Response to Reviewer 1's Comments:

I see that the authors have done a real effort to improve the manuscript, and have also addressed most of my previous comments well. Nevertheless, I still have several comments (given below). I'd rate them somewhere between major and minor, ticked minor but would strongly recommend to consider them before publication, to further improve the paper.

We sincerely thank the reviewer for their thorough evaluation of our revised manuscript and for acknowledging the improvements made during the previous round. We appreciate the reviewer's comments, which we agree will meaningfully strengthen the paper prior to publication. Line numbers refer to the clean version of the revised manuscript, without track-changes annotations.

Minor comments:

1. Method decription:

I do find the description of the methods clearly improved in this revised version. However, at some parts I'm still struggling with the method description.

* One example is the identification of vortex centers along the zero-wind contour (L107ff). Could you explain in more detail how the u=0 contour is found and parametrized? Specifically, how are the points on this contour defined - which are all, except (x1, y1) not grid points of the reanalysis (see Fig. 1). The arc-length steps s_i in Fig. 1 don't seem equal to me.

1. How the (u=0) contour is identified

The (u=0) ridgeline is extracted using a **marching-squares algorithm** applied to the 1.25° × 1.25° regular longitude–latitude grid of the reanalysis products. The algorithm operates cell-by-cell:

1) For each grid cell bounded by four neighboring grid points, we check the sign of zonal wind u at each vertex.

2) If the sign of u differs between the two endpoints of a cell edge, the (u=0) contour must cross that edge. The exact geographic coordinates of the crossing point are computed by **linear interpolation** between the two endpoints, using the magnitude of u at each endpoint as weights.

3) Segments from all cells are connected sequentially to form one or more continuous (u=0) isolines across the domain.

Because the contour crosses cell edges at interpolated positions, **almost no points on the (u=0) line coincide with original reanalysis grid points**, consistent with the schematic in Fig. 1. Only in the rare case where a grid point has exactly (u=0) would the contour pass through a native grid point. These steps are achieved in Python by using the functions: "*from scipy.signal import find_peaks*" and "*from scipy.interpolate import RegularGridInterpolator*".

2. Arc-length parameterization (s_i)

The reviewer is correct that the arc-length steps (Δs_i) are **not uniform**. This is not an error:

1) Each crossing point obtained from the marching-squares procedure lies on a cell edge. The distance between two consecutive crossing points depends on the angle at which the contour traverses each grid cell and therefore varies from segment to segment.

2) We compute the cumulative arc length (s_i) at each point as the cumulative great-circle distance from an arbitrary starting point on the contour, following the Earth's curvature.

3) When searching for local maxima of Montgomery stream function along the ridgeline, we use a fixed half-window of 7.5° central angle (≈ 833 km). This window is much wider than the typical grid step (~ 139 km at 30° N), so the non-uniform sampling of the contour has negligible impact on the identification of vortex centers.

Because this method follows that used by Siu and Bowman (2020) and has been extensively documented there, we have opted to retain the leaner methodological description in the manuscript.

* How is the connection between two identified vortices at subsequent time steps (the "tracking" procedure) exactly done (L130ff)? Some studies use trajectory techniques for that relation (e.g. Clemens et al., 2022, https://doi.org/10.5194/acp-22-3841-2022; Portmann et al., 2021, https://doi.org/10.5194/wcd-2-507-2021), but it is not clear to me from the text what is done here.

We apologize that the description in the original manuscript was insufficiently clear.

Our approach is a geometry-based vortex center tracking method in the Eulerian framework, rather than a Lagrangian air-parcel trajectory method such as those used by Clemens et al. (2022) and Portmann et al. (2021). Whereas trajectory methods track the 3-D motion of individual air parcels, ideal for studying transport pathways and air mass origins, our method tracks the temporal evolution of the dynamical center of each anticyclonic vortex (defined by the local MSF maximum along the (u=0) ridgeline) to characterize the position, propagation, and lifecycle of individual vortices within the different ASMA modes. This center-matching approach follows the methodology of Siu and Bowman (2020).

The tracking procedure consists of three core steps:

1) Independent detection at each time step: At every 6-hourly analysis time, we first identify all valid vortex centers independently using the criteria described in Section 3.1 ((u=0) ridgeline, local MSF maximum, vorticity sign, zonal wind direction, and minimum contour length filters). This yields a complete list of vortex positions for each snapshot.

2) Nearest-neighbor matching between adjacent time steps: For each vortex center at time t, we search for candidate centers at time ($t+\Delta t$, Δt = 6h) within a great-circle distance threshold of 10°. The candidate with the smallest distance is selected as the successor. If two (or more) centers at time t map to the same candidate at $t+\Delta t$, only the closest pair is retained; the other vortex is treated as terminating at that step. If no candidate exists within the threshold, the vortex is considered to have dissipated, and any unmatched center at ($t+\Delta t$) is treated as a newly formed vortex.

3) Persistent vs. transient classification: A vortex track is classified as **persistent** if successors are matched through at least 4 consecutive time steps (i.e., ≥ 18 hours total lifetime). All shorter tracks are classified as transient vortices.

The 10° distance threshold is identical to that used by Siu and Bowman (2020) and is motivated by the observed typical propagation speed of ASMA vortices. This ensures that physically connected centers are matched while avoiding spurious linkage of unrelated features. Because this method follows that used by Siu and Bowman (2020) and has been extensively documented there, we have opted to retain the leaner methodological description in the manuscript.

* Another example concerns Eq. 8: Please explain in the manuscript why the vertical advection and diabatic heating terms are not expanded. In the response letter there is some related explanation, but for readers this should also be in the paper. Please explain also why the mean advection term \bar(v)\partial_y\bar(q) vanishes.

In the revised manuscript, we define the background state as a purely zonal flow consisting of the subtropical westerly jet, the tropical easterly jet, and the (u=0) ridgeline that separates them.

The meridional advection of mean PV is given by $v\,\partial\overline{PV}/\partial y$ (i.e. $v' = v$), which follows directly from our definition of the background state. This background state is especially appropriate for the IP and TP modes, which are situated in the central part of the climatological anticyclone:

1) The time-mean flow on both sides of the ASMA ridgeline is predominantly zonal (i.e.

the ridgeline separates the tropical easterly jet from the subtropical westerly jet, so the time-mean meridional velocity $\bar{v} \approx 0$.

2) The background PV field is zonally uniform along the ridgeline, such that $\partial \overline{PV} / \partial x = 0$.

Although the time-mean meridional wind is larger in the eastern flank region where the BH mode occurs, we elect to use the zonal background state for all three modes for consistency and simplicity, and because it is unclear how to cleanly separate the background and eddy components of the meridional flow in the critical zone at the eastern edge of the ASMA.

We have streamlined the collection of the meridional and vertical advection terms in Eq. (5), revised the description of the background flow, and clarified our rationale for defining it in this way (L167-178).

2. Discussion of PV tendencies:

The discussion of the PV tendencies seems still somewhat confusing to me. What I would find helpful here is:

* Comparison of the total PV-tendency (Fig. 5d) to the sum of all terms on the rhs of Eq 8 or 10, to see how well the calculated budget is closed. This would be important for the interpretation of the effects of the calculated terms.

Thank you. We agree that evaluating the closure of the PV tendency budget is essential for interpreting the relative contributions of the diagnosed dynamical and diabatic processes. Figure R1 shows a budget closure analysis by comparing the total PV tendency with the reconstructed PV tendency obtained from the sum of all diagnosed terms on the right-hand side of Eq. (5). The reconstructed PV tendency, including horizontal advection and diabatic source term, reproduces the major spatial patterns, vertical structures, and magnitude of the total PV tendency. In particular, the characteristic tripolar PV tendency structure and the vertical extent of the positive and negative anomalies are well captured. The residual between the diagnosed total PV tendency and the sum of individual budget terms is smaller than the leading-order contributions and does not alter the interpretation of the dominant dynamical processes. This residual is also consistent in scale and structure with expectations for "eddy" advective transports that are resolved by the reanalysis model (timesteps ~ 10 minutes; full model grid) but not by our advective calculations (6-hourly; coarser grid); see, e.g., Wright et al. (2025).

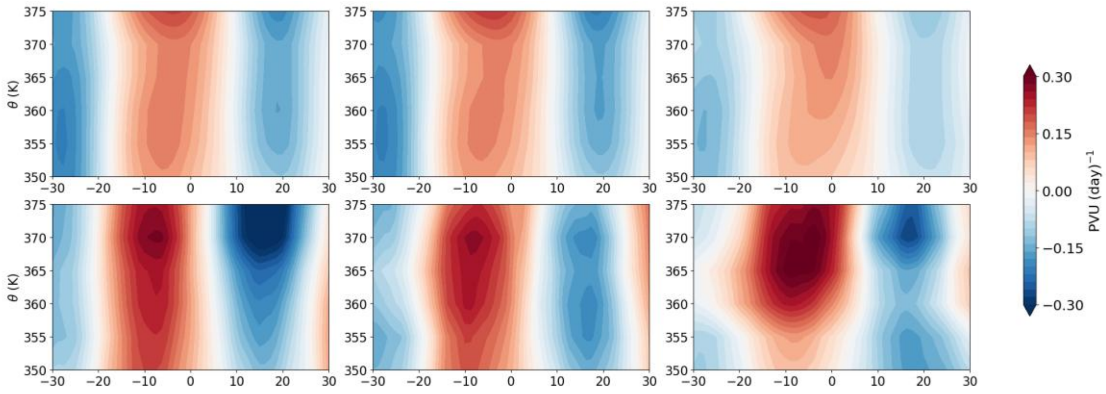

Figure R1: Vortex-relative composite distributions of ∂PV/∂t (top row) and the sum of all terms (advection + diabatic source term; bottom row) within the 350 K to 375 K layer, from left to right columns are JRA-3Q, ERA5 and MERRA2.

* Related is the question about the dominant contributions to PV-tendency. As the different terms largely compensate (in particular the horizontal advection terms), the net total tendency results as a residual from large opposite terms and hence I find it difficult to conclude which terms are the dominant ones.

From an absolute magnitude perspective, the mean zonal advection and mean meridional advection terms are indeed the two largest terms in the budget. Both exhibit dipolar spatial structures across the vortex, but with opposing signs: zonal advection produces a west-positive/east-negative dipole, while meridional advection produces a largely opposing east-positive/west-negative dipole. As a result, they cancel extensively when summed, leaving a much weaker net horizontal contribution. Although this contribution is weaker, it remains the leading contributor to the net tendency (Fig. R1; Figs. 6-9 in the manuscript), and especially to vortex propagation (Fig. 14 in the revised manuscript).

However, the diabatic terms (vertical advection + diabatic source term Q_θ) also play an important role. Although their absolute amplitude is smaller than that of the raw horizontal advection components, they experience far less internal cancellation, their sum is on the same order as the net advection term, and their distributions show greater differences across the three ASMA modes. Consequently, they substantially modulate the spatial pattern and amplitude of the residual net PV tendency and are the primary contributor to differences in intensification across the modes (see revised Fig. 13 and related discussion).

The near-cancellation of horizontal terms reflects the fact that the ASMA vortices evolve near-geostrophically in a balanced background flow, where adiabatic advection largely acts to redistribute PV horizontally without strongly modifying the local vortex strength. By contrast, diabatic heating, especially that due to convection, represents a forcing that drives vortex evolution. Our diagnostic framework is designed to disentangle these two roles: the relatively large advective terms describe propagation and redistribution, while the smaller but largely uncompensated diabatic terms dominate contributions to changes in vortex intensity. The application of this framework has been refined in this round, particularly the definitions of the "propagation" and "intensity" contributions (L339-348). We hope that these changes help to clarify the meanings and impacts of the individual terms.

The budget closure test described above confirms that the sum of all decomposed terms closely reproduces the directly computed total PV tendency, bar a small residual attributable to numerical discretization and unresolved sub-grid processes (e.g., covariance transports at time scales finer than our 6-hourly snapshots). In other words, the large compensation between the advective terms is physically meaningful, not an artifact of the decomposition.

We have revised and expanded the analysis and discussion that synthesizes these points (L358-L390; Figs. 13-14). We believe that these changes will help readers to correctly interpret the relative roles of each term and discourage them from equating raw amplitude with net dynamical importance

* A clearer discussion of the structure of the calculated tendencies (e.g. Figs. 5, S2) in terms of propagation and intensification of the anticyclone. E.g. the total tendency in Fig. 5d shows a clear dipole pattern with positive values to the west and negative values to the right of the anticyclone center. Shouldn't this mean that the vortices always strengthen at their eastern side and weaken at their western side, so move eastward? Maybe I misunderstand something here? Also, it would be great to have an explanation what the tripolar structure in PV-tendency terms exactly means for the dynamics of the anticyclone regarding propagation and intensification (Fig. 5d and L243ff; Fig. S2; L274ff).

Thank you. Your physical intuition about the dipole pattern is correct, and we have added detailed explanations to Section 4.2 of the revised manuscript to make these links more explicit (L249-L259).

First, we clarify the sign relationship that underpins all interpretation. The ASMA vortices are negative PV anomalies (low-PV cores) embedded in the background UTLS flow. Therefore, a negative PV tendency ($\partial PV/\partial t < 0$) means that local PV decreases and the anticyclone intensifies locally. By contrast, a positive PV tendency ($\partial PV/\partial t > 0$) means that local PV increases and the anticyclone weakens locally.

The tripolar pattern of PV tendency arises when the advective dipole is combined with the central monopole contribution from non-radiative diabatic heating. This tripole is the key outcome of our diagnostic framework, as it allows us to disentangle two distinct dynamical behaviors.

The dipole component (flanking peaks of opposite sign) is driven primarily by net horizontal advection. This component governs zonal propagation of the vortex center, as described above. As the dipole is most pronounced along the northern flank of the vortex, this distribution suggests predominantly eastward movement of vortices within the background subtropical westerly jet. It is important to reiterate that these are composite structures averaged over many vortex centers, and individual vortices may move westward or remain in place.

The central monopole component is driven primarily by vertical advection and the diabatic source term. This component, which modulates the overall strength of the vortex, is most pronounced along the southern flank of the vortex. A negative central tendency strengthens the entire anticyclone; a positive central tendency weakens it.

In other words, the tripolar structure describes a vortex that is simultaneously propagating zonally, embedded in the background zonal flow, and changing in intensity. This is a key advantage of our tendency-based framework over traditional PV-area metrics, which cannot distinguish between propagation-driven changes in vortex strength and true intensification.

* Also, I miss explanation concerning the structure of the different PV-tendency terms (L234ff). The structure of meridional advection (negative to the west, positive to the right of the anticyclone core) is more easily understandable given the structure of anticyclonic flow and the PV background gradient. So perhaps start with explaining that, and thereafter also the other terms in more detail. And please also add references to the respective subfigures the text is related to.

Thank you for this suggestion. We have substantially revised the discussion of the PV tendency budget, including a clearer explanation of the meridional advection term in relation to the anticyclonic circulation structure and the background PV gradient. In particular, we have focused throughout this section on distinguishing and explaining the northern advective dipole and the southern, primarily diabatic, monopole. The revised text now provides a more comprehensive interpretation of how each process contributes to the spatial evolution and maintenance of ASMA vortices (L249-259; L330-338; L358-390; L414-420).

* Why does the tripolar pattern of PV-tendencies indicate a process of intensification (L286ff)? In contrast, in L339 it is said "tripolar PV tendency structure (positive near the centre and negative to the east and west) governs the zonal propagation of the anticyclone". Regarding the discussion following L286: Why is the center of the anticyclone (red box in Fig. S2) defined as is? To me this red box seems more to be on the western side of the anticyclone center. A clearer reasoning would be helpful here.

The approach was inspired by studies of the development and movement of mid-latitude cyclones (Tamarin and Kaspi, 2016). In the revision, we have made substantial changes to the definitions of propagation and intensification tendencies to better fit our upper-level anticyclonic vortex composites. In particular, the previous dipole of red and blue boxes to the north of the vortex is used to define propagation in a way that positive tendencies to the west and negative tendencies to the east both contribute to eastward propagation (L341-347; L379-390). Intensification has been redefined to focus on the monopole south of the advective dipole, which contains the vortex center and is heavily influenced by diabatic processes (L346-348; L358-378).

Tamarin, T. and Kaspi, Y.: The Poleward Motion of Extratropical Cyclones from a Potential Vorticity Tendency Analysis, *Journal of the Atmospheric Sciences*, 73, 1687–1707, https://doi.org/10.1175/JAS-D-15-0168.1, 2016.

Specific comments and technical corrections

L61: blank missing: frominherited

Corrected in L64.

Fig. 1 / caption / 2nd line: closes --> closest

Corrected.

L155: Perhaps simplify sentence to "particularly useful to conserved adiabatic-frictionless flow"

Thank you for your suggestion; although we have revised the sentence, we believe the longer form better expresses our motivation for centering the analysis around PV.

L190: Explain in the text what the variables mttpm, mttswr, mttlwr exactly are, as this is likely not clear to all readers.

These definitions are given in the data section in L90-91. The sentence has been moved to L428, with a reference to the definitions added in the parenthetical.

Fig. 3: What exactly is the quantity shown in the figure? If it is simply vortex counts calculated over the period 2000-2020, then e.g. Fig. 3a would indicate that there are about 2 vortices present each time instance at 45E on 5th July (count is around 40). Shouldn't there be at most one vortex per time instance? Maybe I do misunderstand something here - please clarify the description of the figure.

Thank you for pointing out this ambiguity in the figure description. We revised the caption of Fig. 3 to clarify that “multiple vortex centers may coexist at individual time steps, consistent with the trimodal structure of the ASMA.”

At any given time, two or three vortex centers may coexist within the ASMA domain because our sampling grid is 2°×2°. The resulting trimodal structure: Iranian Plateau (IP), Tibetan Plateau (TP), and Western Pacific/Bonin High (BH) modes is a core finding of our study (affirming previous results) and the primary motivation for adopting a multi-center tracking algorithm. The ASMA is not a single, coherent, circular anticyclone, it frequently exhibits distinct, simultaneously active circulation centers corresponding to the three modes. This multi-center structure is well documented in previous work (e.g. Siu and Bowman, 2020) and is consistently reproduced across all three reanalysis products in our analysis.

L235: The text says that "vertical advection is positive near the core and negative farther eastward", but Fig. 5c rather indicates a dipole pattern with positive tendency to the west and negative tendency to the right. Please clarify what is meant here.

This sentence was modified in the updated manuscript. In L242-L245**:** “A dipolar structure of positive and negative PV tendencies is evident in both the horizontal advection terms (Fig. 6a-b). In the composite mean, zonal PV advection exhibits a positive tendency to the west and a negative tendency to the east of the vortex center above 300 hPa, while meridional advection is positive to the east and negative to the west. The dipoles thus largely cancel when the horizontal advection terms are summed together (Fig 6c)”

L282: Check wording in "the propagation and intensification are both account for the anticyclone"

Thank you; the word order has been corrected.

L302: I guess the reference here should be to Eq. 10 (not 8), as the discussion is related to 370K isentrope.

Thank you; the reference has been corrected.

Response to Reviewer 2's Comments:

The revised manuscript has been substantially improved compared with the previous version. The authors have carefully addressed many concerns. In particular: The vortex tracking section has been condensed and clarified with bullet points. The PV tendency framework has been simplified using isentropic coordinates. The discussion of ERA5 heating terms has been expanded. Figures and captions have been improved. The manuscript readability has significantly increased. The topic remains scientifically relevant and suitable for WCD. However, several minor but important issues require further clarification before the manuscript can be considered for publication.

We appreciate the reviewer's positive assessment and constructive feedback on our revised manuscript. We acknowledge the remaining points raised below and have implemented further targeted revisions in the manuscript to address each of them point-by-point.

1. ERA5 non-radiative heating still introduces uncertainty. Although the authors have expanded the discussion and corrected the previous plotting error, the ERA5 treatment still differs conceptually from MERRA-2 and JRA-3Q. Please consider to discuss the uncertainty this introduces into Figures 7–9 and the associated conclusions.

Thank you for this remark regarding conceptual differences in diabatic-heating treatment across the three reanalysis datasets and the associated uncertainties for Figs. 7-9 and our derived conclusions. We have added a paragraph discussing this uncertainty in the manuscript (L424-436) to explicitly address this point, along with two supplementary figures (Figs. S9 and S10) that provide direct comparisons of non-radiative vs moist-physics tendencies based on MERRA-2 and JRA-3Q, for which both approaches are viable.

As noted in Section 2 of the manuscript, MERRA-2 and JRA-3Q provide physically separate output fields for radiative and moist-physics (latent-related) temperature tendencies. By contrast, ERA5 does not directly output a variable corresponding to heating due to moist physics; we approximate its contribution as the residual of total model-physics temperature tendency minus the short- and long-wave radiative tendencies (mttpm-mttswr-mttlwr). This residual term contains contributions from other parameterized processes (turbulence, gravity-wave drag, etc.) that are cleanly separated from the moist-physics terms from MERRA-2 and JRA-3Q, creating a conceptual mismatch between ERA5 and the other two reanalysis products.

Although these additional non-radiative terms are considerably smaller than the moist physics term (typically ~two orders of magnitude; see also Fig. S9), this methodological difference introduces dataset-dependent uncertainty in the amplitude of the diagnosed latent-heating-related PV tendencies, especially near the tropopause where the moist physics term approaches zero and the other contributors may increase in relative importance.

Nevertheless, the qualitative spatial patterns (vertical-horizontal structure of radiative vs. moist-physics-driven PV tendency anomalies) are consistent across all three reanalysis products, as can be seen in Fig. 7b and Fig. S4b (see also Fig. S10).

The major large-scale dynamical conclusions of our study i.e., the competing roles of horizontal advection and diabatic forcing, the tripolar PV-tendency pattern, and mode-specific contrasts between IP, TP and BH vortices are robust across the three datasets and do not rest on the exact magnitude or vertical gradient of the ERA5 residual-estimated non-radiative heating. However, quantitative amplitude comparisons of latent-heating-related terms between ERA5 and the other two reanalysis should be interpreted with caution given this conceptual inconsistency.

2. Line 171: Physical interpretation of the PV tendency decomposition remains insufficient. The revised manuscript introduces Eq. (5) as the central diagnostic framework. However, the physical meaning of several terms and the underlying assumptions remains somewhat ambiguous. Specifically:

Why is only the zonal flow decomposed into a mean and perturbation component? The manuscript states that "the time-mean background flow along the rideline is defined to be zonal "$\bar{v} = \partial\overline{PV}/\partial x = 0$" but the justification for this assumption is not fully explained. Is this valid throughout the entire ASMA domain, or only near the vortex centers? The ASMA is embedded in a complex flow that includes both zonal and meridional gradients, particularly near the flanks of the anticyclone where meridional flow is strong.

The assumptions $\bar{v} = 0$ and $\partial\overline{PV}/\partial x = 0$ are adopted to simplify the background state. As noted in L168, our assumed background state is defined by the time-mean zonal-mean across the ASMA region, which consists of the tropical easterly jet, the subtropical westerly jet, and the approximately zonal ridgeline that separates them. We further assume that the background flow is isentropic. Although meridional flow increases toward the eastern and western edges of the anticyclone, it is difficult to separate this increase in the meridional flow from the action of the eddies themselves (i.e. wave breaking near the jet exit region). To avoid this complication, we adopt a simple zonal-flow background state throughout the domain.

It is important to emphasize that meridional advection is not neglected in our framework. The leading-order effect of meridional motion is captured explicitly by the $v\,\partial\overline{PV}/\partial y$ term in Eq. (5), which describes the advection of background PV gradients by the meridional flow and is a key contributor to both vortex propagation and intensification. We have tightened the explanation of the rationale behind our assumptions in Section 3.2 of the revised manuscript (L168-178).

Is the last term correct? In Eq. (5), the final term is written as: $-(\overline{\mathrm{PV}}+\mathrm{PV}')\frac{\partial\dot{\theta}}{\partial\theta}$. However, based on the standard PV tendency equation, the diabatic source term should be $(\overline{\mathrm{PV}}+\mathrm{PV}')\frac{\partial\dot{\theta}}{\partial\theta}$. Please confirm whether this is a typo in the manuscript.

Thank you. This typo has been corrected.

3. Line 151, the isentropic mass density definition. The isentropic mass density is defined as $\sigma=-\mathrm{g}\frac{\partial P}{\partial\theta}$. In standard atmospheric dynamics, the isentropic mass density is defined as $\sigma=-\frac{1}{\mathrm{g}}\frac{\partial P}{\partial\theta}$. Please confirm.

Thank you for noticing! We have corrected this error in the revised text.