

Final author comments for egusphere-2026-4184

Potential vorticity modification by turbulence in the upper troposphere and lower stratosphere – Part 1: Mechanistic understanding in an upper-level jet-front system

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We are grateful to both reviewers for their detailed reviews from different perspectives and their affirmation that the topic addressed by the manuscript is important. We thank Reviewer 1 for pointing out the difficulties that general readers may encounter when reading the manuscript. We appreciate deeply the in-depth discussion and insights provided by Leo Saffin, which will enrich the discussion section of the manuscript. We also realise a contrasting view on the presentation of the idealised framework. We will provide more guidance for readers less familiar with the topic in the revised manuscript, without compromising the level of details appreciated by experienced readers. Based on the suggestions and comments from the reviewers, we will implement the following major changes in the manuscript:

- Emphasise and highlight the first major contribution of the manuscript better, i.e. The formation of PV tendency tripole by turbulence can be understood and explained with the idealised framework mathematically, which make this result generalisable.
- Provide more explanation for general readers, particularly in the sections discussing the idealised framework and the illustrative example in the dedicated IFS simulation (Sect. 3, 4, and 5), to reduce the possibility of confusion.
- Incorporate the insights from the review of Leo Saffin into the discussion section (Sect. 7) and other relevant parts.
- Improve readability of figures, correct grammatical errors, and rephrase unclear sentences.

This document presents the reviewers' comments in **blue** and our reply in **black**.

Reviewer 1

General:

This manuscript deals with the generation of bands of potential vorticity (PV) in the upper troposphere and lower stratosphere (UTLS) resulting from non-conservative processes like diabatic heating or turbulence. There are two parts: part 1 uses an idealized framework and argues why turbulence due to strong vertical shear of the ambient wind can lead to tripolar

structures in the generated PV field. Part 2 goes on and considers an upper-tropospheric jet-front system in a NWP model simulation with the goal to show that these processes do actually occur in the real world. In this part, the computation of trajectories serves as a bridge between the instantaneous PV tendencies from the idealized framework and the observed patterns in the realistic case, where PV anomalies arise from an accumulation of PV anomalies along trajectories.

The science question behind this manuscript is sound and relevant. I also trust that the computations have been done carefully and are valid. Therefore, I think that the work merits publication. Also, I think that the focus on an upper-level jet-front system in this manuscript is justified, as this is a prototypical flow situation in which one may expect these tripolar patterns. Not surprisingly, the authors hint (in their discussion section) at the fact that the PV patterns turn more complex in more generic flow situations. But I think that's fine, since one should be allowed to pick prototypical situations in an attempt to advance theoretical understanding.

Thank you for your recognition of the scientific relevance of our study. We also appreciate your positive feedback on choosing the upper-level jet-front system as the starting point to understand the PV modification by turbulence.

Yet, I think that the paper does not live up to its potential. This is mostly related to the quality and the style of the text. The text would be well suited for a person who is part of the PhD committee of the first author and has followed the progress of the work over several years. By contrast, the general reader would appreciate (and sometimes need) a presentation on a higher level of abstraction, which assumes a less detailed a-priori familiarity with the topic. Given the text as it is right now, it is rather difficult to find out what is new and why this paper is so important. In particular, the conceptual sections could and should be made more compelling and more lucid. In my eyes such a revision would imply a fairly thorough rewrite of the entire text. Below I provide examples of such issues to clarify what I mean. However, this is not an exhaustive list, which means that addressing each individual issue will not be sufficient to address my criticism.

Thanks also for reading the text critically, especially on the presentation of the idealised framework and its connection to the dedicated IFS simulation results. We will address this concern by clearly emphasising the significance of the idealised framework, and highlighting its generalisability (Sect. 4 and 5).

Major issues

The PV changes due to turbulent fluxes of heat and momentum in Fig. 4d and 4e look rather blobby, and it is only after both are combined that a tripole-like structure becomes

apparent (Fig 4f). Is this a miracle, or is there an easy way to understand this? Does it mean that there is a hidden (so far undetected) constraint? Would a different perspective be more illuminating by revealing such a hidden constraint? In my eyes this issue is relevant to the paper, because if this is just a lucky coincidence of this particular case, it would not serve the purpose of corroborating the arguments from the idealized-conceptual part of the paper.

Thanks for these important questions and we would like to highlight here the usefulness of the idealised framework in understanding these observations and in generalising the results.

The emergence of the tripole-like structure in Fig. 4f (which is the sum of both the turbulent heat and momentum transfer terms) is constrained by the divergent non-advective PV fluxes from the turbulent layer, as presented in Sect. 4.3, and is now illustrated schematically in Fig. R1b. In Sect. 4.3 (and more detailed in Appendix B1), we showed that under most situations, the total non-advective PV fluxes, $\mathbf{H}+\mathbf{M}$, are divergent from the turbulent layer. Since a divergence of $\mathbf{H}+\mathbf{M}$ implies a negative PV tendency, PV will decrease in the turbulent layer. As we move further away from the turbulent layer, $\mathbf{H}+\mathbf{M}$ naturally reduces in magnitude as it gets less turbulent, resulting in a convergence of $\mathbf{H}+\mathbf{M}$ and hence positive PV tendencies above and below the turbulent layer. As a result, a tripole pattern emerges. Therefore, the “hidden constraint” is the divergent non-advective PV flux property depicted in the idealised framework, and it is a property of PV modification due to turbulent mixing.

Therefore, the tripole is not a lucky coincidence, but a manifestation of the divergent non-advective PV flux property deduced from the idealised framework. We would like to clarify that Fig. 4 and the related texts in Sect. 5 are to demonstrate the applicability of the idealised framework in realistic atmospheric flows, but they do not provide the generalisability of the framework. Instead, the mathematical equations and derivations upon which the idealised framework is based are providing the generality of the deduced properties.

We would also like to point out that the “blobby” patterns above the jet core in Fig. 4d and Fig. 4e for the PV tendencies due to turbulent heat and momentum transfers separately are likely a coincidence (due to the slopes of the isentropes and isotachs), as the tripole pattern below the jet core in Fig. 4f (in the tropopause fold) is also noticeable in Fig. 4e and does not show up like a “miracle”. However, this is a good example to demonstrate that, attributing the tripole pattern solely to turbulent heat or momentum transfer is not correct in general. Rather, their sum guarantees a tripole, with some cases having the heat or momentum transfer term dominating, and some others require a mixture of both.

Along similar lines, I find the near-cancellation between the two non-advective fluxes rather dangerous, because it undermines the generalizability of the results from the case study: you may have just been lucky with your choice of the case. Do you provide only a “more nuanced understanding” of a single case, or are the results of more lasting value?

As stated above, the generalisability stems from the idealised framework, which is applicable when the underlying assumptions are satisfied. The case study is an illustration that the idealised framework can be applied to realistic flow situations. Therefore, the idealised framework and the case study are complementary, with the former providing the generality of the results, and the latter validating the results in a realistic case. We hope this clarifies that the results are not limited to a single case, but are addressing the general setting.

It would help me (and possibly other readers) if you interpret or frame your findings on a more abstract level, like for instance: a monopole in turbulence (a single turbulent layer) creates a dipole in the tendencies of u and θ (your Fig 2), and this in turn creates a tripole in the PV tendency (your Fig 4). Your important contribution is then the fact that there may be near cancellations between the non-advective fluxes of momentum and heat, but nevertheless one obtains a tripole in the end. As I mentioned above, the latter appears somewhat as a miracle.

Thanks for the suggestion and we have considered such approaches. However, we prefer not to include such statements as it likely oversimplifies the situation and contradicts an important message of this manuscript.

As a first clarification, the PV tendency tripole induced by turbulence is given by the divergent non-advective PV flux from the turbulent layer (as stated above and discussed in Sect. 4.3). This is based on the analytical framework, which applies to general cases. We provided a mathematical derivation to support the emergence of this tripole pattern and this is an important contribution of this manuscript.

Simply linking the PV tendency tripole to the potential temperature and momentum tendency dipoles may hinder the awareness of the underlying complexity (e.g. Fig. R1a). As discussed in lines 473 to 475, Spreitzer et al. (2019) also observed the same PV tendency tripole, and attributed it mainly to the turbulent heat flux (and hence the dipole of potential temperature tendency), following the argument presented by Shapiro (1976). While this conclusion was supported by the data in their case study, the derivation in Sect. 4.3 and the cross sections shown in Fig. 4d-f clearly demonstrates that both turbulent heat and momentum fluxes are involved in creating the tripole pattern in the general case. A simplified conceptual framework like the one shown in Fig. R1a may easily be

misunderstood as the same as that argued by Spreitzer et al. (2019), which we would like to avoid.

A better interpretation, in our point of view, should therefore start from the divergent non-advective PV flux from the turbulent layer (Fig. R1b), which is found in most flow situations (Sect. 4.3 and Appendix B2). Given this divergent flux property (see green arrows in Fig. R1b), a tripole pattern is expected. We avoid going from “dipole” to “tripole” directly, as this oversimplifies the underlying complexity, which is demonstrated in the detailed derivation presented in Appendix B2.

To help visualise the above statements, we prepared the following schematic, and we will consider if Fig. R1b is suitable for the inclusion into Sect. 4 in the revised manuscript.

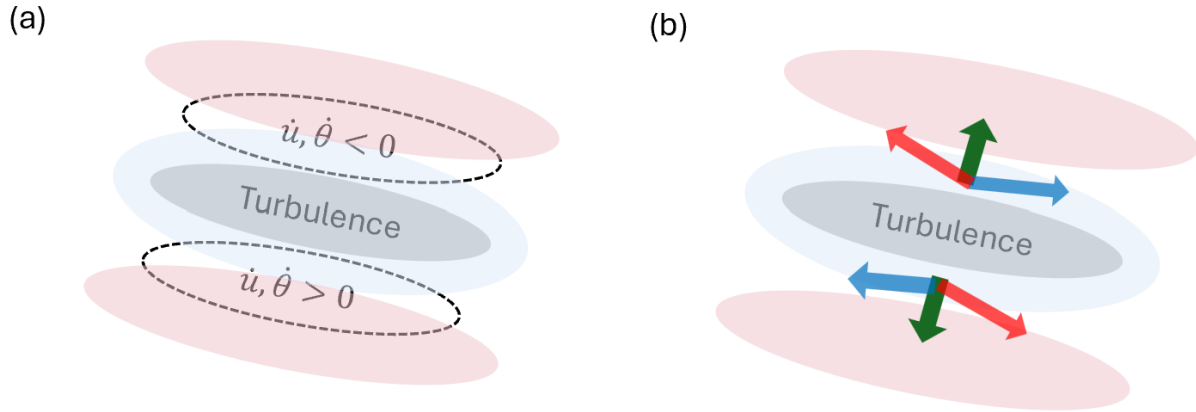


Fig. R1 Schematics illustrating conceptual frameworks for the formation of the PV tendency tripole due to turbulence (with grey ellipses being turbulent layers, red ellipses being positive and blue ellipses being negative PV tendencies due to turbulence). **(a)** The “monopole-dipole-tripole” framework suggested by Reviewer 1, with dashed ellipses indicating turbulence potential temperature and momentum tendencies. **(b)** The “divergent non-advective PV flux from turbulent layer” framework, with the red and blue arrows exemplifying the $\mathbf{H}$ and $\mathbf{M}$ vectors, respectively, and the green arrows indicating the resultant total non-advective PV fluxes due to turbulence.

In section 4 I am asked several times to compare what I see on the plots with what I expect. But for me it was sometimes not clear what my expectation should be.

We apologise that we do not fully understand this comment. We only refer to schematics (Fig. 2 and 3) in Sect. 4. We think this comment may be related to Sect. 5, and we defer the explanation to the specific comment on line 453.

There seems to be an unresolved issue regarding previous work of Saffin et al. (2017). This somehow limits the value of the present work, as it remains unclear how generic the tripole patterns really are.

Thanks for pointing out this apparent discrepancy with Saffin et al. (2017). As we discussed in Sect. 7.1, there are a few methodological differences between the work by Saffin et al. (2017) and the current study, which are possible reasons for the different results (dipole in Saffin et al., 2017; tripole in current study). Nevertheless, the emergence of the tripole pattern is supported by the idealised framework, providing a rigid foundation and generality for this result. Therefore, we consider the differences being mostly of methodological origin.

Furthermore, as commented by Leo Saffin (see below, point 5 of his review), he also thinks that the discrepancy is likely caused by the tropopause-relative vertical composite method in his study, which may mask the individual tripole signal. We are glad that the derivation presented in Sect. 4 has given him the confidence that a PV tendency tripole should arise even with the PV tracer method. Therefore, we do not think that this issue is diminishing the importance or value of the present work.

It is nice that you introduce the two-dimensional vectors H and M (eqn. 12). At the same time, you do not really stick to this formulation, like for instance in eqn. (14), which is formulated without any reference to H and M . I trust that all your calculations are OK, but it was somewhat confusing for me to shift back and forth between the different formulations.

Thank you for your comment. We would like to clarify that the “PV (substance) flux” formulation is used throughout the entire manuscript. For Eq. (14), we start with Eq. (11), where the flux formulation is still clear. The confusing part may come from the last line of Eq. (14), where we expand the flux form into individual terms.

The reason for this is to make the explanation of the importance of vertical wind shear in turbulent heating (Sect. 4.1) easier. To facilitate the connection with previous works which do not use the flux form, we conform to their formulation and allow a more straightforward comparison of the magnitudes of the horizontal and vertical terms. We therefore decided to make this small compromise to improve the linkage with previous studies. This is the only part in the manuscript where the flux form is less prominent, and we hope it is understandable given the explanation above.

Specific comments:

Line 34 “in local Cartesian coordinates”: Considering the Nabla symbol as an operator, there seems (at this stage) no need to refer to any specific coordinate system.

Thanks, we will remove it.

Line 45, “which induce a non-advective PV flux...”

Thanks, we will implement this change.

Line 45, “.... and induce far-field effects....”: strictly speaking this cannot be understood at this point. It requires the additional concept of PV inversion for the balanced flow component to make sense --- which you have not talked about up to this point.

Thanks, we will add a reference to Hoskins et al. (1985).

Line 50, “... influences the nonlinear evolution....”: is the evolution of linear waves unaffected?

Thanks, we mean the evolution of Rossby waves is generally nonlinear. We will remove “nonlinear” to avoid confusion.

Line 55/56, “radiation and turbulence.... cloud processes and convection”: these two pairs of mechanisms do not seem very distinct.

Thanks for your comment. In lines 55-56, we are reporting the conclusions from previous studies. Since the UTLS was considered generally dry, early studies focused on radiation and turbulence. Later studies revealed that the effects of convection and cloud processes are also significant for stratosphere-troposphere exchange.

Line 120, “convection, cloud processes and turbulence”: not clear what exactly you mean by these three terms, moreover they do not seem mutually exclusive.

We think you are referring to line 129. Thanks for the comment. The grouping of the physical processes is based on the parametrisation schemes implemented in the IFS. Individual parametrisation schemes are present in the IFS for convection and cloud microphysical processes, respectively. Radiation is also a separate module in the model. For turbulence, the grouping includes mainly the effect from turbulent diffusion, with the effects of orographic and non-orographic wave drag also counted towards this group. Since the study focuses on turbulence, we only introduce this group in detail in Sect. 2.1. We kindly advise interested readers to consult the documentation of the IFS model (ECMWF, 2021). Similar grouping of physical processes is also adopted in previous studies, including Spreitzer et al. (2019), Attinger et al. (2019, 2021), and Volonté et al. (2026).

Line 157, not clear what feature you refer to by “tongue”.

Thanks for pointing out. We refer to the narrow, equatorward extension of stratospheric air intersected by the dashed line. We will rephrase as “a narrow, equatorward extension of stratospheric air is present at 350 hPa (Fig. 1b, near the dashed line).”

Line 177, “.... based on reverse domain filling”: what is “reverse domain filling”?

Reverse domain filling was a method first used to obtain higher resolution tracer field using backward trajectories launched from a grid (e.g., Sutton et al., 1994). In essence, the method first defines a domain of interest and lays out a grid. Backward trajectories are initialised from every grid point in the domain, and values at earlier locations of the trajectories are assigned to the respective grid point. The values are then effectively advected forward in time to the corresponding grid point. To avoid confusion, we will move the references “(Sutton et al., 1994; Schoeberl and Newman, 1995; Liniger and Davies, 2003)” from line 179 to 177, right after “... based on reverse domain filling”.

Line 307, “below/above the heating maximum”: wouldn’t it be more relevant to frame this change in terms of the maximum of the turbulent flux (rather than the heating maximum)?

Thanks for the suggestion. However, the usage of “heating maximum” instead of “maximum turbulent heat flux” aims at comparing turbulence to other diabatic processes, including “latent heating in clouds and radiative heating” (line 300). Since turbulence inherently occurs in regions with enhanced vertical wind shear, the horizontal term (Eq. 14) cannot be neglected, causing the vertical dipole pattern not an accurate description.

Line 315: I do not really see how the horizontal term creates a horizontal dipole. Can you explain? And: how do I have to visualize a “horizontal dipole superimposed on a vertical one”?

Thanks for the questions. We are happy to provide the following schematic (Fig. R2) as an illustration. Consider a region with diabatic heating ($\dot{\theta} > 0$, indicated by the grey shading), with the heating maximum located at the centre. Therefore, there is a gradient of $\dot{\theta}$ in both y and z direction, with the signs of $\frac{\partial \dot{\theta}}{\partial y}$ and $\frac{\partial \dot{\theta}}{\partial z}$ indicated in the Fig. R1. According to Eq. (14), PV tendencies are contributed by both the horizontal and the vertical terms. Assuming $\frac{\partial m}{\partial z} > 0$, the horizontal term will generate a PV tendency dipole in the horizontal, with positive PV tendency in the negative y-direction, and negative PV tendency in the positive y-direction. The vertical term also generates a dipole in PV tendency, e.g. positive below and negative above the heating maximum if we assume $\frac{\partial m}{\partial y} < 0$. Therefore, if the horizontal term has a comparable magnitude to the vertical term, the horizontal dipole will be non-negligible and superimpose on the vertical dipole.

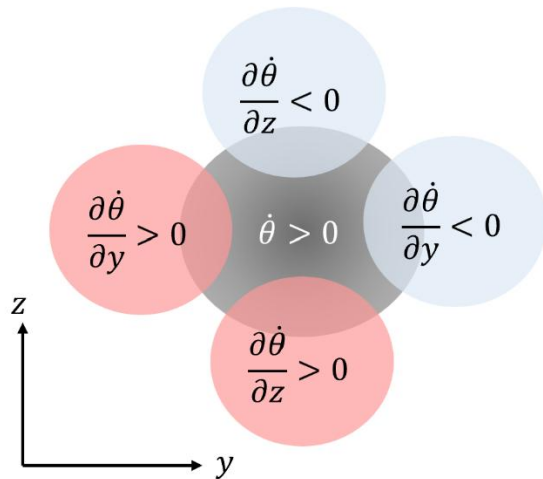


Fig. R2. A schematic illustrating the horizontal and vertical PV tendency dipoles associated with a diabatic heating maximum. Assuming a localised heating source $\dot{\theta}$ with the maximum at the centre (grey shading), the signs of the partial derivatives of $\dot{\theta}$ along y - and z -direction are indicated. Given that the horizontal and vertical terms are of comparable magnitudes, and assuming $\frac{\partial m}{\partial z} > 0$ and $\frac{\partial m}{\partial y} < 0$, the red circles indicate positive and the blue circles indicate negative PV tendencies.

Line 329: can you tell the reader why the angle between m and θ surfaces is “usually acute”?! This seems rather central to this section and may not be obvious to everybody.

Thanks for this question. The sentence at lines 328-329, “This follows ... the usual acute angle between isentropes and isopleths of m .” is a verbal summary of the derivation below (lines 330-345). In simple words, the acute angle is the result of a stable configuration of isentropes and isopleths of m (statically, inertially, and/or symmetrically stable) that is consistent with thermal wind balance. These are explained below in lines 330-345. This sentence is not necessary for the understanding of the argument in Sect. 4.2, and we will rephrase it or remove it to avoid confusion (since we also notice that the angle between two surfaces is not well-defined in the text).

Line 336, then $u \cdot \dot{\theta}$ is positive.... but only above and below the turbulent layer, not right within the turbulent layer.

Yes, as the tendencies within the turbulent layer are small in magnitudes. In line 336, we are referring to Fig. 2a, where we illustrate the dipole. We do not intend to include the turbulent layer.

Line 370, “the scalar product”: which one, you have recently pointed out two scalar products in (17a) and (17b), and it is not clear which one you refer to. Possibly both?

Thanks for the comment. We are referring to both of them and we will switch to “the scalar products”.

Line 382, what does “divergent from isentropes” mean? (and similar for m -isopleths). Similarly on line 388 “diverges from the isotachs”.

Thanks for the question. We described the meaning in lines 370-373 and we will further explain below with a schematic:

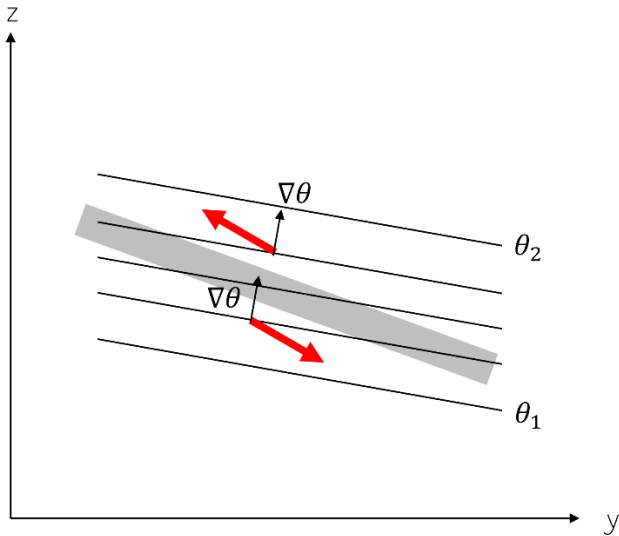


Fig. R3. Schematic illustrating the divergent fluxes (red arrows) from isentropes (black lines, with $\theta_2 > \theta_1$). For fluxes near the turbulent layer (grey shading), we call the fluxes “divergent from isentropes”, if they are pointing oppositely when projected to the direction of $\nabla\theta$ across the turbulent layer.

Consider a set of isentropes near a turbulent layer (grey shadings). Assuming $\theta_2 > \theta_1$, we draw the direction of $\nabla\theta$ at two locations, one above and the other below the turbulent layer. Let the red arrows represent the fluxes at these two locations, we can clearly see that the flux above the turbulent layer has a component pointing in the direction of $\nabla\theta$, while that below the turbulent layer has a component pointing to the direction of $-\nabla\theta$. Therefore, when the fluxes are projected on to the $\nabla\theta$ direction, the fluxes are in opposite directions. We describe it as “divergent” or “diverging” from the isentrope. The same applies to isopleths of m and isotachs. We hope this make the meaning clear.

The arrows in Fig. 4 are very small and hard to read (at least in the printed version).

Thanks for the feedback. We will enlarge panel 4b-f by showing a smaller domain around the jet core to increase readability.

Line 444, strictly speaking the plot only illustrates the “counteracting effect... on the fluxes H and M ”; PV modification is then given by the divergence of these fluxes, but for me it is hard to visualize the divergence in my head. Therefore, it is almost impossible for me to see from the plot what you ask the reader to see in the plot.

Thanks for pointing this out. The counteracting effect discussed in Sect. 4.2 is referring to the angle between H and M being mostly larger than 90° . Since H and M are the non-advective PV fluxes, they are counteracting each other’s effect in transferring the notional PV substances. This is what exactly the “counteracting effect” means, as suggested in the comment. It is true that the PV tendencies are related to the divergence of H and M , and this is more relevant for the next paragraph (starting from line 449). We think the terminology “counteracting effect ... on the PV modification” in lines 443-444 may have caused this confusion. We will consider a change to avoid this misunderstanding.

Line 453: does the reader know what the “typical vertical pattern” should look like? Similarly, “one may expect...”: are you sure the reader has the same picture in his/her head as you have? Actually, regarding the latter claim I am not sure whether I can follow you. It seems that (in my head!) I would have to evaluate the convergence of these fluxes, and this is not straightforward for me. Also, the computed DQ/Dt in panels 4d and 4e indicate a rather blobby picture, so I am not sure whether and to what extent this corresponds to my expectation (if I had any).

We appreciate these questions and we may have overlooked the challenges for general readers. The “typical vertical pattern” refers to the conceptual picture of a vertical PV tendency dipole across a diabatic heating maximum (similar to the vertical part in Fig. R2), which would be familiar to readers informed by results from previous studies (e.g. Wernli and Davies, 1997). Since turbulence generates a dipole of diabatic heating, it would be straightforward for these readers to assume a vertical tripole pattern in PV tendency, similar to Fig. R1a. Lines 452-457 point out that, as shown in Fig. 4d, the expected vertical tripole pattern is heavily distorted. Instead, the PV tendency pattern follows closely the convergence and divergence of the non-advective PV fluxes.

Therefore, we would like to rephrase line 453 from “deviates from the typical vertical pattern” to “deviates from a vertical pattern”, and from “one may expect” to “some may expect”. For general readers without any expectation, the main message is that the PV tendency field is following the flux convergence and divergence, including the regions with “blobby” PV tendency.

Paragraph starting on line 473: there seems to be conflicting information. On the one hand, Spreitzer obtained tripolar patterns while only considering turbulent fluxes of heat, while

here you show that the turbulent fluxes of momentum are actually the key for producing the tripolar structures. Can you resolve? Is this simply a case-to-case variability? If so, then statements such as “turbulent momentum fluxes dominate” only apply to the respective case and do not, per se, have a more general validity.

Thanks for the questions. Yes, we can resolve this apparent conflict. This is actually the main message for lines 474-478. In the work by Spreitzer et al. (2019), the tripole pattern analysed are mainly contributed by turbulent heat fluxes and thus linked to the argument by Shapiro (1976). In the current case, we demonstrate other tripoles which are the combined result of turbulent heat and momentum fluxes (e.g. the tripole above the jet core in Fig. 4f), or mostly turbulent momentum fluxes (e.g. the tripole below the jet core in Fig. 4f). Therefore, the tripole in general is the result of both turbulent heat and momentum fluxes, with the relative contribution varying from case to case. We would like to complement the explanation based on heat fluxes provided by Shapiro (1976) with our results.

If we use the $\mathbf{H}$ and $\mathbf{M}$ formulation, from Sect. 4.3, we knew that the total non-advective PV flux ($\mathbf{H}+\mathbf{M}$) is divergent from the turbulent layer in most cases, causing the tripole pattern. However, some cases can have a larger $\mathbf{H}$ (in magnitude) compared to $\mathbf{M}$, while some cases can have a larger $\mathbf{M}$ compared to $\mathbf{H}$. Therefore, there is no conflict, but only different manifestations of the tripole.

Line 487, “instantaneous PV tendency field”: Just to be sure, when you talk about PV tendencies you always talk about material rates of changes, i.e., material tendencies, right? If this is so, it could and should be stated more clearly or more often. Later in line 495 you refer to material tendencies, but it seems that at this point you think more in terms of “accumulated” tendencies. In any case, both material tendencies and local rates of change can be studied either on an instantaneous basis or on an accumulated basis (parcel-based and grid-point-based, respectively). The issue of local rate of change versus material rate of change needs to be clearly distinguished from the concepts of instantaneous versus accumulated.

Yes, we are only referring to material tendencies. At line 495, we used “material PV change” but not “material tendencies”. We totally agree that material tendencies and local rates of change should not be confused. We therefore avoided using any terminology like “local rate of change” or symbol like $\frac{\partial}{\partial t}(\dots)$. Instead, we keep consistently using $\frac{D}{Dt}(\dots)$, which are commonly understood as material derivative. We hope this clarifies any confusion.

Line 505, “showing a decrease....”: but there seem to be just as many region close to the cut-off that show an increase?!

Thanks for the question, and we agree there is another region with increase in PV to the west. The hook-shaped region with a decrease in PV is only an example of the many features that appears in both the ΔQ and APV field. To avoid confusion, we will change the sentence to “showing a dipole of PV increase and decrease in both fields.”

Line 517 (and later lines), “caused by ... “: a more careful wording would be “associated with....”

Thanks for reading critically. Concerning line 517, the APV bands are computed from the simulation output according to Eq. (7), with turbulence being one of the processes. Using the attribution method outlined in Sect. 2.3, turbulence is shown to be the dominant process that contributes to the total APV (Fig. 5b and d). The APV bands are thus said to be “mainly caused” by turbulence, and turbulence is likewise referred to as the dominant cause of the APV bands (in later lines). We hope this clarifies and justifies the usage of “cause” in line 517 and later lines.

533, what is the “downstream flank of a cut-off”? To a first approximation a cut-off is azimuthally symmetric, which makes it difficult to define a downstream or an upstream direction.

Thanks for your suggestion. We will rephrase it as the “eastern flank”.

Section 7.1: I got somewhat confused with this “summary”. There are tripolar patterns both in the vertical and in the horizontal (see, e.g., Figs 7 and 8 versus Fig. 6a), right? Could you more clearly distinguish between these two? Are both features related to turbulence?

We think you are referring to the last paragraph in Sect. 6.2. First, we would like to clarify that the horizontal tripolar patterns are only used to describe the APV bands (e.g. Fig. 6, 7a, 8a, and 9a). These APV bands are the integrated PV changes over time, instead of the instantaneous PV tendencies shown in Fig. 7b-e, 8b-d, and 9b-d.

For the horizontal tripolar pattern of APV in Fig. 6a, we have shown in Fig. 6b using the process attribution method (Sect. 2.3) that the major cause is turbulence. For the vertical tripolar pattern seen in, for instance, Fig. 7b-e, the field shown is the instantaneous PV tendency due to turbulence. Therefore, both features are related to turbulence. They are also connected as the horizontal tripolar APV bands are the results of trajectories experiencing coherent PV modification by travelling consistently at a particular location of the vertical PV tendency tripole. Consequently, the two are also related to each other, as revealed in the Lagrangian analysis in Sect. 6.

For the instantaneous PV tendency, the vertical tripole is usually tilted (e.g. Fig. 4f), which would result in a horizontal tripolar pattern if the PV tendency due to turbulence on an isobaric surface is extracted, like the elongated bands shown in Fig. 1c and Fig. 1d.

Line 628, tropopause-relative versus turbulence-layer-relative: shouldn't this essentially be the same, given that the turbulent layer is at a well-defined distance from the tropopause almost by design?!

Here we disagree with the comment. While turbulence is commonly found in the vicinity of the tropopause, we do not think there is a “well-defined” distance, meaning a constant distance, between the tropopause and the turbulent layer. Instead, we expect this distance to vary from case to case. Furthermore, since PV tendencies are dependent on the gradients of the turbulence-induced potential temperature and momentum tendencies, they also vary from case to case. Therefore, a tropopause-relative composite may mask the individual tripole patterns of PV tendency attached to their respective turbulent layers. As stated in the review by Leo Saffin (see below, point 5), the differences between tropopause- and turbulent layer-relative perspectives may cause the apparent discrepancy between Saffin et al. (2017) and the current study.

Fig. 10: I am not happy with the dashed line, which is meant to indicate the “coherent direction”. To be sure, this is a quasi-three-dimensional visualization. But still, I thought that the “coherent direction” in this configuration corresponds with the jet axis, and in my eyes this is not what the dashed line indicates.

Thank you for the comment. Yes, this is a three-dimensional visualization, with the black ellipses indicating the isosurfaces of wind speed associated with an upper-level jet, and the purple dashed line indicating the jet axis and the coherent direction. We will redesign the schematic to avoid confusion.

Lines 735-744 and Fig. 10: this section seems rather repetitive at this point. You may consider shifting the section to much earlier, after your idealized considerations and before you analyse the model data. This would facilitate the reader to see in the subsequent analysis what he/she is supposed to see.

Thank you for the suggestion. We will consider if any shortening can be done to reduce repetition. However, we prefer using this figure as the summarising figure, which shows also the Lagrangian perspective.

Line 747, “aims at corroborating...”

Thanks, we will implement this change.

Line 768, “flux is diverging from isentropes....”: what does this mean?

Please kindly refer to the response for the comment on line 382.

Fig B1 (and similar figures): the lines are so thin that it is hard to distinguish their colors.

Thanks, we will make them more readable.

Leo Saffin (Reviewer 2)

General Comments

This paper investigates the effects of turbulence on the PV distribution in an upper level jet. The authors show that an idealized two-dimensional case with diffusive fluxes of heat and momentum produces a tripole in PV tendencies. I.e. A flux of PV (substance) away from the turbulent layers above and below the jet (in most conditions). This idealized model is then used to explain the tripoles of PV seen in fully 3d simulations and that the tripole accumulates over time following trajectories.

This is an excellent paper. A clear derivation of the expected PV tendencies from turbulence in this situation with a thorough evaluation of different flow configurations (Appendix B2) backed up by a case study clearly demonstrating the expected results from the theory. I have a few minor corrections and some discussion of the results below.

Thanks a lot for your positive evaluation of the manuscript and we appreciate your thoughtful comments, especially those connecting the results to your previous study.

Specific Comments

1. The schematic in figure 2 makes the problem appear quasi one dimensional. There should also be structure in the y-direction. It would be better to depict the turbulent layer as turbulent regions that are also limited in the y-direction. Just to show that there is structure in the y-direction, not to change the main point of the schematic.

I think it would also help to combine (a) and (b) and show the jet core with the turbulent regions above and below, similar to figure 10.

Thanks a lot for the suggestion. We agree that showing structure or variations in the y-direction would help demonstrate that the framework is quasi-two-dimensional (instead of only in the vertical direction). We also appreciate the suggestion to combine (a) and (b) and show the jet since it facilitates the connection to the IFS simulation results presented later in the manuscript. We will incorporate these changes into the revised version.

Do you consider the horizontal tripoles in PV tendency (i.e. figure 4f) as simply a result of the slanted structure of the jet stacking the vertical tripoles in a diagonal orientation?

We think you are referring to the PV tendency tripole below the jet core in the tropopause fold region. Yes, we interpret the “horizontal tripole” as the result of the slanted or tilted turbulent layer. As depicted in Fig. 3, the turbulent layer (grey shading) is usually slanted or tilted, and it can be seen from the slanted isotaches and potential temperature or momentum tendency dipole (Fig. 4b,c). Since the divergent non-advective PV fluxes are relative to the turbulent layer, the resultant tripole is also slanted or tilted.

2. In section 2.3 discussing the limitations of the trajectory based method of attributing PV tendencies. You say that Q_{adv} contains errors due to the numerical diffusion of the dynamical core. The advantage of Q_{adv} is that it is not affected by numerical diffusion, it is exactly the PV at the start of the trajectory. Errors in Q_{adv} will arise from the limitation in the trajectories themselves because they are only approximations of the true air-parcel trajectories. The numerical diffusion of the model at each timestep will lead to tendencies in prognostic variables, that then lead to a tendency in PV because the model is not formulated to conserve PV. I would say this should be considered as part of APV. So equation 7 should include a residue because you are missing part of DQ/Dt when breaking it down into different components and not including this process.

So on line 212 you should say that the residue (in equation 8) accounts for

- Discretisation errors
- The derived trajectories are approximations of the true air parcel trajectories
- Nonconservation of PV by the dynamical core of the model

Thanks a lot for this detailed explanation and suggestion. We agree with your description, especially on how numerical diffusion leads to non-conservation of PV in the model. We believe the issue concerns the different ways of attributing the non-conservation of PV by the dynamical core.

We agree that Q_{adv} is not affected by numerical diffusion, but the calculation of $\Delta Q(x(t_0), t) = Q(x(t_0), t) - Q_{adv}(x(t_0), t_0)$ is affected, since $Q(x(t_0), t_0)$, i.e. the PV at the start of the backward trajectory, contains the non-conservation of PV by the dynamical core. It is because the tendencies in prognostic variables induced by numerical diffusion accumulate during the simulation and already affect the PV at later times. Therefore, in lines 206-208, we say “ ΔQ estimated from Q_{adv} ” contains “errors due to numerical diffusion of the dynamical core”. To better communicate that the error comes from $Q(x(t_0), t_0)$, we will change in line 206 from “ ΔQ estimated from Q_{adv} ” to “ ΔQ estimated from $Q(x(t_0), t_0) - Q_{adv}(x(t_0), t_0)$ ” and include a short explanation.

We agree that to fully recover the evolution of PV in the model (assuming the trajectory is perfect), there should be a PV tendency term accounting for numerical diffusion as you suggested. However, we cannot explicitly quantify this with offline trajectories. Therefore, we prefer keeping the current form of Eq. (7), as it shows how the total APV is computed from the simulation, which is the sum of all tendencies from parametrisations of physical sub-grid scale processes.

Nevertheless, we agree that the residue in Eq. (8), which can be interpreted as the difference between ΔQ and APV, contains the three contributions mentioned (i.e. discretisation errors, errors from trajectory calculations, and non-conservation of PV by the dynamical core), and they will be included in line 212 in the revised manuscript.

3. I found it difficult to read the green arrows in figure 4. Can you try different colours or less arrows but larger.

Thanks for the suggestion. We will enlarge panel 4b-f by showing a smaller domain around the jet core to increase readability.

4. The introduction at the start of section 6 is written as though you have no expectation about whether the air parcels will travel through different signs of PV tendencies or not. I think you should make the point that the 2d model would have trajectories staying in the same signed regions because you have taken a coherent direction so v is assumed to be small or zero, and you would also expect w to be small since you are not considering warm conveyor belts in this case (saved for part 2)

Thanks for the comment and suggestions. We would like to clarify the assumption of having a coherent direction. When introducing the quasi-two-dimensional framework, we assume that the variations along the coherent direction (x-direction) is small or negligible compared to those perpendicular to the coherent direction (y-direction), which means $\frac{\partial}{\partial x}(\dots) \ll \frac{\partial}{\partial y}(\dots)$. Therefore, v is not necessarily small or zero (which will be made clear in Part 2 with the case of a warm conveyor belt), as long as $\frac{\partial v}{\partial x}$ is small or negligible compared to variations in the y-direction. For instance, the existence of ageostrophic circulations across the jet would not violate the assumption, provided that the variations along the jet axis remain small compared to those across the jet axis.

Following this, I also found it surprising that you found trajectories that did have alternating signs of PV tendencies (figure 7). You say “these changes can be the result of temporal changes of the upper-level jet itself or the air parcels moving to a different part of the jet”. Could you check which by seeing if there is a significant wind across the axis of the jet?

Also, my suspicion is that it could be just the limitation of the trajectory calculation being hourly and crossing different model airmasses. Did you consider this?

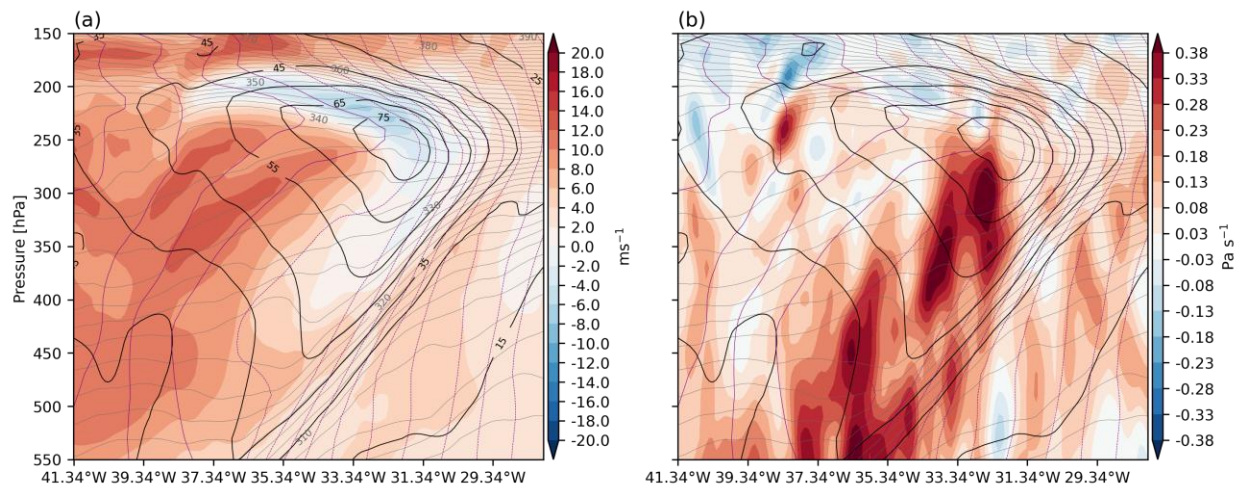


Fig. R4. As Fig. 4, but showing (a) v and (b) ω .

Thanks a lot for the thoughtful questions. To address the questions, we additionally extract v and ω along the vertical section shown in Fig. 4 (Fig. R4). In Fig. R4a, v has a smaller magnitude compared to u . However, v still attains a magnitude of 5 to 10 ms^{-1} in regions near the upper-level jet core. It thus demonstrates that there can be a small, but notable wind across the jet. In contrast, the vertical velocity ω (Fig. R4b) is indeed small (note the unit).

Furthermore, we take trajectory ‘7e’ shown in Fig. 7e as an example and examine vertical sections of the PV tendency due to turbulence along its trajectory at different times (Fig. R5). In Fig. R5, the panels correspond to time (a) $t=0$, (b) $t=-3$, (c) $t=-6$, and (d) $t=-9$ h in Fig. 7e, respectively, and the cross section is perpendicular to the horizontal wind vector at the trajectory location. From Fig. R5, we can observe that the jet and the PV tendency tripole induced by turbulence are evolving in time (e.g. the shape of the jet as indicated by the isotachs, the changing tripole(s) above the jet core, the presence of the tripole below the jet core in panels (b,c), but not (a,d)). These changes of the jet structure and the distribution of the turbulent layers would influence the PV tendency field, which then affects the accumulation of PV changes by an air parcel.

Also, the air parcel appears to move into different locations relative to the jet. The red circles with white fillings (Fig. R5) indicates the position of the air parcel at each time. At $t=-9$ (Fig. R5d), the air parcel is situated at the middle part of the jet above the jet core. As time progresses, it drifts towards the anticyclonic side of the jet, as illustrated in Fig. R5a and b. Together with the changes of the jet itself, they likely explain the alternating PV tendencies encountered by air parcel ‘7e’.

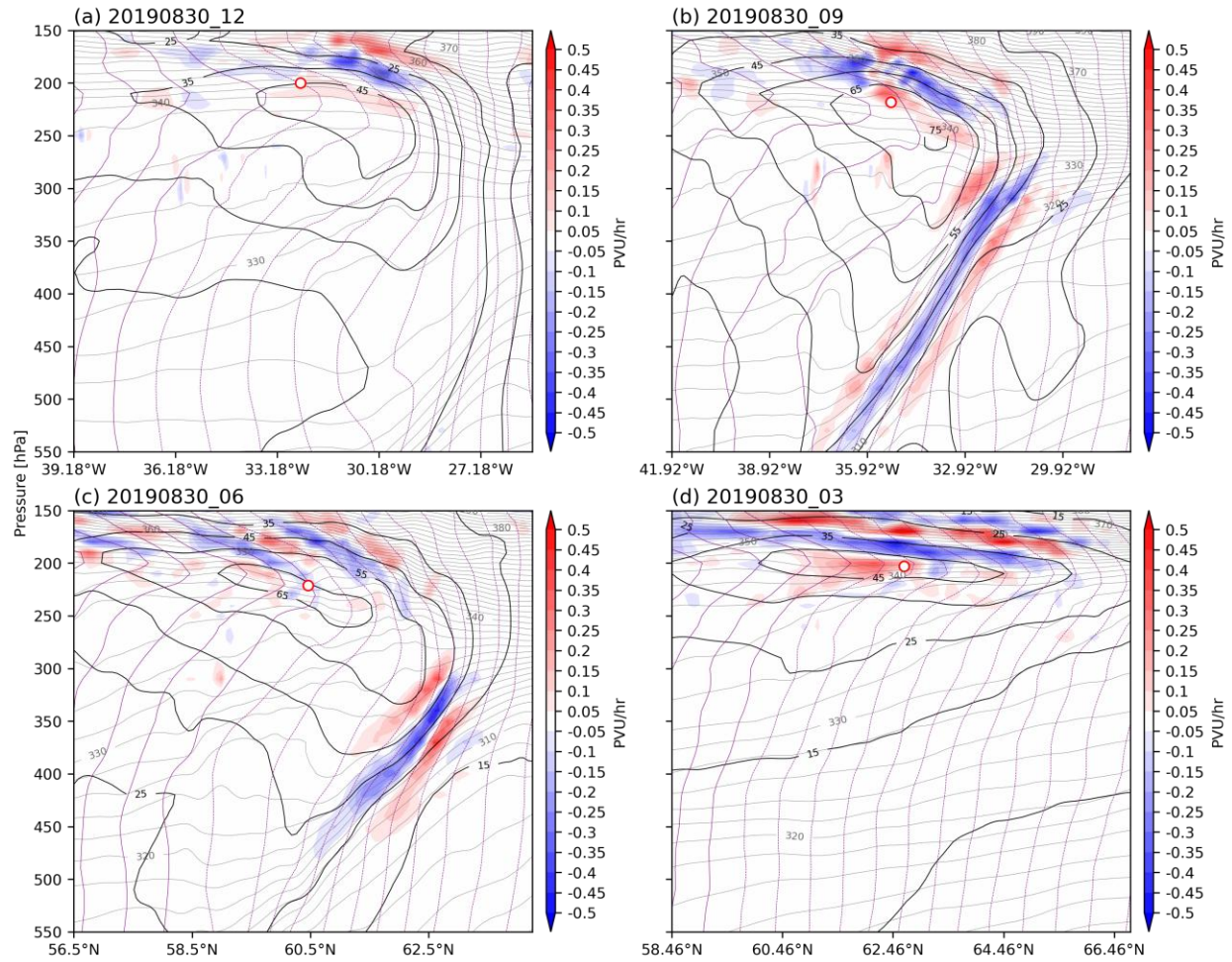


Fig. R5. Vertical sections of PV tendency due to turbulence along trajectory ‘7e’ on 30 August 2019 at **(a)** 12 UTC ($t=0$ h), **(b)** 09 UTC ($t=-3$ h), **(c)** 06 UTC ($t=-6$ h), and **(d)** 03 UTC ($t=-9$ h). The sections are perpendicular to the local wind vector and the position of the air parcel ‘7e’ is indicated by the red circles with white filling. Isotachs of u (black contours), isentropes (grey contours) and isopleths of m (purple contours) are overlaid.

The inaccuracy of trajectory calculation (leading to the crossing of model airmass) may have contributed further. We would not rule out this possibility. However, given Fig. R4 and R5, we think that this is not purely the result of errors in trajectory calculation, but physically related to the temporal evolution of the upper-level jet and the movement of the air parcel relative to the jet.

5. Section 7.1. I suspect the difference from our 2017 paper is, as you suggest, due to using tropopause-relative vertical composites over many case studies. I’m convinced from reading this paper that we should see these tripoles with the PV tracers, but they could be blurred out when averaging over different cases and other situations. I would be very

surprised if the difference was due to differences between the trajectory method and the PV tracer method.

Thanks for sharing your thoughts on this discrepancy! We are delighted that our study convinces you that PV tendency tripoles should exist and we both suspect the apparent difference is most likely due to methodological choices. Given your insight, we will emphasise the usage of tropopause-relative vertical composites over many cases as the most probable reason in the revised version.

The vertical compositing I did also miss out large regions in these jet cases because the tropopause is nearly vertical. I remember Heini Wernli pointing out this limitation in my PhD viva. I think it would be good to highlight that this is a limitation of our 2017 paper, not just a difference.

Thank you for the reminder. We will inform the readers about this discussion in the revised version.

You can rule out the difference in accumulation time because Figure 7c/d in the 2017 paper does show the accumulation over time.

Thanks a lot for the clarification. We will rephrase the part on accumulation time.

The PV tracer for turbulence also does include the full tendencies from the turbulent mixing and you can see the signature of negative PV tracer being transported out of the boundary layer, so it is possible that this is also masking the positive part of the tripole.

Thanks for this input. We will add this information when addressing the possible reasons of the discrepancy found between the 2017 study and the current study.

6. I was thinking this work might also explain the systematically negative dynamics tracer inconsistency at tropopause level seen in our 2017 paper. This PV tendency largely results from the numerical diffusion of wind and potential temperature, so would be similar to your turbulence tendencies (but without the K dependence). Interestingly there is a noticeable tripole in this tracer Figure 5c but it gets dominated by the negative tendencies at longer lead times. I would assume because the air further from the tropopause on average is advected away more.

Comparing your 5a and 5b, it looks like the APV is generally showing weaker magnitudes than Delta Q which could be consistent with the nonconservation of PV from the model dynamics also giving tripoles of PV and being missed from APV. There could also be many other explanations. Have you considered any explanations for this difference?

We greatly appreciate that you could find a linkage between our results and your previous findings on the effect of numerical diffusion. We agree that numerical diffusion may share some similarity with turbulence parametrisation. However, since we cannot explicitly investigate the effect of numerical diffusion in the IFS, we cannot comment further on the tripole signal in Fig. 5c of Saffin et al. (2017).

For Fig. 5a and b in our manuscript, we agree with your observation on the generally weaker magnitudes of APV compared to ΔQ . One reason we have considered concerns the time discretisation during the computation of APV. This is likely the reason for the localised spots with very large magnitudes of APV, probably related to convective activities (eastern edge of the cutoff). Nevertheless, this argument cannot satisfactorily explain the generally (or even systematically) weaker APV.

Therefore, we think numerical diffusion is a possible candidate. We think this is particularly likely for the stratospheric air within the trough, where many fine-scale features are present in ΔQ , but not APV. Since physical non-conservative processes are likely less active in this region (lower stratosphere), we suspect the fine-scale features in ΔQ are related to numerical diffusion. However, as we cannot quantify the effect of numerical diffusion, we keep this as a speculation, but we welcome further discussion in personal communication.

7. In section 7.3, you mention the dependence of the turbulent Prandtl number as important for determining if the PV flux is convergent or divergent in the rare cases. I think it is also worth highlighting that in the typical case it does determine whether the flux is more along isentropes or across isentropes (mass flux into the isentropes and dilution of PV substance in Haynes and McIntyre terms).

Thanks a lot for your insight. We agree and we will mention it in the revised manuscript.

I was also wondering if it could be considered as some function of just $d\theta/dz$ and du/dz . The K terms are only dependent on these gradients (via the Richardson number) and are multiplied by these gradients separately to get the heat flux and momentum flux. It does look like from figure 4 that you get strong momentum fluxes where the heat fluxes are strong, but there are regions where it is solely momentum fluxes - poleward of the jet and the lowest part of the tropopause fold. A quick look at the IFS documentations tells me this wouldn't be straightforward to figure out, but may be worth an extra sentence in the conclusions.

Thanks for this suggestion. We did a preliminary exploration on this approach, but we find the results quite complex, with many additional assumptions required. Therefore, we did not proceed further. We will consider including a short comment in the conclusion.

To elaborate briefly, consider the turbulent fluxes as functions of $\frac{\partial \theta}{\partial z}$ and $\frac{\partial u}{\partial z}$. The turbulence potential temperature and momentum tendencies are then roughly functions of $\frac{\partial^2 \theta}{\partial z^2}$ and $\frac{\partial^2 u}{\partial z^2}$, and PV tendencies $\frac{\partial^3 \theta}{\partial z^3}$ and $\frac{\partial^3 u}{\partial z^3}$. As PV tendencies are related to the third order derivatives of θ and u , this high order dependence makes interpretation difficult. Furthermore, in the implementation of parametrisation, there are also threshold dependencies, which makes diagnosing PV tendencies directly from the distribution of θ and u very challenging.

Technical Corrections

1. L494 - trippole -> tripole

Thanks, we will correct this.

2. Appendix A1 - Replace alpha with 1/rho. It is only used here and not introduced

Thanks a lot for spotting this inconsistency! We will correct it.

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