"Towards a holistic understanding of blocked regime dynamics through a combination of complementary diagnostic perspectives"[1]

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. In the authors' comment on November 16, 2022, we commented on the major points raised by both reviewers and gave information on how to address these.

We here reply to all questions/comments raised by the reviewers and document the adjustments to the manuscript. Please find below the answers and further explanations (highlighted in **green**) to the comments and questions (line numbers refer to the new manuscript).

Comments to Reviewer 1

Summary

This is a nice paper which develops and applies three different perspectives to diagnose the development of a blocking event over Europe. This is a useful approach which helps to see how different processes and their associated theories relate to each other. The first two perspectives are linked through a common diagnostic framework centred on the PV equation (4), while the third is more distinct. The three methods are combined in a 'joint consideration' as opposed to a quantitative framework, but this nevertheless provides a fascinating, balanced and in-depth picture of the case study.

Overall, the paper is clear and well presented and certainly a useful contribution to the literature on blocking. The main conclusion that moist processes play an important role but that this is missed by some methods is sound and supported by the evidence. It is especially nice to see a focus on including and reconciling different viewpoints from the literature. The paper is a little long but I think this is unavoidable given the number of methods used, and in fact a few extra details in a few places might still be needed. I am supportive of publication after consideration of the following.

Many thanks to the reviewer for this positive feedback. We are pleased to be able to contribute to a better understanding of blocking with this study and the methods it contains. We agree that the manuscript is fairly long in its current form, but we are introducing several new methods here that need to be explained in detail. In later publications based on these new methods, the methods section will be shorter.

[1] Please note that the title of the manuscript has changed during the review process (see first major comment of the 2nd reviewer).

Major points:

1. Perhaps the most basic mechanism of forming an anticyclonic PV anomaly is through poleward advection and the beta effect. This is part of the barotropic term (v' dot grad q_0) but this QB term is mostly discussed in terms of the downstream advection of existing PV anomalies. The role of this term in generating, not just re-arranging, PV anomalies could be diagnosed through calculation of the beta term, and should be at least discussed if not diagnosed. It was interesting to see in Fig. 13 that both the heated and non-heated back trajectories originate at lower latitudes than the final anticyclone, so that beta must play some role in generating the PV anomalies for both sets of parcels.

The reviewer's idea of the beta effect and the beta term is not entirely clear to us. The term noted by the reviewer, v' dot grad q_0, is actually the term that describes changes of PV anomalies when air masses move between regions of different values of background PV. This term is "active" in the situation observed by the reviewer in Fig. 13. (The term v_0 dot grad q' describes the downstream advection of existing PV anomalies). The term v_0 dot grad q_0 is negligibly small, because the wind and isolines of PV of the background state are approximately parallel. It should be noted that a PV anomaly is not(!) materially conserved. The anomaly moves into the direction in that the amplifying tendencies (in Eq. 4) are largest (similar to surface lows that move into the direction of the largest surface pressure fall). There is thus no "hidden" beta effect associated with the anomaly moving into a region of different background PV. The PV anomaly budget (Eq. 4) is complete. We do not believe that the reviewer would like us to extract beta = df/dy from that background PV distribution.

We have introduced the term v' dot grad q_0 in the context of Rossby wave dynamics. The reviewer's comment makes us realise that further clarification of the general role of this term would be helpful to readers. We have thus added, after deriving the tendency equation following a PV anomaly, this sentence: "Note that the major contribution to the amplitude change of the anomaly (Eq. 9) is due to terms of the form $\mathbf{v} \cdot \grad q_0$, i.e., due to terms that describe that air masses cross the gradient of background PV." (Line 274f)

We hope that this response and the further clarification added to the manuscript addresses the reviewer's comment.

2. The methods differ fundamentally in whether they consider the cyclonic anomaly to be part of the block as well as the anticyclonic anomaly. I think the cyclone should probably be included given i) it's part of the regime structure used to define blocking here, and ii) it contributes to obstructing the westerly flow and causing some of the impacts outlined in the introduction. I don't think the analysis of methods 2 and 3 should be extended here to include the cyclone, but this limitation needs to be clearly discussed. It could also be summarised in the conclusions what drives the cyclonic part in this analysis.

This is indeed a fundamental difference between the Eulerian perspective and the quasi-Lagrangian and Lagrangian perspectives. The motivation for the development of the quasi-Lagrangian and Lagrangian perspectives is based on earlier work by Schwierz et al. (2004), Pfahl et al. (2015) and Steinfeld and Pfahl (2019), which investigated blocking dynamics by exclusively looking at the anticyclonic PV anomaly. We are aware that based on these 2 out of 3 perspectives in the manuscript, we can hardly say something about the cyclonic counterpart of the regime pattern. We briefly address this point from a Eulerian perspective (since the framework allows us to consider cyclonic anomalies separately) but do not go into much detail as the analysis of the cyclonic regime part is not the main focus in this study. We will therefore not extend the quasi-Lagrangian and Lagrangian perspectives to cyclonic anomalies.

As suggested by the reviewer, we discussed this limitation in the introduction and concluding remarks:

- "Next, we **exclusively** trace the anticyclonic, upper-tropospheric PVAs associated with the life cycle of a blocked weather regime" (Line 84f)
- "The Eulerian perspective has the advantage that it directly quantifies the processes leading to the weather regime index. Although it misses the synoptic activity upstream of the regime pattern, it takes into account the evolution of the full pattern which is in contrast to the quasi-Lagrangian and Lagrangian perspectives that focus exclusively on the dominant anticyclonic regime anomaly. Hence, the multi-perspective analysis is important to understand the full regime evolution. If not considered together, some kind of consideration of the cyclonic part of the regime pattern should be included from a pure quasi-Lagrangian perspective as it helps to obstruct the westerly flow and is therefore related to the impact of blocked regimes." (Line 629ff)

3. The rationale for tracking of PV anomalies could be more convincingly justified. PV is conserved in the absence of diabatic/frictional effects but not PV anomalies (as seen from the beta effect), so why track the anomalies? There is a threshold for PVA, so arguably this method still misses some information on the initial origin of anomalies as well. The method is fine for this paper, as the single event has been studied carefully. But for future use it would be nice to see more validation of this new method, including sensitivity to the choices and/or parameters used.

We thank the reviewer for this comment. The quasi-Lagrangian perspective (that represents the key perspective in this study) was developed with inspiration by the Schwierz et al. (2004) blocking detection algorithm, which is predominantly anomaly-based in its application. To be consistent and to be able to connect it as an add-on to the well-known algorithm by Schwierz et al. (2004), we decided to track PV anomalies (already mentioned in the introduction: *"This approach is strongly leaning on the blocking identification method of Schwierz et al. (2004) based on upper-tropospheric anticyclonic PV anomalies")*. Furthermore, the anticyclonic anomaly associated with the blocked regime is an anomalous PV feature and is easier to detect in an anomaly field than in an absolute field. Identifying PV features on an absolute PV field would not eliminate the choice of a threshold. We here make use of the quantitative PV framework developed by Teubler and Riemer (2016) to look at the processes associated with the amplitude evolution of 'blocks'. And this PV framework is developed for PV anomalies so that we also have to consider PV anomalies in order to apply this framework.

When identifying the PV anomalies, we avoid a criterion for the minimum size of a PV anomaly, such that we can detect the anomaly growing into a block much earlier compared to other studies. Furthermore, we are not primarily interested in where the PV anomaly originates, but rather the temporal evolution just before it is identified as a block. We currently work on a year-round climatological analysis of blocked weather regime dynamics based on the full ERA5 period (1979-2021) where we use a running threshold for the detection of negative PV anomalies (with a stronger/weaker threshold in winter/summer). Detailed information about the thresholds and justifications will be given in a follow-up manuscript.

We adjusted the text in the manuscript and added some more information in the introduction and concluding remarks:

- "The PV framework of Teubler and Riemer (2016) was **originally** used for ridges and troughs **(identified as negative and positive PVAs)** within a Rossby wave packet and is now **generally** applied to upper-tropospheric negative PVAs to quantify the processes associated with the PVA amplitude change (Fig. 1b)." (Line 89f)
- "Once a few adjustments have been made to the framework (such as developing a year-round threshold and sensitivity tests of the chosen parameters), it can be systematically applied to all blocked regime life cycles in ERA5." (Line 640f)

Minor points:

1. The authors do a good job of selectively covering the plethora of suggested blocking methods in the introduction. But I think a mention of the methods of Noboru Nakamura and Clare Huang would be a good addition, especially since the authors claim to have gone further than others in combining adiabatic and diabatic processes, something which the Nakamura/Huang theory also attempts.

We thank the reviewer for noticing this. In the revised manuscript, we added the following sentence in the introduction: "A notable study in this context by Nakamura and Huang (2018) discusses the onset of a block by the convergence and the subsequent constrained zonal propagation of wave activity as a jet stream 'traffic jam'." (Line 45f)

2. A few more details on the decomposition are needed - e.g. how exactly are v_{up} and v_{low} defined?

The details of decomposition are documented in detail in Teubler and Riemer (2021). To avoid redundancy and keep the manuscript in an acceptable length, we decided only to include the following details in the methods section:

"Here, the separation level of upper-tropospheric and lower-tropospheric PV anomalies lies between 600 and 650 hPa. Piecewise PV inversion with the non-divergent wind field under nonlinear balance (Charney, 1955; Davis and Emanuel, 1991; Davis, 1992) is performed on pressure levels between 25°N and 80°N and yields the wind fields v_{up} and v_{low} associated with the upper-tropospheric and lower-tropospheric PV anomalies, respectively. The piecewise PV inversion thus provides the possibility to consider the influence of the dynamics in the lower troposphere and the influence of the wave on itself, separately from each other. A detailed documentation with all selected thresholds for the PV inversion can be found in Teubler and Riemer (2021).“ (Lines 138-144)

3. More justification is needed that the (v_low dot grad q_0) term encapsulates the baroclinic effects. At face value, this seems a cruder definition than that of Martineau et al (https://doi.org/10.1029/2022GL097791)

Thank you for pointing us to the very recent and very interesting line of work by Martineau and co-authors, which we were not aware of. These authors apply an energy framework, whereas we apply a PV framework. These frameworks are not directly comparable because the energy of a PV anomaly does not only depend on its amplitude but also on its shape. Differences between an eddy kinetic energy framework and the PV framework are discussed in some detail in Wirth et al. (2018, near the end of their Sect. 3f; we note that Martineau et al. consider potential energy also). In the PV framework, baroclinic instability is due to the mutual amplification of Rossby waves that counter-propagate along two PV gradients with opposite signs. This reversal in the PV gradient is most prominently found between the low-level (lower boundary) theta gradient and the upper-level PV gradient associated with the (mid-latitude) jet stream. The PV perspective does thus consider upper-level amplification that is due to low-level PV (or boundary theta) anomalies as baroclinic growth – and vice versa. We would not consider the PV perspective to be cruder than an energy perspective.

From the reviewer’s comment it is not entirely clear to us at which point in the manuscript there would be a need for more justification. When we introduce the baroclinic PV tendency in line 154ff, it seems clear to us that we consider the PV perspective and the baroclinic growth of the upper-tropospheric anomalies. We thus leave the manuscript unmodified in response to this comment.

4. Are all projections etc performed over the region shown in Fig. 2a?

The projections are performed over the northern hemisphere in the latitudinal band between 25°N and 80°N as defined in Equation (5), since our PV tendencies are limited to this latitudinal domain. When we perform the projections, the focus is automatically drawn to the region shown in Fig. 2a where values different from zero exist and thus indicate the weighting of the PV tendencies/PV anomalies. We apologise that the region used to perform the projections wasn’t mentioned in the submitted manuscript. We added this crucial information in Section 2.4 after we introduce the equation for the projection (Eq. 6): “Note here, that the normalized projection is performed over the northern hemisphere in the latitudinal band between 25 °N and 80 °N since the PV tendencies are limited to this domain.” (Line 212f)

5. line 237 - 10% sounds like a low tolerance here. What is the sensitivity to this?

In this study and in particular in more systematic, climatological studies, the aim is to capture all anticyclonic PV anomalies associated with the respective blocked regime (here the European Blocking case in March 2016). We define the regime mask based on the -0.3 PVU contour in the year-round composite of upper-tropospheric PVAs during all times when the European Blocking regime is active. For this particular case study, the sensitivity for this threshold is small. The fact that at least 10% of the regime mask has to be covered by a PVA to be associated with the regime life cycle results from the thought that we only consider the dynamically-important PVAs. We decided not to make any adjustments in the manuscript but rather perform a sensitivity test for the follow-up systematic analysis when a variety of regime life cycles are considered.

6. Fig 5 was a bit too dense for me - could try fewer contours for the div term? And label that colorbar

We apologise for the complexity of Fig 5 and the lack of the colorbar labels. The modified version of the figure now includes less contour intervals for the divergent PV tendency term. Information about the steps of the PV tendency contours can be found in the figure caption.

7. It's interesting how the eddy fluxes come into these analyses, and not clear how closely the splitting/merging corresponds to the conventional picture of these. It might help to note for the eulerian analysis of this that the eddy fluxes are often diagnosed upstream of the existing block, to quantify their role in maintaining the blocking structure against the mean westerlies (Illari 1984). A crude quasi-lagrangian approach?

Thank you for this comment. It is indeed very interesting to note that the eddy fluxes from the Eulerian perspective may have this indirect effect (in the sense that it is not directly captured by the projected piecewise tendencies). In a composite study that we have performed subsequent to this case study, we do note that there is the Illari (1984)-type dipole that you refer to (in the average over many GL cases; and also for the other three blocked regimes in the North Atlantic-European region).

We now note this aspect in Section 4.1: "Note, however, that the eddy fluxes may still help to maintain the regime pattern by reducing the strength of the westerly flow upstream (Illari 1984). A dipole pattern associated with the eddy fluxes that indicates such a reduction is found in the average over many cases of Greenland blocking (Teubler et al. 2022)." (Line 388f)

8. I struggled to understand the role of the radiation, especially as it seems to strengthen the trough in fig 6b, contrary to the idea of radiative damping. It seems to be a relatively uniform cyclonic influence across the domain. Is it due to bottom-amplified LW cooling acting to reduce stratification everywhere, or something else?

Longwave radiation is strongly influenced by the climatological distribution of moisture along the tropopause leading to stronger longwave cooling in the troposphere than in the stratosphere. This positive vertical heating gradient is associated with the generation of positive PV tendencies across the tropopause. Hence, positive PV tendencies contribute to a strengthening of the trough (positive PV anomaly) and weakening of the ridge (negative PV anomaly). Both, a horizontal and vertical cross section of PV tendencies due to long wave radiation within RWPs and a more detailed discussion can be found in Fig. 10 of Teubler and Riemer (2021).

9. Could unify the names for the four periods between fig 11 and the text, for clarity.

We decided to label the four periods shown in Figure 11 with (a) - (d). The discussion of Fig 11 is adjusted accordingly. This makes it easier for the reader to see which period is being referred to.

10. Is the direct diabatic effect (section 4.2) only seen because the lower troposphere is excluded? (heating should give a negative PV anomaly above and a positive anomaly below)

Yes, we consider the dynamics in terms of the evolution of upper-level PV anomalies. At this level the nonconservative PV tendencies due to latent heat release are an order of magnitude smaller than advective tendencies. The reviewer is correct that positive nonconservative tendencies would be found at lower levels. We did not, however, analyse those levels in this study.

11. p24: The case that the divergent PV tendencies are a moist impact seems a bit overstated. It seems to imply that all divergent tendencies reflect this, but the correlations are only consistent with 20-40% of the variance being shared.

We thank the reviewer for this comment and agree that based on the correlation coefficient between the divergent PV tendencies, warm conveyor belt outflow activity and the share of heated trajectories, the statement seems to be a bit too strong. Still, we want to make the point here that there is a connection between the three variables. In particular we want to highlight that warm conveyor belt activity as a diabatic PV modification process can most likely be seen in the divergent PV tendency term. But we agree with the reviewer that a systematic climatological analysis would be needed to confirm this.

12. Top of p28: could you make a link between the methods here, between the heated trajectories (lagrangian) and the divergence term (quasi-lag)? Seems consistent with the theory of Methven (https://doi.org/10.1002/qj.2393) that the role of the heating is not direct but to enhance ascent of low-level, low-PV air up the warm conveyor belt into the block.

Thanks. In the revised manuscript, we included the following sentence in Section 5: “A high fraction of the backward trajectories from the PVA$^{-}_{qL}$ has a diabatic history from a Lagrangian perspective and divergent PV tendencies contribute to an amplification of the PVA$^{-}_{qL}$ from a quasi-Lagrangian perspective. Heating appears as an indirect effect that leads to an increase in transport of low-level, low-PV air up the warm conveyor belt into the PVA$^{-}_{qL}$, consistent with Methven (2015)." (Line 574f)

Comments to Reviewer 2

Summary

This work is a case study that aims to get a holistic view of different dynamics of blocking. This is a very important and meaningful direction to work on. Three perspectives (Eulerian, quasi-Lagrangian and Lagrangian) are used. The case study highlights the importance of moist processes (warm conveyor belt), which drive divergent outflow aloft and PV tendency.

The paper is well written overall. It will likely be well suited for publication after addressing the following comments (and more importantly, comments from the other reviewer). (Reviewer 1 raised excellent points and I try not to repeat what they said.)

We thank the reviewer for the positive feedback on our manuscript and reply to the comments below.

Major comments

1. Title: I think the main selling point of this study is that it gives a holistic view of different dynamics of blocking. The three perspectives (Eulerian, quasi-Lagrangian and Lagrangian) are not as attractive. Consider revising the title. I am also unsure about the word "unifying" in the title. The three perspectives are presented (metaphorically) like three separate dishes, not as one. "Unify" might not be the appropriate word.

> We agree with the reviewer that the title of the manuscript (currently: "*Towards a diagnostic framework unifying different perspectives on blocking dynamics: insight into a major blocking in the North Atlantic-European region*") needs revision. We decided to change the title to "Towards a holistic understanding of blocked regime dynamics through a combination of complementary diagnostic perspectives". Although the title does not include specifically the information that this study is exclusively based on a case study, we think this will become clear quickly when reading the abstract.

2. The Eulerian perspective seems to be inferior, because it "misses the processes associated with the development of PVAs advected into the region" (line 379). Does it have any advantage over the other perspectives?

> An intriguing motivation for the projections and hence the Eulerian perspective is that it directly quantifies the processes leading to the evolution of the weather regime index (up to constants). Additionally, the quasi-Lagrangian perspective cannot take into account the role of adjacent positive PV anomalies, which are part of the full weather regime patterns and should not be neglected. On the other

hand, only the quasi-Lagrangian perspective can quantify the processes upstream to the weather regime. Hence, the multi-perspective analysis is important to understand the full regime evolution.

In the concluding remarks, we tried to make this clear now by adding “The Eulerian perspective has the advantage that it directly quantifies the processes leading to the weather regime index. Although it misses the synoptic activity upstream of the regime pattern, it takes into account the evolution of the full pattern which is in contrast to the quasi-Lagrangian and Lagrangian perspectives that focus exclusively on the dominant anticyclonic regime anomaly.” (Line 629f)

3. I think this work lays out very good foundation where different proposed dynamics of blocking can be compared together. Right now, the direct latent heat release, indirect moist effect through divergent outflow and selective absorption (Yamazaki and Itoh, 2009) are considered. Many other proposed mechanisms, like the well-known eddy-straining idea (Shutts 1983), does not seem to be sufficiently discussed in results. Would be good to explicitly discuss them in results.

We appreciate this comment. However, the amount of studies that investigate blocking dynamics is immense and it is hard to bring up all the different aspects without the manuscript becoming enormously long. Also, the idea of eddy-straining introduced by Shutts (1983) evolved into newer concepts such as the selective absorption mechanism introduced by Yamazaki and Itoh 2009, based on Shutts original ideas. Nonetheless, we have still included the following sentences in the concluding remarks, which briefly discuss Shutts' (1983) eddy-straining idea as a potential next step in a systematic analysis of blocked weather regimes: “The quasi-Lagrangian perspective developed here could be the key tool to test and verify many theories of blocking dynamics (e.g. also the eddy-training idea of Shutts (1983)) for a variety of events.” (Line 644f)

4. This work considers blocking from the perspective of weather regimes. I could be biased against weather regimes, but I feel like the perspective of weather regimes here brings few benefits but more burden. For example, amplification to the secondary ridge over the US East Coast might be confused with the block (line 360).

Maybe it is too much to ask you to give up on weather regime and redo the analysis, or to give up the phrase “blocking dynamics” and instead say “regime dynamics”. But I still think it takes up too much words and figures, and some of them can be moved to appendix/supplement, as it is not the key or a selling point.

Thank you for this comment. First of all, looking at blocking from a weather regime perspective brings in a new angle to the classical considerations, which are mostly limited to the anticyclonic anomaly. Especially with the Eulerian framework we can gain insights into the dynamics of the cyclonic part during a blocked weather regime life cycle and investigate the full pattern. Weather regimes have implications for extended-range weather forecasting and a better understanding of their dynamics can help to improve the forecasts on subseasonal-to-seasonal (S2S) time scales. In particular the unique year-round weather regime definition used in this study brings in many advantages, especially the objective definition of the life cycles and its life cycle stages (onset, maximum and decay). Thus, a wide variety of blocking theories can be considered and tested for different life cycle stages in subsequent studies in which the frameworks are applied climatologically.

We made modifications from ‘blocking dynamics’ to ‘regime dynamics’ in the entire manuscript, in particular in the abstract, introduction and concluding remarks.

Regarding the comment for the misleading signals over the U.S. East coast: We are aware of the baroclinic PV tendency over the U.S. East coast associated with a negative PV anomaly that dominates the overall baroclinic PV tendency signal in the projections. However, this negative PV anomaly in the EuBL pattern is shown in Fig. 2a and is part of the full regime pattern. An elimination would lead to the fact that the weather regime index evolution can no longer be considered.

5. The quasi-Lagrangian analysis might be able to explain why the block becomes strong and large, but not why it is stationary. Stationarity is also a key aspect to create extreme weather events. Dynamics to make a block stationary should at least be included as one of the future directions.

This is a really good point. We have made additions in the concluding remarks. In a climatological study, we are currently dealing with this topic (what causes the block to remain stationary?) and refer to this aspect and ongoing work in the concluding remarks: “A next step will be a climatological investigation of blocked regime life cycles in ERA5 which addresses the dynamics of the three different life cycle stages (onset, maximum stage, decay) from the quasi-Lagrangian perspective.” (Line 638f) and “It is of interest to study which processes are important in the buildup of the negative PVA of the blocked regime and by which processes the block is maintained and kept stationary. Furthermore we plan to address the dynamics of the decay process of blocked regime life cycle patterns.” (Line 642f)

6. Line 242: The amplitude metric is spatial integral of q’ over the area A. Since the threshold of q’ is not zero but -0.8 PVU (line 229), would it be better to choose the amplitude metric instead to be the spatial integral of (q’+0.8) over the area A? In this way, whether marginal points cross the threshold or not would not make a big difference.

We thank the reviewer for this comment. We definitely see the risk that with the suggested adjustment (threshold for the area to be integrated = 0.0 PVU) the area of the PV anomaly will drastically increase, anomalies that one would consider to be separate based on synoptic intuition would be combined together into single large and undiscriminating anomalies, and important signals cancel out as a consequence. Because we have included this constraint in a boundary term and the budget is in our opinion sufficiently closed for the purpose of this study, we see no need to change the threshold here.

Minor comments

7. Section 4.1: Related to reviewer 1 major point 2, when separately considering the cyclonic and anticyclonic anomalies, please say that the main cyclonic part of the regime pattern does not contribute to obstructing the westerly flow (Figure 2).

> We partly agree with the reviewer here. The cyclonic part of the regime does not contribute directly to obstructing the westerly flow. However, for a high-over-low configuration or an omega block, the positive PV anomalies are important in the formation and maintenance of a stationary pattern. We would not like to dismiss any role of the positive PV anomalies in obstructing the westerlies and strongly believe that the separate consideration of the positive PV anomalies is an important part from the Eulerian point of view.
>
> We therefore decided not to add the suggested phrase.

8. Line 163: "eddy flux convergence may change PVAs locally but may neither generate new nor amplify existing PVAs in a globally averaged sense. Furthermore, eddy flux convergence may not change the area-integrated amplitude of PVAs that are defined by a boundary at which q'=0" I'm not sure about if these statements are true.

> These statements are true in the sense that they are mathematically correct. The (2D) spatial integral of the divergence of a quantity can be written as a line integral of that quantity (projected onto the line segment) along the boundary that defines the area of integration (divergence theorem or Gauss's theorem). Here, that quantity is **v**'q'. For global integration the flux at that "boundary" vanishes (**v**' is the anomaly of the horizontal wind). For an anomaly defined by q'=0 at the boundary, **v**'q' vanishes at that boundary also. To help clarify, we have added "(because the flux vanishes at the boundary of the global domain)" in line 173f. For the second statement we already refer to Teubler and Riemer (2016), where the case of an anomaly bounded by q'=0 is explicitly discussed.

9. Line 642: "we are able to close our q' budget…". But this requires taking Delta-A from observed area change? If so, is this "cheating"? Around line 260, you should briefly say that Bnd is taking from observed area change (not just in appendix).

> Thank you for your careful reading of the appendix. We compare the diagnosed tendencies with the observed evolution. In doing so, the reviewer is correct that we take the observed area evolution into account for this comparison also, i.e., we do not diagnose the *Bnd* from a single instant in time. This way to diagnose a term is in contrast to that of the tendencies discussed in the evolution of the main anomaly in Fig. 10. It did not cross our minds that this procedure may be interpreted as "cheating". From the perspective of *diagnosing* (not predicting) different contributions to the evolution, we do not see why this procedure would not be fair. We agree that this difference in diagnosing a tendency term should not be "hidden" in the appendix and thus follow your helpful suggestion to be more explicit in the main text about how we estimate *Bnd*.

To make our computation clearer in the main text, we substitute in line 278 “Notwithstanding these limitations, …“ with “Evaluating Bnd by using observed changes ΔA of the area A of the PVA, …”.

10. Fig. 11: How is the effect of splitting/merging events on PVA quantified? Does this require knowing the observed area change?

For the quantification of the splitting/merging effect, we look at the amplitude of the merging PVAs prior to the merging events and the amplitude of the splitting PVAs after the splitting event. Therefore we use the isentropic PV anomaly field integrated over the PVA object based on the definition via vertically-averaged PV. The accumulated amplitudes of the splitting and merging PVAs gives us then a simple proxy of their net effect. Therefore we do not require the observed area change here.

We included the following sentence in the caption of Figure 11: “Isentropic PVA amplitudes of the splitting and merging PVAs are summed up separately for the quantification of the net effect of splitting and merging events.”

11. Line 10: “All three perspectives highlight the importance of moist processes…” Does the Eulerian perspective highlight the importance of moist processes?

The Eulerian perspective is limited in a sense that it “sees” only what happens in regions where the strongest (regime-pattern related) circulation anomalies are found. Therefore, much of the synoptic activity upstream of Europe prior to the European Blocking onset is missing. However, the Eulerian perspective captures the signals in the divergent PV tendencies during the active regime life cycle, when the PVAs are located on their expected positions. In summary, the Eulerian perspective misses information prior to the onset but captures the important moist processes during the regime life cycle.

We decided to change the ‘and’ to ‘or’ in the following sentence in the abstract: “The three perspectives highlight the importance of moist processes during the onset **OR** maintenance of the ‘blocked’ weather regime.”

12. Line 355: There no Fig. 6d. Please fix the typo.

Thanks, we have fixed the typo.

13. Line 554: “… the quasi-Lagrangian perspective reveals an amplitude strengthening of the main PVA over Europe by the merging of further PVAs…” This statement in section 5 (synopsis) don’t seem to be supported in section 4.2 (quasi-Lagrangian perspective), especially Figure 11 finds merging and splitting to have *weakening* effect.

We thank the reviewer for making us aware of this. Yes, the splitting and merging events in the maintenance stage cause a weakening effect of the net amplitude change (Fig. 11). We also see that the number of splitting events exceeds the number of merging events during the maintenance stage (see red-colored markers in Fig. 8). However, three of the identified splitting events are associated with a remarkable change in amplitude and area (>30%!) and might dominate the net effect shown in Figure 11. Still, we believe that the merging events occurring during the maintenance stage contribute to the intensification of the PVA amplitude, especially as the majority of all identified merging events take place in the maintenance stage.

We adjusted the respective part in Section 5: “The attraction and absorption of a synoptic-scale anticyclonic eddy by the block as a maintenance mechanism is a well-known concept (Yamazaki and Itoh, 2009) and is investigated herein the quasi-Lagrangian perspective. The tracking algorithm that considers splitting and merging events detects several merging PVAs$^{-}_{qL}$ from the southwest that amplify the existent main PVA$^{-}_{qL}$ over Europe. More than 50\,\% of the merging events identified around the EuBL occur within the regime life cycle and provide evidence that these merging events are important for maintaining or re-intensifying the PVA$^{-}_{qL}$ strength. However, few splitting events in the maintenance stage are associated with noteworthy effects on the amplitude and area of the PVA$^{-}_{qL}$ and dominate the net effect on the PVA$^{-}_{qL}$ amplitude for the active life cycle stage. Nevertheless, with the novel tracking algorithm, it is for the first time possible to quantify the well-known concept of Yamazaki and Itoh (2009)” (Lines 581-589)

References All references mentioned in this document are listed as references in the submitted manuscript.