This manuscript presents a trajectory-based analysis of the transport of smoke from documented North American pyroCb events during 2013–2023. The compilation of a large number of events and the attempt to characterize their seasonal, regional and vertical transport pathways are valuable. The applications to satellite interpretation and campaign planning are also of interest.

However, several important methodological limitations are not adequately reflected in the interpretation of the results. I therefore recommend major revision. The manuscript could become a useful contribution, should the central claims be better aligned with what passive, unweighted, seven-day trajectories can actually demonstrate.

Response: We thank the reviewer for the careful and constructive assessment of our manuscript. We agree that the manuscript should more clearly distinguish what can be demonstrated by the passive, unweighted, seven-day trajectory framework from broader interpretations and potential implications. In the revised manuscript, we will clarify these methodological limitations, revise or qualify statements where necessary, and better align the interpretation and conclusions with what the present analysis supports, as detailed in our responses below.

General remarks

1. Scope and claimed impacts

The title includes "potential impacts", but the study does not directly investigate radiative, chemical, cloud or air-quality impacts. These are mostly hypothesized from the transport patterns or discussed based on previous studies. The title and conclusions should therefore focus more clearly on transport and possible implications.

Response: We agree that the present study does not directly quantify radiative forcing, ozone changes, cloud effects, or surface air-quality impacts. The primary focus of the study is the transport of pyroCb smoke and the identification of the regions and atmospheric layers that may subsequently be influenced by these plumes. This is also reflected in the title, where "Smoke transport" is the primary focus and the qualifier "potential" was intentionally used for the associated impacts to distinguish these possible implications from impacts directly quantified in the present study.

In addition to the trajectory-based transport diagnostics, we use OMPS Aerosol Index and MLS CO as event-conditioned observational corroboration of the major transport pathways, providing corresponding signatures of absorbing aerosols and combustion-related trace gases. These observations are not intended as direct quantitative calculations of radiative, chemical, cloud, or air-quality impacts. Rather, the transport diagnostics and their observational corroboration identify where pyroCb smoke and associated tracers are transported, while the potential atmospheric implications of these pathways are discussed in relation to previous observational and modeling studies. Throughout the manuscript, we have also generally used conditional terminology such as “potential,” “can,” and “may” when discussing these effects.

For these reasons, we believe that the current title appropriately reflects the scope of the study and prefer to retain it. However, we agree with the reviewer that the distinction between the transport demonstrated by the present analysis and its possible downstream implications should be made clearer. Therefore, in response to this comment and the related comments below, we will revise the manuscript throughout—including the Methods, Results, Discussion, Conclusion, figure descriptions, and other relevant passages—to more consistently distinguish the transport and observational findings demonstrated here from potential radiative, chemical, cloud, air-quality, and dynamical implications. We will also revise or qualify specific statements where the present wording is stronger than supported by the trajectory analysis, as detailed in our responses to the comments below.

2. Self-lofting and SWIRLs

The trajectory calculations do not account for radiative self-lofting, which can substantially modify both the altitude and horizontal pathway of intense smoke plumes as they ascend through vertical wind shear, whereas the climb rate of the intense plumes during the first days can be several hundred meters per day. Although self-lofting is acknowledged, its effects on the results shown in Figs. 3–6 are neither evaluated nor adequately discussed.

Similarly, the SWIRLs formed from intense stratospheric plumes are closely linked to self-lofting and can strongly affect residence time and meridional transport. The 2017 PNE, which produced several such vortices (Lesterlin et al., 2021), would provide a useful test case for comparing passive trajectories with observed plume evolution.

Response: It is true that the current trajectory calculations do not directly account for radiative self-lofting. However, some influence of aerosol-related heating may be indirectly represented in the MERRA-2 meteorological fields used to drive LaTM, as discussed in lines 428–432. We will clarify this limitation and its implications more explicitly in the revised manuscript, including in response to the related comments below.

Thank you for sharing the reference by Lesterlin et al. (2021) on SWIRLs; we will include it in the revised manuscript where SWIRL dynamics are discussed. However, to our knowledge, the 2017 PNE is an exceptional event among the 545 pyroCb events analyzed in this study in terms of producing strong and persistent SWIRLs. A detailed comparison of the observed PNE plume evolution with passive LaTM trajectories would require a separate event-focused analysis and is beyond the scope of the present climatological study. Moreover, the evolution of the PNE and its associated vortices has already been investigated in detail in the literature, including the study suggested by the reviewer.

We nevertheless agree that such a comparison would be useful and interesting, and we plan to investigate this in a future dedicated study using available observations, potentially including our balloon measurements.

3. Meaning of the trajectory PDFs

The trajectory PDFs represent the frequency of idealized parcel occurrences, not aerosol concentration or smoke loading. Each event appears to contribute similarly, irrespective of its emitted mass or intensity, and all events are initialized using the same five-kilometre-deep vertical layer. Therefore, terms such as “concentration” and “smoke loading” should be used more cautiously.

Response: We agree that the trajectory PDFs should not be interpreted as actual aerosol concentration or smoke loading. We have already noted this in the Discussion (lines 633–638), but we will make it clearer earlier in the manuscript and use these terms more cautiously throughout.

Since this is a climatological study of 545 pyroCb events, we used the same general initialization approach for all events so that they could be compared consistently. The 5-km vertical layer was chosen as a representative plume depth and also appeared reasonable based on the case-study and sensitivity analyses. However, we agree that it does not represent the actual mass distribution of every individual plume. The PDFs should therefore be interpreted as relative parcel-occurrence statistics rather than mass-weighted aerosol concentration.

4. Source distribution versus transport susceptibility

The apparent hotspots are determined first by where pyroCbs occurred during 2013–2023 and only subsequently by atmospheric transport. Therefore, the maps conflate source frequency with downstream transport.

The diagnostics cannot, by themselves, identify regions intrinsically "more susceptible" to smoke pollution or predict future smoke risk. Future source locations will depend on fire weather, fuel availability from previous burning, among other factors. For example, one would not necessarily expect the same frequency of severe pyroCb-generating fires in areas where the available fuel has recently burned. The authors should explicitly state that the results are conditional on the observed 2013–2023 source distribution. Claims about future susceptibility, aviation risk and broader wildfire pollution should be considerably qualified.

Response: We have already tried to clarify and distinguish these different contributing factors throughout the manuscript, including the source distribution and the atmospheric conditions controlling the subsequent transport. For example, our seasonal and regional analyses discuss how the transport patterns are influenced by both pyroCb frequency/location and the meteorological conditions. Since this is a climatological study, separating the individual contribution of each factor is not always feasible or intended.

We also already note in Section 7 (lines 533–536) that these transport diagnostics are conditional on the 2013–2023 period and should be interpreted as a probabilistic guide rather than a prediction for a specific future event. We agree, however, that additional factors such as fire weather and fuel availability may influence future pyroCb occurrence, and we will revise the manuscript to make this clearer. At the same time, we believe the broader and stronger transport features identified in this climatology remain useful for indicating likely future transport patterns under similar source and meteorological conditions.

5. UTLS definition and interpretation of vertical transport

UTLS is not used consistently. In some passages it includes the upper troposphere and lower stratosphere, whereas elsewhere it is treated as a separate third layer. The tropopause and UTLS definitions should be clearly stated and applied consistently. The language should be revised consistently. Statements such as transport "from the troposphere into the UTLS" are awkward when the upper troposphere is itself part of the UTLS. Similarly, a convective overshoot normally refers to penetration above the tropopause rather than merely into the broadly defined UTLS.

Response: We agree that the definitions and terminology should be stated more clearly and applied more consistently throughout the manuscript. In Fig. 5, the tropospheric category includes parcels located below the tropopause and the stratospheric category includes parcels located above the tropopause, while the UTLS is defined as the ±2 km region surrounding the tropopause. Thus, the UTLS diagnostic intentionally includes portions of both the upper troposphere and lower stratosphere and is not intended to represent a completely separate third atmospheric layer. We will clarify this definition in the Methods, Fig. 5 caption, and related discussion.

In lines 426–428, our intention was to describe the vertical transport of smoke from the lower and middle troposphere toward the UTLS by deep convection. We agree that the term "convective overshoot" is not appropriate in this context because overshooting convection specifically implies penetration above the tropopause. We will therefore revise this wording and describe the process more appropriately as deep convective vertical transport.

For Fig. S6, we will reproduce the cross-tropopause transport analysis using the diagnosed tropopause itself as the crossing threshold—3.5 PVU in the extratropics (|latitude| ≥ 20°) and 370 K in the tropics (|latitude| < 20°)—so that there is no ambiguity in the definition of cross-tropopause transport.

We will state these definitions and their distinct purposes explicitly throughout the revised manuscript so that the UTLS definition and terminology, the Fig. 5 altitude-relative diagnostics, and the Fig. S6 cross-tropopause transport diagnostic are clearly distinguished.

6. Tropical transport and apparent ascent

The conclusion that smoke "ascends through the tropical UTLS" is not supported by the analysis. In the latitude–altitude composites, air transported equatorward along rising isentropic surfaces will appear at progressively higher absolute altitudes even without substantial cross-isentropic ascent.

Changes in parcel altitude, potential temperature and tropopause-relative altitude as a function of age would be more informative than the present aggregated cross-sections. The possible role of cloud processing and scavenging for smoke entering the tropical tropopause layer should also be discussed.

The role of the North American Monsoon Anticyclone is particularly underdeveloped, although it is likely important for smoke originating over the western US. NAMA should be considered before invoking interaction with the ASMA farther downstream.

Response: We agree that the increase in absolute altitude toward the tropics should not by itself be interpreted as evidence that the smoke undergoes cross-isentropic ascent through the tropical UTLS. Our interpretation was based on Fig. 6 together with the additional dynamical context shown in Fig. S5. In Fig. S5, some of the projected transport vectors are approximately aligned with the isentropic structure, consistent with equatorward transport along rising isentropic surfaces, while some vectors cross the isentropic contours and may indicate a cross-isentropic component. The figure also shows the approximate PV structure and tropopause height for additional context. However, these diagnostics are not sufficient to separate or quantify the relative contributions of isentropic transport and cross-isentropic ascent.

We will therefore revise the wording "ascends through the tropical UTLS" and related statements to present a more cautious interpretation: the increase in altitude toward the tropics may reflect a combination of equatorward transport along rising isentropic surfaces and, for some parcels, a possible cross-isentropic component. We will make clear that the present analysis does not quantify the relative importance of these processes.

We agree that a dedicated analysis of parcel altitude, potential temperature, and tropopause-relative altitude as a function of age could provide a more quantitative separation of these processes. The present Fig. 6 already includes absolute and tropopause-relative altitude together with parcel-age information, while Fig. S5 provides the corresponding isentropic, PV, and tropopause context. For the present climatological study, we will clarify the interpretation of these existing diagnostics and avoid making a stronger attribution than they support.

We will also briefly discuss the possible effects of cloud processing and scavenging on smoke reaching the tropical tropopause layer. In addition, we agree that the role of NAMA should be discussed more clearly. We already identify the recurrent summer subtropical-Pacific pathway as being consistent with NAMA/subtropical-ridge influence, and we will expand this discussion before considering possible farther-downstream interaction with the ASMA.

7. Model validation

Section 4 is lengthy and repetitive, yet the validation remains qualitative and is restricted to the first seven days after one event. Visual similarity between the trajectory PDFs and CALIOP or OMPS features does not establish model accuracy in a quantitative sense.

The section should be shortened and possibly strengthened using objective diagnostics, for example the plume-altitude or spatial bias.

A second example addressing intercontinental transport is necessary because trans-Atlantic pathways are one of the main results. An event involving substantial self-lofting or vortex confinement would be particularly informative.

The statement that LaTM appears to perform better than NAME based on visual comparison should be removed unless a like-for-like comparison is performed.

Response: LaTM has been previously validated and applied in several studies, including applications to wildfire, pyroCb, and volcanic plume transport, as discussed in Section 3. In the current manuscript, our intention was to provide an additional pyroCb-specific evaluation using the Chuckegg Creek event. We agree that Section 4 is lengthy and will shorten it in the revised manuscript while retaining the key comparisons with CALIPSO and OMPS.

We do not plan to add a validation case involving substantial self-lofting or vortex confinement. LaTM does not explicitly represent radiative self-lofting or smoke-vortex dynamics, and an event strongly controlled by these processes would therefore not be an appropriate direct validation case for the wind-driven trajectory framework used in this study. Incorporating and evaluating a self-lofting capability in LaTM would require substantial additional development and analysis, which we plan to investigate in future work. Moreover, among the 545 North American pyroCb events considered in our 2013–2023 dataset, the 2017 Pacific Northwest Event (PNE) is, to our knowledge, an exceptional case documented to have produced persistent smoke-charged vortices and substantial self-lofting. We will nevertheless discuss the potential influence of self-lofting on our transport diagnostics as a model limitation.

We also note that the existing Chuckegg Creek evaluation already provides evidence for trans-Atlantic transport within the 7-day integration period. On days 6–7, the LaTM trajectories extend toward Greenland and Iceland, where corresponding OMPS AI signatures are observed in Fig. S3. Therefore, we believe that the current evaluation already provides support for the 7-day trans-Atlantic transport pathways examined in this study. However, in response to the reviewer's suggestion, we will add a second case study focused specifically on trans-Atlantic transport to further demonstrate and evaluate LaTM's capability to reproduce this pathway. This additional case will be selected to represent predominantly meteorologically driven transport rather than an event dominated by substantial self-lofting or vortex confinement.

We will also remove the statement suggesting that LaTM performs better than NAME, since a like-for-like quantitative comparison was not performed.

8. Satellite and balloon comparisons

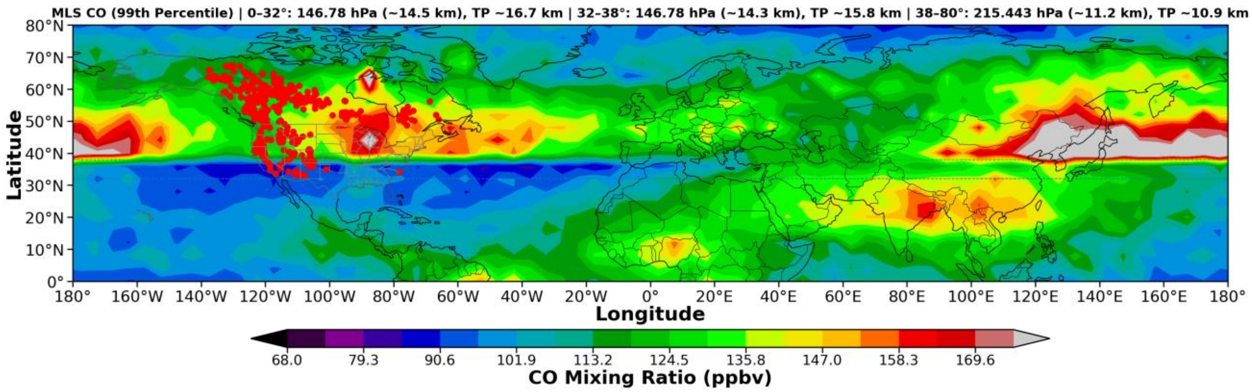
The climatological OMPS AI and MLS CO fields may contain unrelated signals from African and Asian dust, African and Amazonian fires, Asian pollution and ASMA circulation. Their broad spatial similarity with the trajectory PDFs should therefore be described as contextual corroboration rather than source-specific validation.

The balloon measurements are interesting but weakly integrated into the study. They are discussed at disproportionate length while being presented mainly in the supplement. If they provide direct validation, the source attribution and comparison should be shown more clearly in the main text. Otherwise, the discussion should be shortened and retained as an example of possible operational application. A separate study may be more suitable for the detailed balloon results.

Response: The OMPS AI and MLS CO data were processed for the subsequent 7 days after each event, and only the higher-percentile values were selected and plotted as contours in Fig. 7a and b. In addition, the diminishing tails from other sources (e.g., Asian dust, pollution, and ASMA-related circulation) create a relatively clear distinction between the North American signatures and those associated with other regions. For MLS CO, this distinction is somewhat less clear around the 15–20°N latitude band, but the North American pyroCb-related signature can still be inferred.

If we use MLS CO values from pressure levels closer to the tropopause (see the figure below), the MLS CO contour distribution shows reasonably good agreement with the LaTM results, particularly Fig. 5 (middle), which represents the UTLS layer. The OMPS AI distribution also shows a closer correspondence with the UTLS transport pattern in Fig. 5 (middle). Therefore, we believe that the AI and CO signatures provide observational corroboration of the North American pyroCb transport pathways. Some influence from unrelated sources may still remain, and we will elaborate on this limitation and interpretation in the revised manuscript.

We will also shorten the discussion of the balloon measurements. Our intention was to demonstrate the potential operational application of this study, rather than to use the balloon measurements as direct validation. We will make this clearer in the revised manuscript.



Specific remarks

L. 90–91: Define OMPS-AI and MLS-CO at first use.

Response: We will define OMPS Aerosol Index (OMPS-AI) and MLS carbon monoxide (MLS-CO) at their first occurrence in the revised manuscript.

L. 128–131: The proposed interpretation is unclear. A lower tropopause does not necessarily constitute a barrier to the absolute vertical development of a pyroCb. Moreover, if it limits vertical development, why are the tropopause-relative injection heights apparently greater at higher latitudes? Please reconsider or better explain this interpretation.

Response: We are referring to the absolute injection altitude of the pyroCbs, as shown in Fig. S1. In Fig. S1, we plotted the absolute injection altitudes of the pyroCb events and calculated the mean absolute injection altitude as a function of latitude. We will clarify this interpretation in the revised manuscript.

L. 174: Please provide more details on the MLS CO product, particularly the pressure level, used.

Response: We have already stated the pressure level used for the MLS CO analysis (215 hPa) and provided some related information in lines 470–474. However, we agree that these details should be introduced earlier. We will provide additional information on the MLS CO product and clearly specify the pressure level used in Section 3, where the dataset is first described.

L. 179–183: Why are trajectories initialized only at and below the reported maximum injection altitude? Given the uncertainty in the maximum height, a sensitivity test including levels above it would be useful. More importantly, the uniform five-kilometre layer cannot be assumed to represent “the entire mass” of each plume.

Response: We have conducted and included a sensitivity test in the Supplement (Fig. S4) that adds 1 km to the reported maximum injection altitude, as briefly described in lines 293–296. In the model validation section, we used an initialization altitude of 13.7 km (12.7 km + 1 km), adding 1 km to account for uncertainty in the reported maximum injection altitude of 12.7 km, as stated in lines 226–229.

We also tested the sensitivity of releasing air parcels only from the reported maximum injection altitude and compared those results with the 5-km-deep initialization. Although this comparison is not shown in the manuscript, the major transport features showed broad agreement between the two approaches.

The 5-km layer was used as an overall representative smoke-layer thickness for all 545 events in this study, and the model validation section shows that this thickness works reasonably well for representing plume vertical thickness. However, we will revise the manuscript to clarify that the 5-km layer may not represent the entire mass of an individual event.

L. 193: Which tropopause definition from the repository is used? Please specify whether this is a lapse-rate, cold-point or dynamical tropopause.

Response: We will specify the exact tropopause definition used from the repository in the revised manuscript.

Figure 2: The OMPS AI panels are difficult to read. Please adjust the colour scale and panel size.

Response: We will reproduce Fig. 2 with an adjusted OMPS AI color scale and larger panel size to improve readability in the revised manuscript.

L. 292–305: The sensitivity analysis is described as “rigorous”, but the supplement mainly presents qualitative comparisons. Please provide quantitative results or use more cautious terminology.

Response: We agree and will revise the wording to use more cautious terminology when describing the sensitivity analysis, since the comparisons presented in the Supplement are primarily qualitative.

L. 339–341: The higher altitude towards the tropics may mainly reflect isentropic equatorward transport rather than diabatic ascent.

Response: We agree that the higher geometric altitude toward the tropics may partly reflect equatorward transport along rising isentropic surfaces, and we will include this interpretation in the revised manuscript. At the same time, Fig. S5 shows that while some of the projected transport approximately follows the isentropic structure, some vectors also cross the isentropic contours, suggesting a possible cross-isentropic component. Therefore, the observed increase in altitude may reflect a combination of isentropic equatorward transport and some cross-isentropic/diabatic ascent. We will revise the text to distinguish these processes more clearly, while noting that the present diagnostics cannot quantify their relative contributions.

Figure 3: The comment concerning "higher altitude in the tropics" seems to refer to panel c, not panel b. The colour scale should be adjusted so that features other than the broad meridional altitude gradient can be distinguished.

Response: We will explicitly label the panels as (a), (b), and (c) and refer to them accordingly in the text to avoid any confusion in the revised manuscript. Figure 3 is intended to provide an overview of the mean altitude distribution, while Figs. 6, 8, and S5 provide more detailed views of the vertical and altitude-gradient structures.

L. 421: "Ascend through the tropical UTLS" is an overinterpretation of the aggregated latitude–altitude distribution. The equatorward isentropic transport should be distinguished from actual cross-isentropic ascent.

Response: We agree that the aggregated latitude–altitude distribution alone cannot distinguish equatorward isentropic transport from actual cross-isentropic ascent. Our interpretation was based on Fig. 6 together with the additional dynamical context in Fig. S5 (bottom), where some projected trajectory vectors approximately follow the isentropic contours, while others cross them, including toward lower latitudes. This suggests that the increase in altitude toward the tropics may reflect a combination of equatorward transport along rising isentropic surfaces and a possible cross-isentropic component. However, these diagnostics are not sufficient to quantify their relative contributions or conclusively diagnose diabatic ascent. We will revise the wording "ascend through the tropical UTLS" and related discussion to make this distinction clearer.

L. 426–427: Tropospheric smoke includes upper-tropospheric smoke, so transport "from the troposphere into the UTLS" is ambiguous. Convective overshoot normally refers to penetration above the tropopause.

Response: We agree that the wording is ambiguous. Our intention in lines 426–427 was to describe upward transport from the lower and middle troposphere to higher altitudes, including the UTLS, through deep convection. We will revise this wording accordingly and use the term "convective overshoot" only when referring specifically to penetration above the tropopause.

L. 430: Please distinguish radiative self-lofting from dynamically driven ascent in warm conveyor belts. Evidence for radiative lofting from the free troposphere across the tropopause is not demonstrated here, nor am I aware of any solid observational evidence for such process.

Response: We would like to clarify that radiative self-lofting and dynamically driven ascent associated with warm conveyor belts are already discussed separately in the manuscript. In the paragraph at lines 421–432, we discuss several possible mechanisms for vertical smoke transport, including radiative self-lofting at lines 428–431. We also explicitly note at lines 431–432 that LaTM does not directly model this process. In the following paragraph (lines 433–439), we separately discuss dynamically driven ascent associated with warm conveyor belts (WCBs), stratosphere–troposphere exchange (STE), and other synoptic-scale processes. The WCB/STE pathway is also introduced in the Introduction (lines 54–57) with reference to Khaykin et al. (2025), and cross-tropopause transport is further illustrated in Fig. S6 and discussed at lines 525–527.

However, we agree that the statement at line 430 should better reflect the uncertainty regarding radiative self-lofting as a mechanism for transporting smoke across the tropopause into the lower stratosphere. We will therefore revise this statement and add relevant previous studies that have investigated radiatively driven lofting of smoke from the free troposphere toward the tropopause and lower stratosphere, including de Laat et al. (2012) and Ohneiser et al. (2023). We will also clarify that LaTM does not explicitly simulate or diagnose radiative self-lofting. However, because LaTM is driven by MERRA-2 meteorological fields, some large-scale aerosol-radiative influences may be indirectly represented in the meteorological forcing, although this should not be interpreted as an explicit representation of plume-scale self-lofting.

De Laat, A. T. J., Zweers, D. C. S., Boers, R., and Tuinder, O. N. E.: A solar escalator: Observational evidence of the self-lifting of smoke and aerosols by absorption of solar radiation in the February 2009 Australian Black Saturday plume, Journal of Geophysical Research Atmospheres, 117, https://doi.org/10.1029/2011jd017016, 2011.

Ohneiser, K., Ansmann, A., Witthuhn, J., Deneke, H., Chudnovsky, A., Walter, G., and Senf, F.: Self-lofting of wildfire smoke in the troposphere and stratosphere: simulations and space lidar observations, Atmospheric Chemistry and Physics, 23, 2901–2925, https://doi.org/10.5194/acp-23-2901-2023, 2023.

L. 443–445: Please use “parcel age” rather than “residence time”.

Response: We agree and will replace “residence time” with “parcel age” in the Fig. 6 caption and other relevant places in the revised manuscript.

L. 460–462: UV AI depends primarily on absolute aerosol-layer altitude and aerosol properties, not directly on tropopause-relative altitude.

Response: We agree and will revise this statement for clarity. What we intended to convey in lines 459–462 is that smoke occurrence is relatively enhanced near the tropopause at higher latitudes, as seen in Fig. 5 (middle). We did not intend to imply that UV AI depends directly on tropopause-relative altitude; rather, UV AI is influenced by aerosol amount, absorbing properties, and the physical altitude of the aerosol layer.

L. 465: Replace “above 70° N” with “north of 70° N”.

Response: We will replace “above 70° N” with “north of 70° N” in the revised manuscript.

L. 470–475: I do not see clear evidence of a coherent “tail” moving from Asia towards North America. The MLS field likely combines CO from several unrelated sources, including central African biomass burning, Asian pollution and monsoon transport, as well as Amazonian and North American fire emissions. The OMPS AI field can additionally contain Saharan dust. The role of NAMA is absent from this discussion, and ASMA is not defined at its first occurrence.

Response: We have addressed this in our response to Comment 8. We will define ASMA at its first occurrence in the revised manuscript. We have already briefly discussed the role of NAMA in lines 566–568; however, we will expand the discussion of NAMA in the revised manuscript.

Figure 8: Add panel labels (a), (b), etc.

Response: We will add panel labels (a), (b), etc. to Fig. 8 in the revised manuscript.

L. 557: Please clarify what is meant by “standard zonal flow”.

Response: We will clarify what is meant by “standard zonal flow” in the revised manuscript.

L. 567: Define NAMA at first use.
Response: We will define NAMA at its first occurrence in the revised manuscript.

L. 567–575 and L. 594–598: The role of the North American Monsoon Anticyclone in transporting western North American smoke towards the subtropical Pacific and lower latitudes deserves a more complete discussion.
Response: Thank you for the suggestion. A more detailed investigation of the North American Monsoon Anticyclone (NAMA) and its role in pyroCb smoke transport would require substantial additional analysis and will be considered in future work. However, in the revised manuscript, we will expand the discussion of NAMA and its possible role in transporting western North American smoke toward the subtropical Pacific and lower latitudes, as also noted in our responses above.

L. 633–637: The statement that the PDF is not mass-conserving is important and should appear earlier in the Methods.

Response: We will include this statement earlier in the Methods in the revised manuscript.

L. 693: “Ascends through the tropical UTLS” remains too strong and should be reformulated.

Response: We agree that this wording is too strong. As discussed in our responses above, Fig. 6 together with Fig. S5 suggests that the increase in altitude toward the tropics may reflect a combination of equatorward transport along rising isentropic surfaces and a possible cross-isentropic component. We will therefore revise “ascends through the tropical UTLS” and related wording to present this interpretation more cautiously.