

Responses to RC1

General Remarks

My ratings are based on a balance between the main part of the MS (very good, minor revision) and the descriptions of the case studies (poor, major revision).

The authors present a study on the evolution of aerosols over Wuhan during 2010-2024. The study is based on the use of a polarization lidar providing information on the vertical structure of aerosol particles and their polarization state as a source of information on aerosol sphericity to discriminate between dust and non-dust aerosol, where dust is considered natural and non-dust is considered anthropogenic. In support, they use a comprehensive set of data including satellite observations, ground-based monitoring data, air mass back trajectories, radiosonde data and meteorological information. Considering the variation of the lidar-derived AOD, the authors split the time series into two stages. During stage I (2010-2017) the AOD decreases, during stage II (2018-2024) the AOD is on average constant. The variations of the AOD are discussed, reasons for the decrease in Stage I are presented and fluctuations in Stage 2 are discussed. A similar analysis is made for dust and non-dust aerosol and for the latter also at two levels: in free troposphere and in the total atmospheric boundary layer. Time series of aerosol mass concentrations PM_{2.5} and aerosol precursor gases NO₂ and SO₂ are presented in support of suggestions of changes in aerosol chemical composition. In addition, two case studies are presented.

The manuscript is overall well-written and the analysis is interesting. However, I have some comments, questions and suggestions for further improvement which need to be addressed before it can be accepted for publication in ACP. In particular, while the main part of the MS is quite clearly presented (minor revision), two case studies have been added which are much less clear and need much clarification (major revision).

Response: We sincerely appreciate your thorough review and valuable comments in this round, which have greatly contributed to improving our work. We have clarified the definition of the two periods with distinct variation patterns in AOD and dust optical depth (DOD), and we have added further analyses and discussions on the causes of the observed ‘two-stage’ variation patterns over the past 15 years. In particular, the statements in the two case studies have been substantially modified to make them much clearer and more easily understood by readers. Point-by-point responses are provided, and the manuscript has been revised accordingly.

Specific Comments:

Comment: The main subject of this MS is the decrease of the AOD in 2 different periods. However, there is no discussion on how these different periods were determined.

Response: Thank you very much for pointing this out. Based on the long-term variation of monthly AOD, two stages can be preliminarily identified: an evident declining phase (Stage I) and a fluctuating phase (Stage II), with the transition occurring around 2017-2018. To determine the transition point between these two stages, we tested different scenarios by shifting the transitional month (i.e., the end of Stage I) from January 2017 to December 2018. The corresponding slopes of AOD variations for Stage I and II are presented in Figure 1R. Ideally, the transitional month would yield both the steepest decline in Stage I and a slope that is closest to zero in Stage II; however, identifying the optimal choice requires balancing between these two criteria. As illustrated in Figure 1R, the transition most likely appeared within the first 2-3 months of 2018. Therefore, for clarity and consistency, we adopt December 2017 as the end of Stage I and January 2018 as the beginning of Stage II. Relevant descriptions have been added in Section 3.1. (please see lines 187-193)

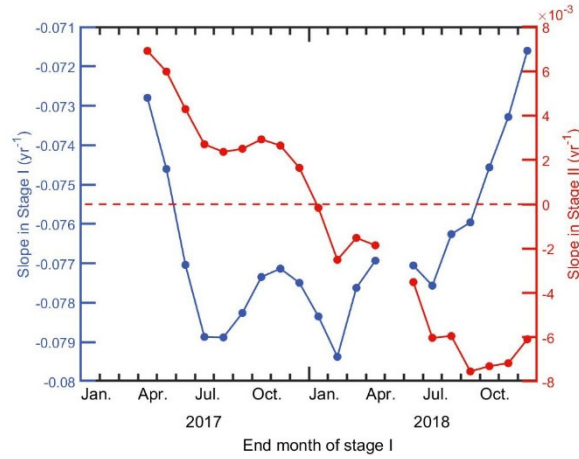


Figure 1R. Slopes of AOD variations for stages I and II by using each month from Jan. 2017 to Dec. 2018 as the transitional month.

Comment: Stage I is from 2010 to 2017, but for DOD it ends 2020. However, the DOD variation in stage II 2017-2024 is very small (smaller than for total AOD in stage II), so what is the motivation to extend the DOD fit to 2020?

Response: We do not subjectively determine the transitional point between the two stages of DOD variation. Slopes calculated by shifting the end month of DOD fit from January 2017 to September 2024 are presented in Figure 2R. When the DOD fit is terminated between 2017 and 2020, the resulting slopes remain around -0.010 yr^{-1} , indicating that the downward trend persisted during that period. It should be mentioned that in Figure 2R, the large fluctuation in DOD slope is primarily driven by pronounced seasonal patterns over Wuhan; nevertheless, a clear divergence between the two periods, with the end of 2020 as the transition point, is evidently seen. However, as the end of the DOD fit extends to months after 2021, the slope rapidly approaches zero. Therefore, we choose to extend the DOD fit to the end of 2020. The different transitional time for total AOD and DOD, derived from long-term lidar observations, is also a clear indication that the main characteristics of AOD over Wuhan are not controlled by natural sources.

We have added some sentences regarding the determination of the end of the downtrend for long-term DOD variation in the revised manuscript. (please see lines 251-253)

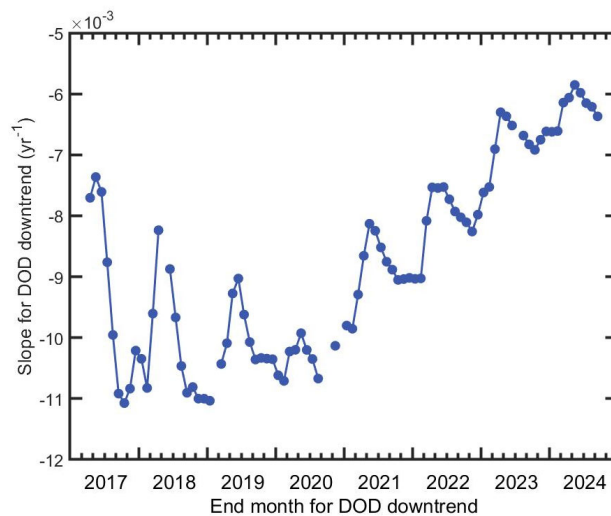


Figure 2R. Slopes of DOD variations for downtrend using each month from 2017 to 2024 as the end of the stage.

Comment: Another question is that for both total AOD and DOD a maximum is observed in 2014 and AOD was high and variable before the decline in 2014. The Clean Air Action plan started in 2013. Would it be more logical to start the fit in 2013? Does it make a difference?

Response: For AOD, the annual mean values in 2011 (0.89) and 2012 (0.83) were higher than those in 2014 (0.72). The exceptionally high monthly mean AOD of 1.18 in 2014 was caused by transboundary biomass burning smoke intrusion, as discussed in the case study in Section 4.1. If excluding this extreme case, the annual mean AOD in 2014 will decrease to 0.65, which is comparable to the 2013 value of 0.60. Therefore, the 2014 AOD still aligns with the long-term downward trend. Additionally, several air pollution control policies were implemented over the past 15 years to promote sustainable development. For instance, the “*Technical guidelines for air pollution control projects*” (Ministry of Ecology and Environment of the People's Republic of China, 2010) was issued at the end of 2010 and came into effect in 2011. The “*Air Pollution Prevention and Control Action Plan*” in 2013 was one of the most impactful policies. If adopting 2013 as the starting year of the fit, the slope of AOD in Stage I was -0.072 yr^{-1} , which is slightly less than -0.077 yr^{-1} obtained when starting from 2010. As a result, it is more reasonable to regard the long-term declining trend as beginning with the first year of our observations, i.e., 2010.

For DOD, dust emissions are mainly influenced by dust activities and meteorological factors in desert regions, rather than local urban pollution. As noted in section 3.2, recent extreme springtime dust events have been linked to usually strong Mongolian cyclone conditions, early spring snowmelt, and low soil moisture in the Gobi Desert (Gui et al, 2022; Chen et al., 2024).

Comments: The case studies need to be better explained.

1. In particular, the authors start with Fig. 7 with only information on the major event, on 13 June, in Fig. 7j. It would be more logical when Fig 8a would be shown as introduction to the event, followed by clear explanations of all other Figures presented.
2. In addition, the one air mass trajectory shown does not arrive on at the time of the peak PM, why are not more trajectories shown?
3. That one trajectory indicates an anticyclonic circulation, does that explain the day-to-day UVAI pattern? And the highest UVAI NW off the burnt area on 6.11 and south on 6.12 and the intensification and elongated pattern in 6.13?

Response:

1. Thank you for the reviewer’s suggestion. We have reversed the order of the original Figures 7 and 8, re-numbering them as Figures 8 (lidar observation) and Figure 9 (spaceborne measurements). The corresponding descriptions in Section 4.1 have been modified accordingly.
2. According to the reviewer’s suggestion, HYSPLIT backward trajectories originating from Wuhan for each day during 10-13 June have been added to Figures 9b-e. The statements have been revised to “**The two-day backward trajectories starting from Wuhan at an altitude of 1.5 km were simulated each day during 10-12 June using the HYSPLIT model (Figures 9b-d). Due to the lack of lidar observations over Wuhan on 13 June, we instead simulated a backward trajectory initialized at ground level at 0900 LT, based on the peaks of surface-measured PM_{2.5} and CO concentrations (see Figure 8a). All trajectories passed through the fire-affected areas, suggesting that the observed aerosol layers over Wuhan were likely associated with ABBS.**” (please see lines 412-417)
3. We have updated the backward trajectories and the associated analysis, and anticyclonic circulation is no longer involved. The backward trajectories of air masses originating from Wuhan passed through the fire-affected regions, indicating that the lidar-observed elevated aerosol plume over Wuhan likely originated from transboundary smoke, as supported by the height and time information of the lidar observations. While day-to-day UVAI variations reflected the dispersion of smoke plumes from the fire region, leading to enhanced UVAI values in Wuhan and the surrounding areas. Therefore, the combination of simulated backward trajectories and satellite-measured UVAI provides valuable evidence for inferring the origins and

compositions of the aloft aerosol layers observed in the free troposphere over Wuhan. For instance, the trajectories starting on 11 and 12 June (Figure 9c and d) traced back to areas of high UVAI on 10 and 11 June, respectively (Figure 9g and h). Similarly, the trajectory starting on 13 June approximately coincided with the intensification and elongation of the UVAI pattern on that day. The UVAI evolution has been described in detail as follows. “On 10-11 June, ABBS plumes initially generated in Anhui and subsequently dispersed toward the boundary region between Anhui and Henan provinces, with only a small fraction extending to Hubei. After 12 June, most ABBS were transported southwestward toward Wuhan and adjacent regions, resulting in a pronounced UVAI increase near Wuhan (Figure 8a).” (please see lines 417-420)

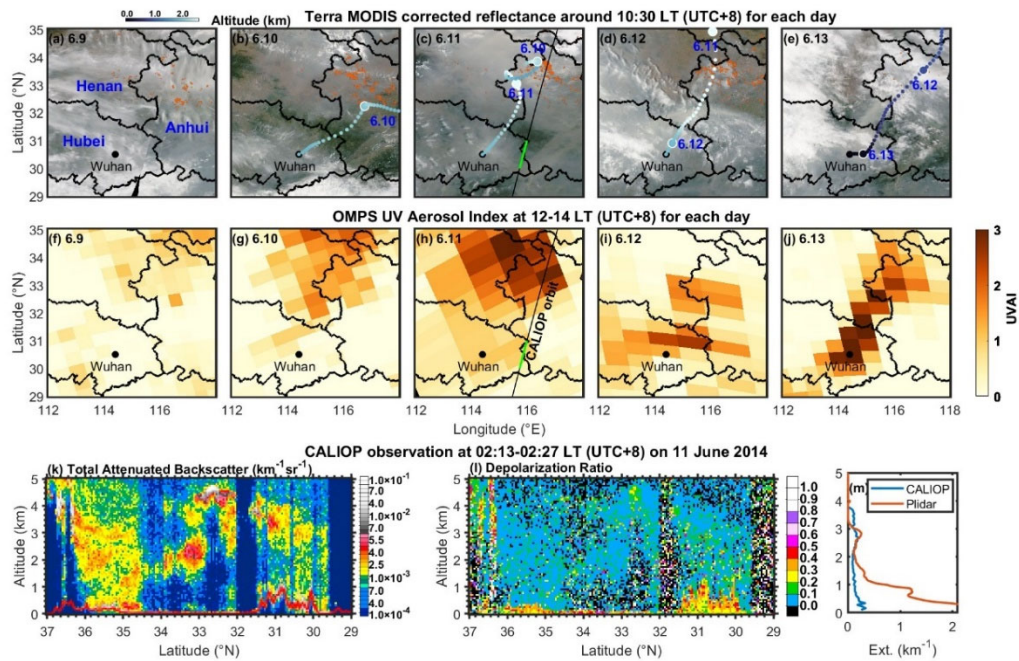


Figure 3R. (Figure 9 in revised manuscript) (a-e) MODIS corrected reflectance and fire/thermal anomalies in central China around 10:30 LT from 9-13 June (MODIS, 2024). Panels (b-d) includes 2-d backward trajectories originating from Wuhan at 1.5 km at 2000 LT on 10-11 June, at 0400 LT on 12 June, and at 30 m at 0900LT on 13 June; (f-j) UVAI measurements from OMPS at 12-14 LT during 9-13 June; CALIOP-observed 532-nm (k) total attenuated backscatter coefficient, (l) volume depolarization ratio, and (m) extinction profile between 30-31°N at 02 LT on 11 June. The extinction profile observed by polarization lidar at 02-04 LT on 11 June was also presented. The CALIOP orbit footprints corresponding to (k) and (l) are shown in (c) and (h), with green lines highlighting the profiles presented in (m).

Comments: In more detail:

1. Fig. 7 a-e: Why is the MODIS corrected reflectance shown? Or is the burnt area (red spots) the most important? Figs a-e show the haze but also the clouds in 3 out of 5 scenes (a, d, e) which, in e obscure all relevant info. What does that mean for the UVAI? Is it measured only above the clouds? Fig e does not show the burnt area because of the cloud cover.
2. Figs f-j indeed show the increasing UVAI, but, in view of the clouds, what is the relation with the surface concentrations in Fig 8a on 6.13? The variation of UVAI in Fig 8a shows remarkable good agreement with PM2.5, likely because it was scaled this way, but in view of the clouds this does not seem credible.
3. Why is Fig 7I shown? What does it tell us? Depol is very small.

Response:

1. MODIS corrected reflectance imagery was used to display the actual smoke plumes (in gray) together with prevailing weather conditions. Fire and thermal anomalies were used to indicate the specific fire locations.

UVAI was applied to show the absorbing aerosols that intermingled with or above clouds (Torres et al., 2007). The combination of these three datasets enables the identification of fire sources, as well as the diffusion and transport of fire-emitted smoke plumes. The revised statements are as follows. **“To illustrate the actual smoke plumes and prevailing weather conditions, Figures 9a-e present MODIS corrected reflectance and fire/thermal anomalies over the region spanning 29-35°N and 112-118°E during 9-13 June”** (please see lines 413-414) and **“To intuitively show the spatial extent of the smoke plumes, Figures 9f-j present the UVAI data.”** (please see line 420) Additional descriptions of MODIS and UVAI have been included in Section 2.2. (please see lines 118-138)

- As mentioned in the previous response, UVAI represents absorbing aerosols that are mixed with or located above clouds; thus, it has no direct relationship with $PM_{2.5}$. The observed increases in both were driven by smoke transported to Wuhan and descended to the surface, as shown by ground-based lidar, multiple satellite observations, and trajectory simulations.
- Figure 4R shows a local pollution event near Wuhan observed by CALIOP, characterized by enhanced backscatter concentrated below 3 km and low VDR values of 0-0.1 (in blue), indicating the presence of spherical particles. In comparison, the VDR in our case study ranged from 0-0.2, represented by alternating yellow (0.1-0.2) and blue (0-0.1), suggesting a mixture of spherical and non-spherical particles. Relatively high VDR values (>0.15) are indicative of fresh smoke that is less oxidized and surface-coated (Haarig et al., 2018). This result supports the interpretation that smoke plumes were transported over a short distance from adjacent provinces to Wuhan. Nevertheless, VDR alone is not a reliable indicator of particle aging, as it also includes contributions from molecular depolarization. The particle depolarization ratio (PDR) provides a more robust measure and was further adopted (using ground-based lidar observations as shown in Figure 8) to analyze the aging level of smoke particles. For this reason, we prefer not to overinterpret the CALIOP-derived VDR here. The revised description is as follows. **“On 11 June, CALIOP-observed total attenuated backscatter showed that the ABBS plumes were mainly distributed from the surface to an altitude of 5 km. The volume depolarization ratio varied from 0 to 0.2 (blue pixels with occasional yellow ones), indicating a mixture of spherical and non-spherical smoke particles. Relatively high VDR values (>0.15) generally suggest the presence of fresh smoke, which is less oxidized and surface-coated (Haarig et al., 2018).”** (please see lines 420-424)

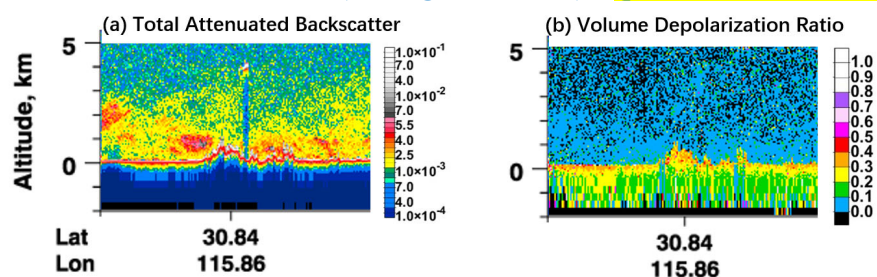


Figure 4R. CALIOP observed (a) total attenuated backscatter and (b) volume depolarization ratio of polluted continental aerosols near Wuhan at 1810-1823 UTC on 21 August 2017.

Comments:

- Figure 7 shows an air mass trajectory but why for only 1 day? Why not for every day? Or several times on a day? And what does it show? It went back from Wuhan, at 2000LT, and as indicated by dates (m.d), it arrived on the 11th? It probably started low, on the 9th, suddenly was lifted to 3 km and stayed there all day on the 10th and dropped again very fast back to 1 km on the 11th. Is that how we should read it? And then we should look at the other maps when the trajectory overpasses a burnt area and may have picked up smoke.
- The maps indicate it was on the 10th, between about 33 and 34 N, and the CALIOP track (what was the overpass time? Ascending or descending? What are the red lines at the bottom of the CALIOP Figures? Please mention) shows high AOD between 32 and 34 N but at the east of the air mass trajectory, and UVAI

(when was OMPS overpass LT?) was not very high under the CALIOP overpass (Fig. g). CALIOP has crossed the air mass trajectory but a little later. The air mass passed over the burnt area on 6.10 and may have picked up some smoke, but UVAI shows a relatively small signal. The green lines in the CALIOP trajectory indicate smoke presence but UVAI is not enhanced.

3. Fig. g to f suggests that smoke had been transported south and arrived over Wuhan. Why are Figures shown for 12 and 13 June?
4. In the text: L319-320 burnt area over northern Anhui, so why is smoke formed in Henan? How does that relate to the air mass trajectory and the CALIOP and OMPS observations?

Response:

1. In light of the reviewer's comments, more HYSPLIT-simulated backward trajectories (one trajectory simulated for each day) have been added in the updated Figure 9. Please recheck them.
2. For clarity, we have added the specific time (convert to local time, i.e., UTC+8) of the day in the title of subfigures for MODIS, OMPS, and CALIOP in the updated Figure 9. The overpass time of the CALIOP track is at ~02LT on 11 June (~18 UTC on 10 June), corresponding to the descending phase at nighttime. The red lines at the bottom of the CALIOP Figures indicate the ground levels. It can be noted that the overpass time of CALIOP is between the UVAI measurements on 10 and 11 June (Figures 9 g and h). The CALIOP orbit footprint overlaps with the strongly enhanced UVAI in northern Anhui and slightly enhanced UVAI in eastern Hubei (Figure 9h). The updated green line on the CALIOP orbit footprint represents the CALIOP measured extinction profile presented in Figure 9m. We have also added a detailed description of satellite observation in the revised manuscript. (please see lines 410-429)
3. Lidar observed the aloft aerosol layer from ground to ~3.5 km over Wuhan on 12 June, and surface PM_{2.5} measurement suggested the existence of high aerosol load near the surface of Wuhan. These observations indicate that smoke aerosol continuously impacted Wuhan on 12-13 June. This is why we also provided the Figures for these two days.
4. Smoke plumes were initially generated in Anhui and subsequently dispersed toward the boundary region between Anhui and Henan. Thank you for pointing out these contradictory statements. Relevant statements have been revised as follows. "ABBS plumes initially generated in Anhui and subsequently dispersed toward the boundary region between Anhui and Henan provinces, with only a small fraction extending to Hubei." (please see lines 417-419) We have clarified the combined observation of MODIS OMPS and CALIOP in previous responses, and the relevant revisions have been presented. (please see lines 410-429)

Comment: In other words: guide the reader and explain what is important!

Response: Thank you very much for the reviewer's valuable comments, in particular regarding the case studies. For clarity, we have added more detailed explanations in the revised manuscript (please see lines 410-429), as can also be seen from the responses to the comments above.

Comment: Likewise, guide us through Fig. 8. The first thing I wonder about is that PM_{2.5} and CO peak in the evening of 12 June. So why was the air mass trajectory not calculated for the peak PM_{2.5} and CO time? That would show the sources, right?

Response: Thank you very much for the valuable suggestion. More simulated backward trajectories during 10-13 June have been added. The backward trajectory passed through the fire-affected areas, suggesting that the observed aerosol layers over Wuhan were likely associated with the fire-emitted smoke particles. Relevant statements have also been revised in the manuscript accordingly. (please see lines 412-417)

Comments:

1. Another is that CALIOP overpass is early in the afternoon and your lidar observations started at 16:00 LT,

both on the 10th. Why is there no comparison?

2. There is a description between L324 and L339, but I miss guidance. For instance, L333-334; “severe air pollution was observed”: when (date, time, height), etc. Fig b shows me that BLH was about 1.5 km at 16:00 on 11.6, but the temperature profiles was at 20:00, when BLH had dropped to about zero. And depol at that time was close to 0.1 (red) in the BL. (in contrast to L336) and how do these contrasting statements and observation reflect the next sentence? (L336-339).
3. Further questions are how the extinction, and RH profiles are used? These profiles show clear gradients and transitions which may indicate different layers and may be connected with backscatter profiles in Fig 8b. The authors mention high RH. Which however is only shown in Fig 8e and 8g in a thin layer just above the surface. Further above the RH is so low that little or no hygroscopic growth can be expected. And also PDR does not show correlation with the RH variation.

Response:

1. On 11 June, extinction coefficient profiles from CALIOP near Wuhan at 02 LT and polarization lidar measurements during 02-04 LT have been included in updated Figure 9m of the revised manuscript. We have also added the following text regards the comparison “**In addition, Figure 9m compares particle extinction observed by CALIOP at 30-31°N with that measured by ground-based polarization lidar located approximately 140 km away. The particle extinction profiles agree well above 2.0 km. However, below 2.0 km, the mean particle extinction from the ground-based polarization lidar (0.71 km^{-1}) was significantly higher than that from CALIOP (0.15 km^{-1}), reflecting horizontal nonuniformity in aerosol loading, i.e., more severe urban air pollution compared to the relatively clean rural areas (green line in Figures 9c and d).**” (please see lines 424-429)
2. Thank you for pointing this out. The statement has been revised to “**Below 1.0 km, α_p increased from 1.08 km^{-1} on 10 June to 1.31 km^{-1} on 12 June, indicating that ABBS in the FT gradually descended and mixed into the BL.**” (please see lines 389-390) We previously overlooked the enhanced volume depolarization ratio in the afternoon of 11 June, which led to a PDR close to 0.1 at 0.8 km during the night of 11 June (purple line in Figure 8j). Accordingly, we have revised the previous conclusion that smoke particles near the surface were fully aged. The related statements have been revised to “**...From 10 June to the morning of 11 June, the mean δ_p of 0.06-0.07 was observed below 1.0 km, indicating that smoke particles mixed with urban/industrial pollutants near the surface and were sufficiently aged. However, in the evening of 11 June, some unaged soot particles descended to near-surface levels, with δ_p close to 0.1 at ~0.8 km.**” (please see lines 390-393) In addition, as BLH and temperature were not very relevant, we have removed them from Figure 8 and the corresponding discussions.
3. The explanation regarding RH is overly complicated. Since it does not affect the main conclusion of this work, we have decided to remove the related statements. Descriptions of extinction and PDR have been revised accordingly in the manuscript. (please see lines 385-393)

Comment: In summary: there is a lot to be said about Figs 7 and 8, please explain.

Response: More detailed explanations have been added in responses to the comments above.

Comments:

1. And this also applies to the haze case in Sect. 4.2. Figs 9d-k are not explained or used, so why are they presented.
2. Figs a-c are used but attributing the increase in PM only to NO₃ formation seems a big step. Why did that not happen on the previous days? Rather, the decrease in NO₂ in the evening of the 25th, as opposed to increase of nocturnal NO₂ due to chemical reactions, could also be due to a change in transport from areas with smaller NO₂ emissions.

Response:

1. The descriptions related to Figures 10d-k in the manuscript were as follows “on 26 January, a larger mean α_p of 1.11 km^{-1} and a lower mean δ_p of 0.05 were derived below 1.0 km , compared to values of $0.17\text{-}0.19 \text{ km}^{-1}$ and 0.11 on 23-24 January, respectively.” For clarity, “As seen from Figures 10d-k” has been added in the revised manuscript. (please see lines 446-447) To avoid complicated explanations and misleading, RH profiles and BLH have been removed in the revised manuscript.
2. The purpose of presenting the haze event is mainly to show the typical vertical distribution and optical properties (e.g., extinction and depolarization ratio) of winter urban pollution over Wuhan, based on our remote sensing measurements. Since we did not have comprehensive atmospheric chemical measurements, providing additional explanations would involve a risk of overinterpretation. To ensure rigor, we have removed analyses of NO_2 and O_3 from Figure 10a and the corresponding text in the revised manuscript. Only explanations and analyses of aerosol optical properties are retained, and the related statements have been revised accordingly. (please see lines 439-451)

Detailed suggestions

L42: In most of the para the authors discuss AOD, and the slow-down in the decrease of AOD, which is also discussed further below (L165). Therefore, it seems a bit strange that on L42 the slowdown in $\text{PM}_{2.5}$ is mentioned, which has been reported to respond differently than AOD.

Response: AOD and $\text{PM}_{2.5}$ concentrations are both important indicators of environmental quality. The $\text{PM}_{2.5}$ concentrations are closely linked to human activities near the surface, whereas AOD reflects the aerosol loading throughout the entire atmospheric column. The observed slowdown in $\text{PM}_{2.5}$ concentration reduction in China illustrates the weakening of emission control measures, which leads to the central question of this study: Do weakened emission control measures affect AOD over Wuhan? For clarity, we have highlighted our motivation as follows “In our previous study, we reported a consistent downward trend in AOD during 2010–2020 (Yin et al., 2021b). However, it appears that this decline in AOD ceased and even slightly reversed after 2018 (Yin et al., 2021). Given that the reduction in surface $\text{PM}_{2.5}$ concentrations also slowed after 2018 (Geng et al., 2024), it remains unclear whether the consistent downward trend in AOD merely decelerated or completely halted at some point after 2018. In addition, in the additional four years following 2020, several factors including industrial shutdowns during the COVID-19 pandemic, abnormal Asian dust events (Gui et al., 2022; He et al., 2022b), and extreme precipitation (Wang et al., 2023a; Li et al., 2024), may have further influenced AOD levels in Wuhan. Therefore, it is of great interest to extend the analysis by incorporating more recent datasets from our polarization lidar observations to assess the impact of the weakened emission control measures on AOD over Wuhan.” (please see lines 46-53)

L90 “Note that α_p values below 0.3 km were assumed equal to that at 0.3 km , possibly causing an uncertainty of <0.05 in AOD”. Fig. 6 shows seasonally averaged profiles with strong gradients above 300 m . Would it be more logical to linearly interpolate to the surface?

Response: We appreciate your thoughtful review. Since this work builds on our previous studies (Yin et al., 2021; Jing et al., 2024), we consider it more appropriate to maintain the same data processing approach for consistency. A similar method was also adopted by Baars et al. (2017), who assumed height-independent extinction below the altitude with a complete field-of-view.

Linearly interpolating the extinction profile to the surface would introduce outliers with unrealistically large or even negative extinction coefficients. Thus, both approaches inevitably induce uncertainties in AOD. Besides, as there are only 10 vertical bins within the lowest 0.35 km , their influence on the column-integrated AOD ($0\text{-}7 \text{ km}$) is limited, with an associated uncertainty of <0.05 . Additionally, this study focuses on the long-term evolution of AOD based on large datasets, and the extrapolation method applied to the low-altitude with incomplete FOV does not affect the main conclusions of this work.

L100 suggest to change policy to protocol

Response: “policies” has been modified to “protocols”.

L174 attributed

Response: “attributing” has been modified to “attributed”.

L190 Figure 2a caption “fewer than 15 cloud-free profiles being recorded in a given month” why this restriction to 15 profiles? It is not easy to see in the Figure whether the extinction profiles are monthly averaged. Or is that because you use monthly mean data in Fig b?

Response: “...the particle extinction coefficient...” in Figure 2 caption has been revised to “...the monthly mean particle extinction coefficient...”.

Here, the pie plot of the number of profiles in each month during 2010-2024 is shown in below. Months with <15 profiles account for only 3% of the total 155 months with valid data. Therefore, if the number of profiles in a certain month is less than 15, it indicates that the statistical results for that month are not representative.

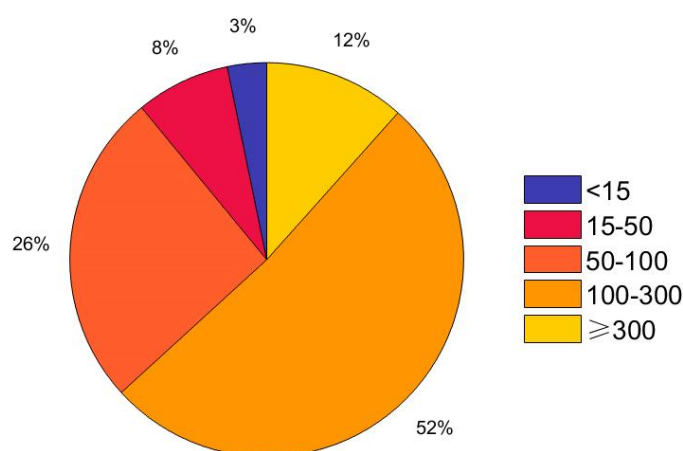


Figure 5R. Distribution of valid aerosol profile numbers in each month.

L 222 Fig 3 caption: change to “DOD for monthly mean” and “The dark orange line”.

Response: “The long-term evolution of” has been deleted, and “Dark orange line” has been modified to “The dark orange line”. The same content in Figures 3 and 4 has also been modified.

L235 “In Wuhan, the non-dust component is primarily attributed to anthropogenic aerosols”. In the case studies you show the effect of biomass burning aerosol (anthropogenic). Attributed to straw burning. To your knowledge, are there wildfires in the area contributing to the aerosol content, i.e. which are not anthropogenic?

Response: There are three major types of biomass burning: forest fire, agricultural straw open burning, and fuel combustion (Chen et al., 2017). Among these, forest fires can generate natural wildfire smoke. According to the study of forest fire characteristics in China by Ying et al. (2018), forest fires in Hubei and surrounding provinces (roughly marked by a blue circle) are mainly caused by recreational activities, such as campfires, smoking, or negligent burning by tourists and residents. Forest fires from industrial sources (e.g., forestry or agricultural activities) and cultural practices (e.g., fireworks and firecrackers) play a secondary role, while lightning-induced fires are essentially absent. Therefore, the non-dust component in Wuhan can be primarily attributed to anthropogenic aerosols. The corresponding statements in the manuscript have been revised as follows. “**In Hubei and surrounding provinces, wildfires are primarily caused by campfires, smoking, negligent burning by tourists and residents, forestry and agricultural activities, and fireworks (Ying et al., 2018). Natural source fires, such as those induced by lightning are negligible. Therefore, the non-dust component in Wuhan can be primarily attributed to anthropogenic sources.**” (please see lines 270-272)

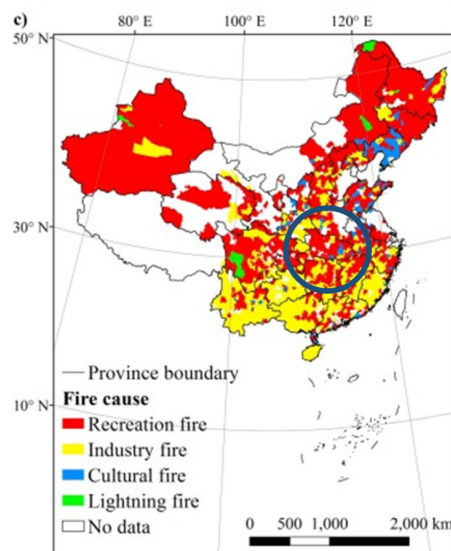


Figure 6R. Patterns of fire cause at the county level in China (Ying et al., 2018).

L237 notably attributed

Response: “a notable” has been modified to “notably attributed”.

L240-245 The ABL develops throughout the day and aerosols are usually mixed throughout the ABL, with a gradient across the inversion at the top of the BL. Were the BLH be determine from the individual lidar profiles? If not, how was BLH determined?

Response: The BLH data were obtained from ERA5 reanalysis, derived from the ECMWF Integrated Forecasting System's turbulent diffusion and turbulent orographic form drag schemes. Specifically, the bulk Richardson number (R_i) method was used to calculate BLH, where R_i represents the ratio of turbulence generated by buoyancy to that generated by mechanical shear (Seidel et al., 2012). The boundary layer height is defined as the lowest level at which the bulk Richardson number reaches the critical threshold of 0.25 (ECMWF, 2017).

L261 Fig 4 caption: “as in Figure 2”

Response: “to” has been modified to “as in”. The same content in Figures 3 and 6 has also been modified.

L 267 & 270 concentrations

Response: “concentration” has been modified to “concentrations”.

L 273 effectiveness of emission control measures?

Response: “the effectiveness of control” has been modified to “the effectiveness of emission control measures”.

L281 Profiles are shown in Fig. 6a

Response: The original sentence is a general summary of Figure 6, not only profiles, but also the monthly variation of BL and FT non-dust AOD.

L288 combusted or combustion, change to burned or burning (here and all other occurrences)

Response: “combusted” and “combustion” have been modified to “burned” and “burning”.

L307 Fig. 6 caption: seasonally averaged; the same as in Figure;

Response: Three places of “seasonal average” have been modified to “seasonally averaged”.

L325 UVAI is zero and indicates the presence of UV-absorbing aerosols: is that a typo?

Response: “presence” has been modified to “absence”.

L406 fluctuated with a rate of 0.002 /yr. That would mean that AOD increased with that rate and varied around that line by +/- 0.2

Response: “the non-dust AOD fluctuated with a rate of 0.002 yr⁻¹” has been modified to “the non-dust AOD showed a fluctuating trend”.

L416 “plenty of aerosols”, do you mean “much aerosol” or “high aerosol concentrations?”

Response: “plenty of aerosols” has been modified to “high aerosol concentrations”.

L418&374 AOD increase 6.1 times, do you mean “increased by a factor of 6.1?”

Response: “the AOD increased 6.1 times” has been modified to “the AOD increased by a factor of 6.1”.

L421 variations

Response: “variation” has been modified to “variations”.

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