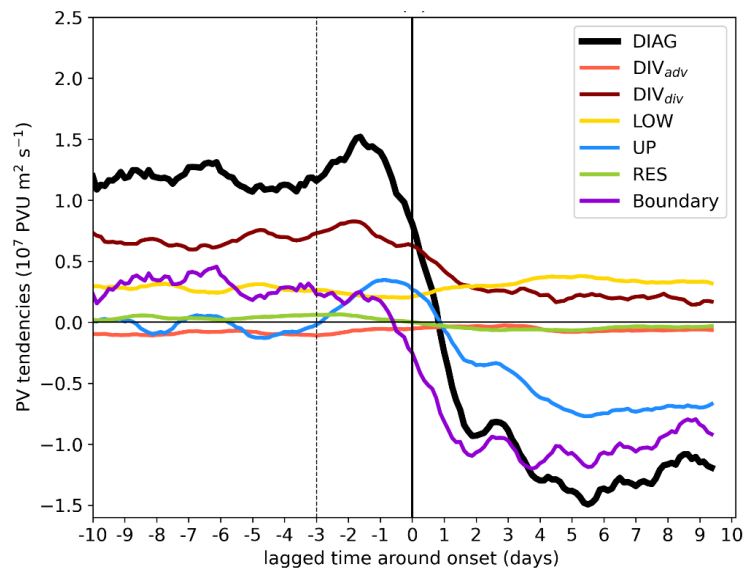
weakening in amplitude. This implies that the maximum amplitude is reached at day +1, even though the amplification itself occurs prior to the onset.

We realised that the way the figure was introduced in Section 3.3.1 may have been misleading. To clarify this point, we added the following sentence: “*The curves show the diagnosed change in amplitude and its contributing terms.*” (L. 337 ff).

- Lines 321-323:  $DIV_{div}$  is certainly the most positive contribution. But is it positive the entire pre-onset period? How long back do you need to go before it (and DIAG) become neutral? Additionally, DIAG amplifies in the days leading to onset (when the composite comes more into focus) without much movement in  $DIV_{div}$ ? Do diabatic processes explain this accelerated amplification? What are the major contributions to the increases? Can this be quantified?

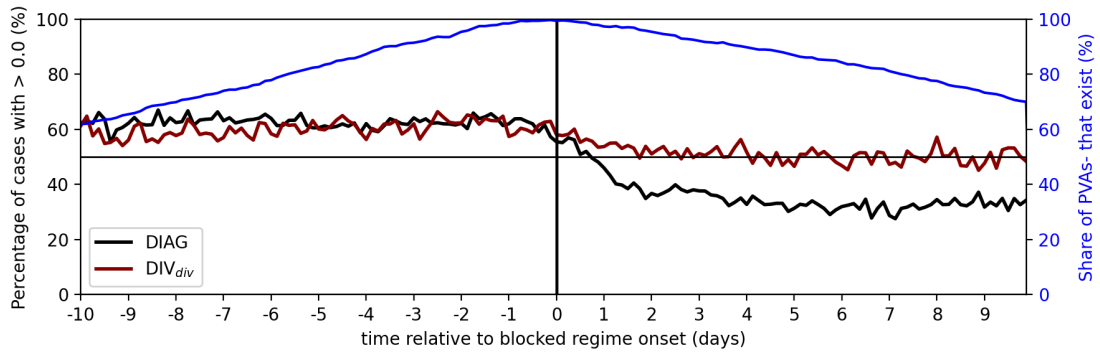
We thank the reviewer for these insightful questions.

In the mean picture,  $DIV_{div}$  provides the largest positive contribution when considering all onset PVAs<sup>-</sup> across all blocked-regime life cycles prior to onset (see Figure 6a). This result remains unchanged when extending the analysis further back in time (up to 10 days before onset; see the dark red line in Figure R2).



**Figure R2:** Same as in Figure 6a in the manuscript, but shown for the full period covering the days +/- 10 days around blocked regime onset.

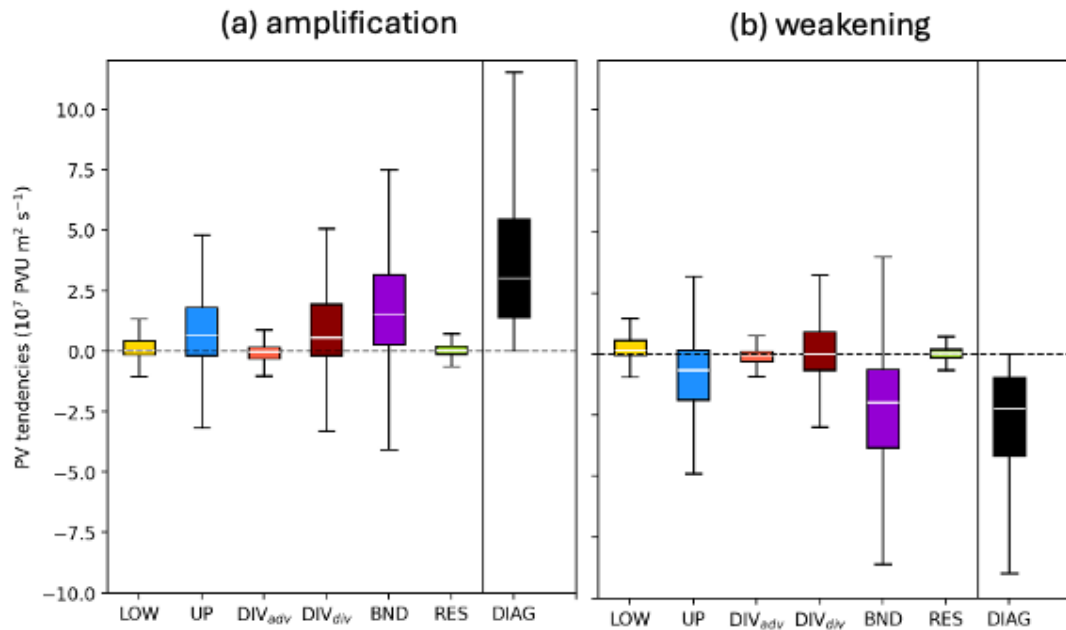
Going back in time, DIAG shows little change in the mean (Figure R1, black line), such that DIAG never gets neutral in the mean. Regarding the case-to-case variability in the sign of area-integrated  $DIV_{div}$  and the total amplitude change DIAG of the onset PVAs<sup>-</sup>, we show here Figure R3, illustrating the share of cases with positive  $DIV_{div}$  contributions and an amplification ( $DIAG > 0$ ). Prior to blocked regime onset, about 60% of all onset PVAs<sup>-</sup> experience amplification at each time step, which is consistent with the fraction showing amplification associated with  $DIV_{div}$ . In summary, while the mean evolution indicates persistent amplification by  $DIV_{div}$  over the analyzed period, considerable variability can still exist among individual cases.



**Figure R3:** Percentage of onset PVAs- showing a positive diagnosed amplification ( $DIAG > 0.0$ ; black) and percentage of onset PVAs- showing a positive contribution of  $DIV_{div}$  ( $DIV_{div} > 0.0$ ; dark red). The blue line shows the share of PVAs- that exist at the respective time lag.

Finally, to quantify the individual contributions, Figure R4 shows the budget terms for time steps during which an onset  $PVA^-$  amplifies prior to onset (panel a) and for periods during which an onset  $PVA^-$  weakens in the days leading up to onset (panel b). During amplification phases (Figure R4a),  $DIV_{div}$ , UP, and Boundary emerge as key contributors, although substantial case-to-case variability is evident. The contribution of  $DIV_{div}$  is associated with moderate upper-tropospheric divergent winds. In contrast, during weakening phases, when the diagnosed amplitude is negative (Figure R4b), UP and Boundary dominate the budget, while  $DIV_{div}$  remains close to zero. Notably, during the 2–3 days preceding onset (when  $DIAG$  reaches its largest positive values, indicating the strongest amplification), the dominant contribution arises from UP rather than from  $DIV_{div}$ , LOW, or Boundary.

We decided to only slightly adjust the wording in the first paragraph of Section 3.3.1 (the regime-centred perspective; L.337-351) and parts of Section 3.4 (L. 449-484; see track changes for more details on what has been changed).



**Figure R4:** Contribution of different PV tendency terms for all time steps with (a) a positive DIAG (strengthening of amplitude) and (b) a negative DIAG (weakening of the amplitude). The white line shows the median.

- Lines 339-340: Does the amplification of EuBL and AR over the high latitude regimes not also a reflection of their latitude? They would have easy access to low-PV tropospheric air as they are situated close to or the mean jet axis. Is this also related to their size?

Thank you for this insightful comment. The smaller size of diagnosed upper-tropospheric PV anomalies at higher latitudes and their comparatively weaker amplification are physically consistent with the background-state dependence of PV and our methodological choices. Climatological upper-tropospheric PV and its meridional gradients increase toward higher latitudes. As a result, applying a fixed PV-anomaly threshold for the Northern Hemisphere tends to identify more compact features poleward, whereas broader anomalies are required to exceed the same threshold at lower latitudes. In addition, the use of a fixed 500–150 hPa layer implies a shallower physical depth at higher latitudes, which can further smooth and limit the apparent spatial extent of PV anomalies.

The stronger amplification rates diagnosed at lower latitudes are also expected, as baroclinicity and diabatic processes are generally more pronounced on the equatorward side of the jet, and weaker background PV gradients allow anomalies to grow and expand more efficiently. We therefore interpret the observed latitudinal differences as reflecting both dynamical factors and the influence of the diagnostic framework, rather than indicating a reduced dynamical relevance of high-latitude PV anomalies.

We adjusted the text: *“One difference among the regimes is the rate of onset PVA-amplification. Low-latitude regimes — defined here as regimes whose onset PVAs-*

*are typically located at comparatively lower latitudes — (AR, EuBL) tend to amplify more strongly than high-latitude regimes (ScBL, GL). This behaviour is consistent with weaker background PV gradients and more favourable conditions for baroclinic and diabatic amplification at lower latitudes, and may also partly reflect the larger diagnosed spatial extent of PVAs<sup>-</sup> in these regimes given the use of a fixed upper-tropospheric layer and PV threshold (not shown).” (L. 360 ff)*

- Figure 7: I found the grey DIAG bar hard to see. Perhaps consider a grey line across the other bars to mark the height of the grey bar.

Thank you. In the revised version, we increased the width of the grey bar while simultaneously decreasing the width of the stacked contribution bars. This adjustment makes the grey bar much easier to distinguish.

- Figure 8: I was perhaps surprised that the PV tendencies did not change on each dipole between the upstream and retrogression cases, given that they are both moving opposite directions.

Thank you for raising this point. Although the upstream and retrogression pathways are associated with opposite directions of motion, this does not imply that the PV tendencies on each dipole should reverse sign. The PV tendency reflects the local generation and destruction of PV, which is primarily controlled by diabatic processes (e.g., latent heating) and by the deformation and vertical circulation associated with the large-scale flow, rather than by the direction in which the anomalies propagate. In both pathways, the dipole structure remains embedded in a similar synoptic-scale environment, so the sign and spatial pattern of PV tendencies on the cyclonic and anticyclonic parts of the dipole are comparable. Propagation is governed mainly by how existing PV anomalies are advected and reorganized by the flow, whereas the PV tendencies diagnose how the anomalies are locally amplified or weakened.

- Lines 365 - 367: How can two objects merge without increasing the overall size of the merged object? I was confused by this statement.

Thank you for pointing out this ambiguity. We agree that the original wording was confusing. The boundary term being negative does not automatically imply a decrease in total amplitude. It just measures PV flux across the feature's boundary, so if weaker “fringes” of the anomaly split off, the boundary term is negative, but the core PV can continue to intensify, resulting in overall amplitude growth. This explains how GL can show a negative boundary contribution yet still experience growth in the onset PVA<sup>-</sup>. We adjusted this in the manuscript: *“This can be explained by the way the boundary term is defined. The boundary term measures PV flux across the feature's boundary, so if weaker “fringes” of the anomaly split off, the boundary term is negative, but the core PV can continue to intensify, resulting in overall amplitude growth. This explains how GL can show a negative boundary contribution yet still experience growth in the onset PVA<sup>-</sup>.” (L. 390 ff).*

- Lines 436 - 447: Would it be valuable to show how the positive and negative contributions of  $DIV_{div}$  change with time in Figs 7/9?

We were initially unsure how to interpret the reviewer's suggestion, as we could not identify a meaningful way in which separating positive and negative contributions of  $DIV_{div}$  in Figs. 7 and 9 would address an open physical question. However, reflecting on this comment led us to realize that our statement in L. 443–445 was misleading, as it did not sufficiently emphasize the consistency between the signals shown in Fig. 11 and Fig. 7. Our original intent was to point out that the spatial composites in Fig. 11 do not show a clear WCB signal for the weakest—though non-negligible— $DIV_{div}$  contributions identified in Fig. 7. We now clarify this in the revised text and explicitly discuss how spatial compositing may weaken the WCB signal, while also acknowledging that mechanisms other than WCB outflow can contribute to the diagnosed divergent amplification: *“This is likely due to several factors, including changes in anomaly size and shape that smear the signal, as well as contributions from upper-tropospheric processes not directly related to WCB outflow, such as jet-related ageostrophic (dynamically forced dry) secondary circulations and balanced Rossby-wave dynamics.”* (L. 471 ff). We thank the reviewer for prompting this clarification.

- Linked to Figure 11 in Lines 436 - 454, the discussion surrounds the importance of WCB and diabatic heating in retrogression of blocking, linking these statements to current and previous results. It was not clear to me from these results how this was the case. However, several results including Figure 7, 8 and 11 show that diabatic and WCB anticyclonic amplification are much more prominent in progressive regimes.

We thank the reviewer for this comment, which helped clarify our interpretation. We agree that Figures 7, 8, and 11 show that WCB activity and associated  $DIV_{div}$  tendencies are overall more pronounced in progressive regimes (=upstream pathway). This is now explicitly stated in the revised manuscript. Our intention was not to imply that WCBs are stronger during retrogression, but rather that weaker and more spatially variable WCB-related diabatic tendencies can still contribute locally to the westward extension of PV anomalies in some retrogression cases. In contrast, progressive regimes are characterized by stronger and more coherent WCB outflow that primarily supports ridge amplification.

We have revised the two paragraphs in Section 3.4 accordingly (L. 449-484), as well as two sentences in the summary and concluding discussion (L. 554 ff).

## Reviewer 3

This paper examines the onset dynamics for four blocked weather regimes over the North Atlantic–European region using ERA5 reanalysis data. They traced negative upper-tropospheric PV anomalies and diagnosed the contribution from dry and moist processes. They showed two distinct onset pathways to blocked regimes.

Overall, the manuscript is well-written with clear structure and logical flow. The authors did a good job on describing the background and research questions clearly. The results are nicely presented that directly addresses the posed questions, presenting a coherent narrative that is easy to follow. Their methodology is also solid and built upon their previous work. The tracking of negative upper-tropospheric potential vorticity anomalies (PVAs<sup>-</sup>) combined with a quasi-Lagrangian PV tendency decomposition framework to diagnose contributions from dry and moist processes represents the state-of-art approach. By applying this framework across four distinct blocked regime types (Atlantic Ridge – AR, European Blocking – EuBL, Scandinavian Blocking – ScBL, and Greenland Blocking – GL), the study offers a novel perspective that effectively complements and extends existing Eulerian analyses of blocking dynamics. This method provides valuable insights into the life-cycle evolution of PVAs<sup>-</sup> and the relative roles of dry versus moist processes in different blocking configurations.

This manuscript shows some significant findings. First, about the non-local Development: they demonstrated that most PVAs<sup>-</sup> do not form in situ but propagate via upstream or retrogression pathways. This challenges simpler, local formation paradigms. Second, they shown the dominant role of moist processes: The study robustly demonstrates that divergent PV tendencies (DIVdiv), linked to WCB activity, are the primary driver of PVA<sup>-</sup> amplification prior to blocked regime onset. This clarifies the ongoing debate on the importance of latent heating. Most interestingly, they shown that pathway over regime type: The dynamics (especially moist contributions) depend more on the PVA<sup>-</sup> pathway (upstream vs. retrogression) than on the eventual blocked regime type. This is a key insight for understanding variability. Lastly, they synthesizes Eulerian and quasi-Lagrangian perspectives into a coherent narrative: blocking onset involves remote moist amplification of PVAs<sup>-</sup> followed by their local dry reorganization into the large-scale blocked pattern. They are complementary stages of the same process.

The reviewer also has some major concerns regarding the below points, and would like to request the authors to consider addressing these concerns.

We thank the reviewer for this overall positive feedback on our study and comment below on their concerns.

- First, there is some ambiguity in the interpretation of terms in the PV tendency equation — need for clearer separation of "moist" versus "dry" contributions. In the quasi-Lagrangian PV tendency framework, the authors attribute the LOW term to baroclinic interaction, the UP term to upper-tropospheric (quasi-)barotropic dynamics, the DIVadv and DIVdiv terms to (indirect) moist contributions, and the NONCONS term to non-conservative processes (including direct diabatic effects and friction). However, divergent processes are also inherent to 3D dry dynamics. In this

framework, the divergent terms ( $DIV_{adv}$  and  $DIV_{div}$ ) include both diabatic and adiabatic contributions, which are mixed and not separable in the presented analysis. Consequently, the claim of weak baroclinic interaction (adiabatic contribution) appears unconvincing, as the two effects cannot be clearly distinguished. Furthermore, divergent flow associated with secondary vertical circulations plays a central role in baroclinic conversion and eddy amplification. Similarly, in the Rossby wave packet (RWP) and eddy kinetic energy (EKE) frameworks, the ageostrophic flux term (linked to downstream development) is known to be important for eddy growth (e.g., Chang and Orlanski, 1993), and this process persists and can dominate even in purely dry dynamics. The authors equate the UP term with downstream development, yet the UP wind is non-divergent, whereas downstream development is fundamentally driven by ageostrophic (divergent) processes. Attributing the divergent terms predominantly to diabatic (moist) effects is therefore not fully persuasive without a separation of the diabatic and adiabatic components of the divergent flow.

We thank the reviewer for this detailed and insightful comment. We agree that in the quasi-Lagrangian PV tendency framework, the divergent terms ( $DIV_{adv}$  and  $DIV_{div}$ ;  $DIV$ ) inherently include both diabatic and adiabatic contributions, and these cannot be fully separated in our current analysis. As a result, we acknowledge that attributing these terms predominantly to diabatic (moist) processes is an approximation. Similarly, we recognize that the UP term, which is non-divergent, is not strictly equivalent to downstream development in the Rossby wave packet sense, which relies on ageostrophic (divergent) fluxes. Our interpretation of weak baroclinic interaction is therefore based on the relative magnitude of the LOW term compared to other terms, but we clarify that this refers to the resolved contribution in our framework rather than a strict separation of adiabatic and diabatic processes.

Section 5.3 in Teubler and Riemer (2021) is dedicated to the question to what extent upper tropospheric divergence can be attributed to ‘moist’ and ‘dry’ dynamics. They find that  $DIV$  is more sensitive to proxies of latent heat release than to proxies of dry dynamic forcing, leading to the qualitative conclusion that  $DIV$  can be “predominantly” attributed to latent heat release. Similarly, Hauser et al. (2023) compare  $DIV$  and WCB occurrence during the episode of their case study and find that WCB trajectories are present for all sub-periods in which  $DIV$  is substantially positive. The current manuscript further substantiates the relationship between WCBs and  $DIV$  in Fig. 11. In a qualitative sense, it thus seems well justified to attribute  $DIV$  “predominantly” to latent heat release, although we acknowledge that ambiguities remain to the specific extent to which moist-processes dominate.

In the methods section (Section 2.2), we adjusted the introduction of  $DIV$ : *“Upper-level divergence can arise from both dry dynamical forcing and latent heat release below. In particular, strong divergence is frequently associated with diabatic outflow near the tropopause, such as from WCBs (e.g., Madonna et al, 2014; Pfahl et al., 2015; Hauser et al, 2023). A detailed sensitivity analysis by Teubler and Riemer (2021; their Section 5.3) demonstrates, however, that upper-tropospheric divergence is more sensitive to proxies of latent heat release than to proxies of dry dynamics, supporting the interpretation that these terms predominantly reflect indirect moist contributions, while acknowledging that dry dynamical forcing may still contribute and*

*cannot be quantitatively disentangled with the present diagnostics (dark red arrows in Figure 1b).” (L. 172 ff).*

A note on downstream development: The EKE and the PV framework of Rossby wave packets are – in general – not directly comparable (see Sect. 3f in Teubler and Riemer 2016). The PV perspective on downstream development (and more generally, Rossby wave propagation) does not rely on divergent flow (e.g., Wirth et al. 2018, Fig. 8). Furthermore, Rossby waves in the 2D barotropic model exhibit downstream development, i.e., group propagation, but the full flow is non-divergent. Admittedly, these differences between two key diagnostics for Rossby wave packets are somewhat mind-boggling. [The root cause of these differences is that EKE maxima and PV anomalies are not equivalent.] Of importance for the current study is that our diagnostic framework is self-consistent: We consider a PV metric (not a local eddy kinetic energy maximum) and thus our interpretation of the dynamics has to follow that of the PV perspective. Ambiguities in DIV from the PV perspective are thus associated only with the difficulty of separating “dry” and “moist” secondary circulations.

- Second, diabatic contributions are likely concentrated in the NONCONS term, whose small net value may reflect cancellation. In the authors' interpretation, the NONCONS term encompasses non-conservative processes, including direct diabatic effects (notably latent heating) as well as friction, radiative effects, and turbulent mixing. The authors report this term as small. However, this small net value is likely the result of substantial cancellation among opposing processes — in particular, strong positive PV tendencies from latent heat release and strong negative tendencies from frictional dissipation. To accurately assess the true diabatic (especially condensational) contribution to PV evolution, these individual non-conservative processes should be diagnosed and presented separately rather than combined into a single residual term.

[Please also refer to Reviewer 1's comment, which raises a similar point, and our corresponding reply.]

We thank the reviewer for this insightful comment and agree that, in principle, diagnosing individual diabatic processes would be valuable for interpreting PV evolution. The reviewer is also correct that the relatively small net value of the NONCONS term could, in general, result from cancellation between different nonconservative contributions. However, evidence from previous work suggests that such cancellation is unlikely to be the dominant explanation in the upper-tropospheric and lower-stratospheric context considered here. Teubler and Riemer (2021) performed a detailed decomposition of the PV budget for ridges within Rossby wave packets—an analysis closely related to the present study—using YOTC (= Year of Tropical Convection; e.g., Moncrieff et al., 2012) data. Their results show that the PV tendencies associated with the convection, cloud, and turbulent schemes are each individually about an order of magnitude smaller than the radiative and advective tendencies (see their Figs. 8 and 9). Importantly, these results do not indicate that the small non-radiative, nonconservative contribution arises from a cancellation between large positive and negative terms.

The vertical levels analyzed in our study further support this interpretation. Our PV budget is evaluated on isentropic surfaces representative of the upper troposphere and lowermost stratosphere, where moisture content is generally low. Consequently, relatively small contributions from latent heat release are physically plausible. Consistent with this, in a related case study using ERA5 reanalysis data (Hauser et al., 2023), we found radiative tendencies to dominate the nonconservative PV budget, with only a small residual contribution from the remaining nonconservative processes (see their Fig. 10).

Finally, we note a practical limitation: ERA5 does not provide separate diagnostics for individual non-radiative nonconservative processes (e.g., latent heating, friction, turbulent mixing). Only the radiative tendencies are available as distinct terms (see Table 13 in the ERA5 online documentation). As a result, a further decomposition of the NONCONS term is not feasible within the framework of the present study.

We have added a brief discussion reflecting these points when motivating the neglect of nonconservative tendencies in the revised manuscript (Section 2.2, when introducing the PV tendency terms): *“In principle, this small net contribution could also arise from cancellation between opposing non-conservative processes; however, available process-based budget analyses for upper-level PV anomalies, based on Year of Tropical Convection (YOTC) data (e.g., Moncrieff et al., 2012)}, do not indicate such cancellation to be dominant (Teubler and Riemer, 2021).”* and *“Because the available reanalysis data do not permit a further separation of individual non-conservative processes beyond the bulk NONCONS term, and given its small magnitude, NONCONS is not analysed explicitly in the present study, following previous studies (e.g., Teubler and Riemer, 2021; Teubler et al., 2023; Hauser et al., 2024).”* (L. 182 ff).

- Third, composite budget analysis does not establish causality. The composite PV budget analysis illustrates correlations between PV tendency terms and blocking onset/maintenance, but it does not demonstrate causal relationships. The processes that amplify negative PV anomalies ( $PV^-$ ) do not necessarily drive the transition to a blocking regime. Instead, the reorganization of the large-scale flow (even if contributing relatively small amplitude changes) may be more critical for blocking establishment and persistence. For example, a similar PV budget signature to that shown in Figures 6, 8, and 9 could potentially emerge for an arbitrary anticyclonic system embedded in zonal flow, without it necessarily developing into a blocking event. This raises questions about the specificity of the diagnosed mechanisms to blocking dynamics.

We thank the reviewer for this important conceptual remark. We would like to clarify that we do not try to predict whether a blocking event will occur from individual PV tendency terms (e.g. at specific lead times such as day  $-3$  or  $-5$ ). This was never the intention of the analysis, and we agree that such an interpretation should not arise from the manuscript.

In this study, our focus is on understanding (1) the processes associated with the amplification and evolution of onset PVAs, (2) the locations where these processes

amplify or weaken PVAs<sup>-</sup>, and (3) the propagation of PVAs<sup>-</sup>. Most importantly, we do not investigate the difference in the underlying dynamics of PVAs<sup>-</sup> that are linked to blocked regime onsets versus those that are not. The identified PV tendency terms increase our understanding on which processes contribute to the growth of PVAs<sup>-</sup>, but they cannot give an explanation on “sufficient drivers” of blocked regime transitions. The reorganization of the large-scale flow, including advection, upstream and downstream interactions, and the background state, may be equally or more critical for establishing and maintaining blocked regimes, even when the amplitude changes of individual PV anomalies are relatively small. However, we also want to emphasize that several studies have shown that removing latent heating results in weaker blocking or even the absence of block formation (e.g., Steinfeld and Pfahl, 2020; Wandel et al., 2024). These and further studies suggest that latent heat release precedes and helps drive block development, rather than being solely a consequence of it.

We went through the manuscript and adjusted parts to clarify the diagnostic character of our analysis and to avoid any potential interpretation in terms of predictive causality for blocking events (e.g., last two paragraphs of Section 4.2 (L.563-584); Section 4.4 on relevance, limitations and future work (L. 624 ff)).

- Further, regarding Figure 4: The two illustrated pathways overlap, with the violet colors largely obscured by the orange colors. To improve clarity, the front (overlying) color could be rendered slightly transparent, allowing both pathways to remain distinguishable.

In the revised version of the manuscript, we have added contour lines for the frequency shadings for clarity, making the two frequencies more distinguishable between the pathways.

- Lastly, regarding clarification on the V<sub>up</sub> and V<sub>low</sub>: Are the V<sub>up</sub> and V<sub>low</sub> fields two-dimensional (defined at a single level) or three-dimensional (spanning multiple vertical levels)?

V<sub>up</sub> and V<sub>low</sub> are three-dimensional wind fields. They are obtained from piecewise PV inversion and are defined on multiple pressure levels. The resulting winds are subsequently interpolated onto isentropic levels for the analyses. Thus, V<sub>up</sub> and V<sub>low</sub> retain vertical structure and are not confined to a single representative level.

To place this more prominently in the manuscript, we added the following sentence in the description of the quasi-Lagrangian PV framework in Section 2.2: “*The PV inversion yields three-dimensional wind fields defined on pressure levels, which are subsequently interpolated onto isentropic surfaces for the analysis.*” (L. 143 ff).

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