We really appreciate the reviewers for the valuable and constructive comments, which are very useful for the improvement of the manuscript. We have replied the reviewers’ comments point-to-point in below. The reviewers’ comments are cited in black, while the responses are in blue. The revised parts in the manuscript are marked in red. All the page number and line number are referred to the revised manuscript.

**Major issue**

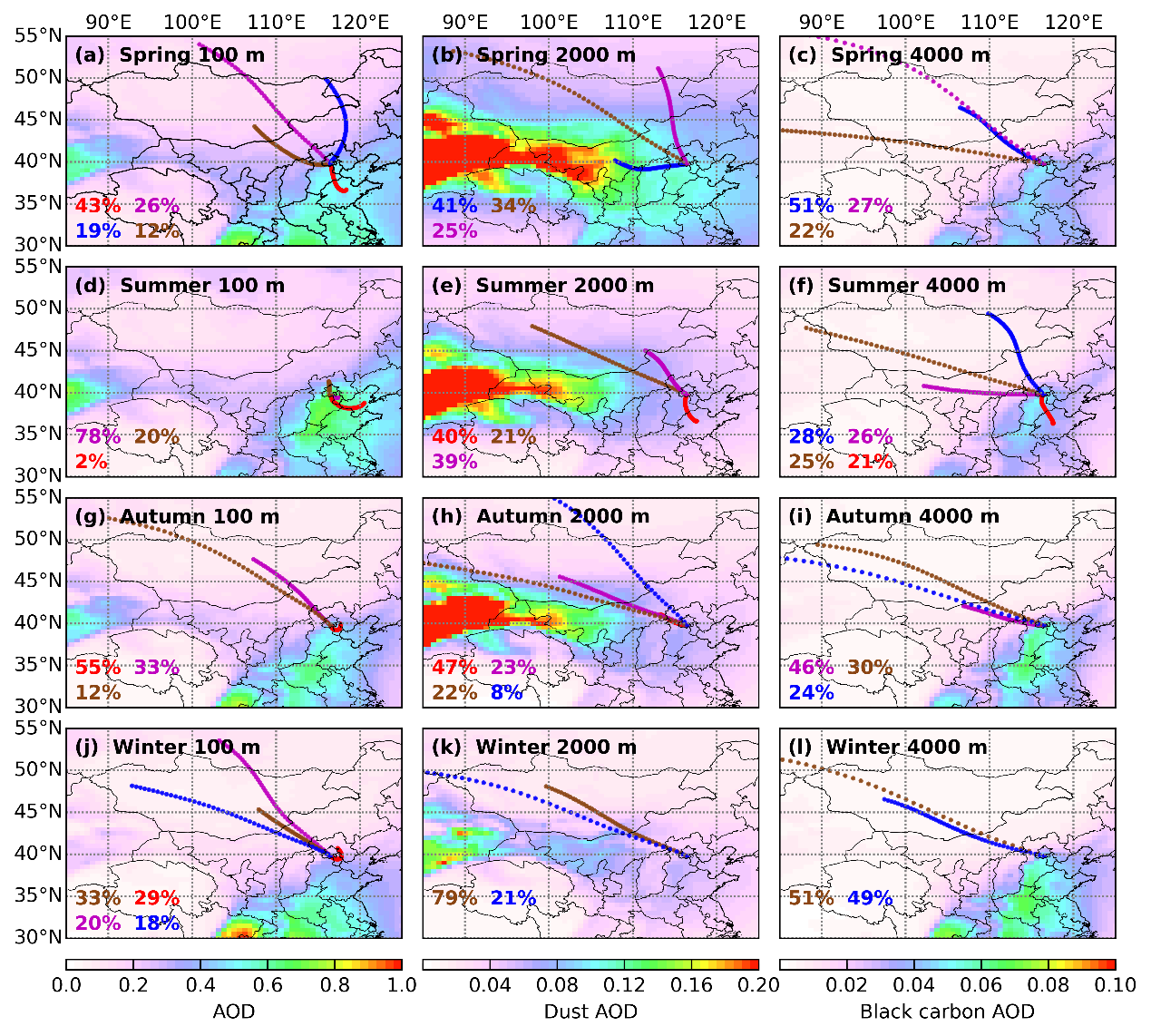
(1) The authors speculate too much in the data analysis. For example, in page 18, line 10-12, "The high anthropogenic aerosols mass concentration in the upper air (0.4.0.9 km) over Beijing in summer is mainly caused by the growth of particle hygroscopicity under the influence of southern transport.", this statement cannot be supported by the results presented in the manuscript. As the authors described, all profiles with high relative humidity (> 85%) have been ruled out from the data analysis (see page 6, line 20-21). Then, hygroscopic growth should not be significant. And more importantly, no results of hygroscopic growth factors were shown, how this conclusion can be made? Similar issue is also laid in the analysis of MBL height and aerosol mass concentration (There is a minor issue associated with this). I hope the authors can focus on their own results and start from these results to re-think what they can conclude.

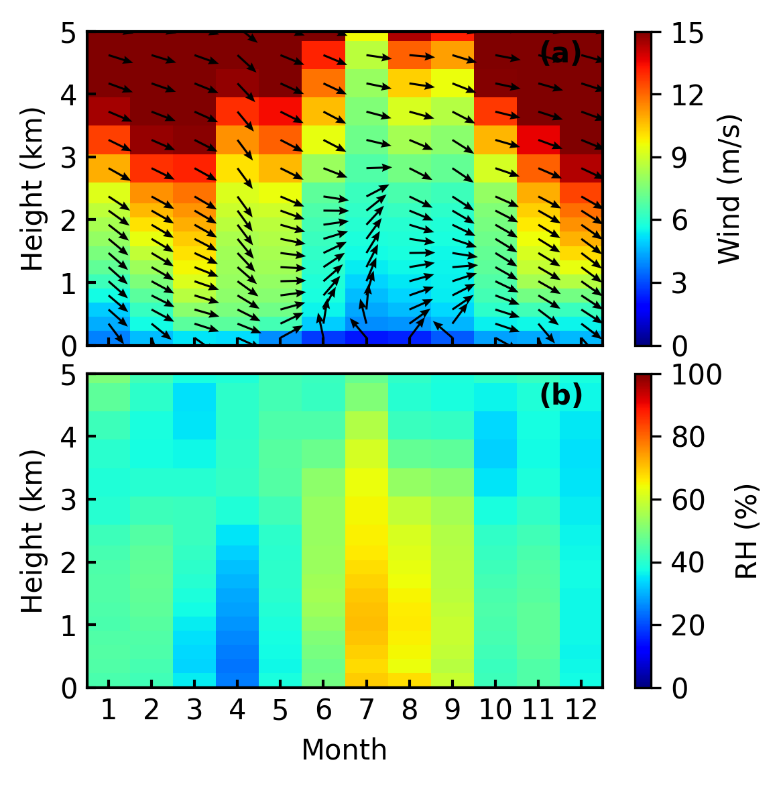
R: Thank you for your comment, which is very meaningful and valuable in improving the quality of our manuscripts. Due to the lack of measurement of the hygroscopic growth factors at the top of the mixing layer, we have no direct evidence to prove the effect of hygroscopic growth on upper anthropogenic aerosols. However, during our observation period, there are frequent southern transport scenarios in the upper air over Beijing in summer, with weak southerly winds, high relative humidity levels, and weak wind speed (Figure R1, Figure R2), the meteorological conditions are conducive to the hygroscopic growth of particles based on previous researches (Tang et al., 2015; Tang et al., 2016). Due to the limitations of the POLIPHON method, we excluded cases with relative humidity greater than 85%. However, previous studies revealed that the hygroscopic growth of particles also occurs when the relative humidity is between 40% and 86% (Wu et al., 2020; Xia et al., 2019). The hygroscopic growth of particles at the top of the mixing layer was inferred based on previous findings and air pollution characteristics during our observation period.

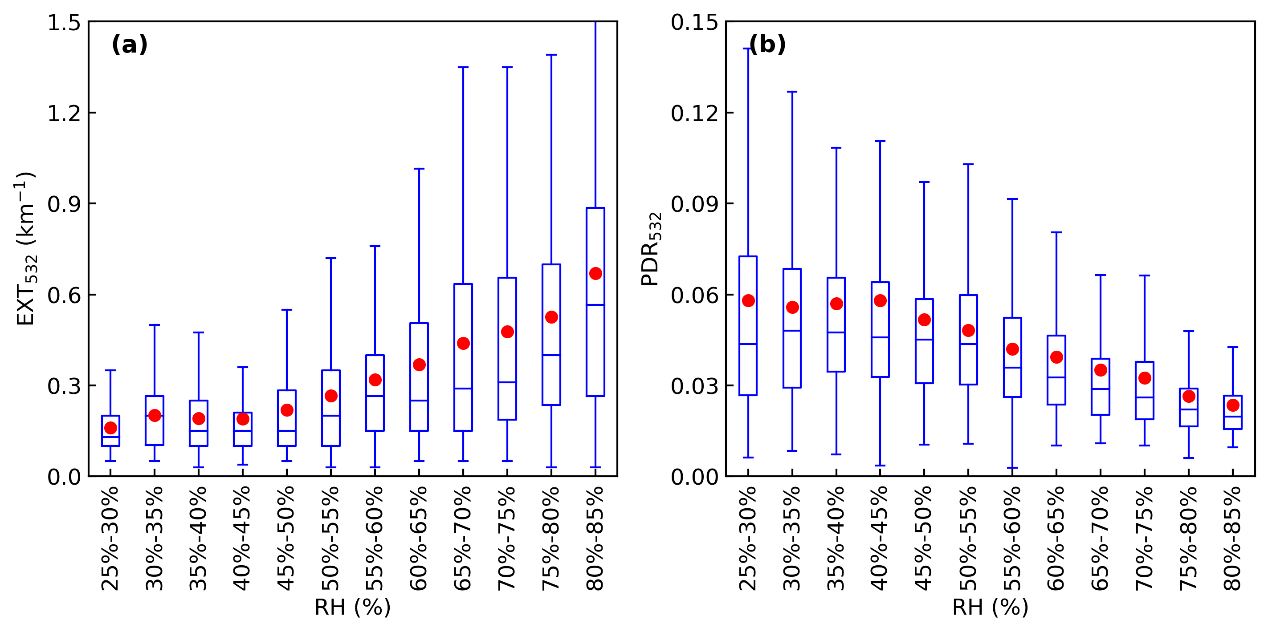
We further analyzed the relationship between the relative humidity and EXT532 and PDR532 at the top of mixing layer in summer (Figure R3). When the relative humidity was greater than 40%, the EXT532 increased with the increase of relative humidity, that is, the air pollution increased with the increase of humidity. More importantly, with the increase of relative humidity, the PDR532 gradually decreases, indicating that there is hygroscopic growth of particles at the top of mixing layer (Dawson et al., 2020). Because the PDR532 is related to hygroscopicity, it is inversely proportional to the sphericity of atmospheric particles. Assuming a uniform refractive index, moistened aerosol particles are thought to be more spherical due to condensation of water vapor and surface tension, resulting in a lower PDR532. Thus, there is hygroscopic growth of particles at the top of mixing layer, but the contribution of hygroscopic growth to anthropogenic aerosols mass concentration at the top of mixing layer is still uncertain.

To sum up, we corrected our statement in the manuscript: “The high anthropogenic aerosols mass concentration in the upper air (0.4-0.9 km) over Beijing in summer is mainly caused by the southward transport in the upper air, where the atmosphere is relatively stable and moist, favoring hygroscopic growth of particles.” This is also a limitation of our observations, which we highlight at the conclusion of the manuscript.

We also include these important results in the manuscript. Please refer to Page 13 Line 22–29, Page 17 Line 29–31, Page 18 Line 29–30, Page 19 Line 16–26, and Figure 9 in the manuscript.

*******Figure R1****. Cluster analysis of seasonal* *48–hour air mass backward trajectories in Beijing from May 2019 to February 2022: initialized at (a) 100 m, (b) 2 km, and (c) 4 km in spring, initialized at (d) 100 m, (e) 2 km, and (f) 4 km in summer, initialized at (g) 100 m, (h) 2 km, and (i) 4 km in autumn, and initialized at (j) 100 m, (k) 2 km, and (l) 4 km in winter. We calculated the hourly air mass backward trajectories during each season. Then, cluster analysis was carried out in 2–4 categories directions. The percentages at the bottom right of each subplot indicate the percentage of each backward trajectory. The color on each subplot indicates the AOD, dust AOD, and black carbon AOD for each season obtained from the MERRA‒2 global reanalysis data.*

***Figure R2****. Vertical profiles of the monthly mean (****a****) wind speed and direction, (****b****) RH* *obtained from the ERA5 reanalysis data from May 2019 to February 2022. The black arrow in (****a****) shows the wind direction, and the upward indicates the south wind.*



***Figure R3****. The box and whisker plots of the relationship between the (a) RH and EXT532, and (b) RH and PDR532 at the top of mixing layer in summer. The box and whisker plots showing the 5th, 25th, 50th, 75th, and 95th percentiles, the red dots represent the mean values.*

**Minor issues**

(1) Page 5, line 8, the authors should be specific that the gradient of which quantity they used in the MBL height determination. And how do they treat the very shallow nonctual boundary layer height within the incomplete overlap region?

R: Thanks for your comments. The mixing layer height (MLH) was retrieved using the gradient method (Sicard et al., 2006; Flamant et al., 1997), which is the most classic and widely used method, gives the MLH as the altitude of the minimum gradient of the range-squared-corrected signal:

 (1)

*P(R)* represent the backscatter signal collected by telescope from range *R*. The retrieve range of the gradient method is 0.25‒4 km, so the minimum MLH is 0.25 km and the maximum MLH is 4 km. Therefore, we cannot capture the very low MLH, especially at night, which may lead to overestimation of the MLH in Beijing. This is also one of the shortcomings of PRL retrieval of MLH, and we also include these statements in the manuscript and highlight this shortcoming in conclusion. Please refer to Page 5 Line 16–20 and Page 19 Line 16–26 in the manuscript.

(2) Page 6, line 21-22, the authors need to explain why they intended to use lidar-derived MBL height in MBL AOD calculation instead of using ERA-5 MBL height, although they think ERA-5 height is reliable and can be used to evaluate the lidar-derived MBL height.

R: The nighttime boundary layer height (BLH) of ERA5 is extremely low, only tens of meters, while the lowest detection height of PRL is 0.25 km. If the BLH of ERA5 is used, a large amount of data at night will not be available when discussing the BLH and anthropogenic aerosols/dust mass concentration, so the PRL retrieved MLH was employed. We have also added these notes to the manuscript. Please refer to Page 10 Line 10–12.

(3) Page 7, line 24, "Bac" -> "BAC"

R: Thanks for pointing out the unsuitable expression. We have corrected the mistake accordingly. Please refer to Page 7 Line 28, Page 7 Line 30, and Page 8 Line 3.

(4) Page 9, line 17, why do the authors think "it suggests a strong sytematic coupliing between ML and FT, ...", instead of they are both modulated by the same mechanism, like regional transport of aerosols.

R: Thanks for pointing out the unsuitable expression. It is possible that they are both modulated by the same mechanism, which was overlooked in our previous analysis, and we have corrected our statement: “They may be regulated by the same mechanisms, such as regional transport of aerosols.”. Please refer to Page 9 Line 28–29 and Page 18 Line 26.

(5) Page 10, line 25, PDR at 532 should be at percentage, namely, 0.082, or adding a percent sign (%) instead. And the authors need to check the manuscript thoroughly, because there are many places with this error.

R: Thank you for your comments, we have corrected the relevant mistakes and carefully checked the manuscript for similar errors.

(6) Page 11, line 33, "building warming" -> "building heating".

R: Thanks for pointing out the unsuitable expression. We have followed this suggestion and corrected it accordingly. Please refer to Page 12 Line 13.

(7) Page 11, line 33, "MEGGA" -> "MERRA". (also in caption of fig. 8)

R: We have followed this suggestion and corrected the mistake accordingly. Please refer to Page 12 Line 13.

(8) Page 13, line 17, "upper air pollution transport" is more appropriate.

R: Thanks for pointing out the unsuitable expression. We have followed this suggestion and corrected it accordingly. Please refer to Page 14 Line 5.

(9) Page 15, line 22, "bottom" should be removed.

R: Thanks for pointing out the unsuitable expression. We have followed this suggestion and corrected it accordingly.

(10) Page 15, line 29-32, correlation of coefficient cannot be used to determine the goodness of fit for non-linear models. Therefore, it cannot be compared between linear fitting and non-linear fitting, just by looking at correlation of coefficient. The author should either use a different metric to do the comparison or remove such statement.

R: Thanks for pointing out the unsuitable expression. We have removed these statements in the manuscript.

(11) Page 17, line 14-15, the authors should be specific when mentioning "near the ML" or "around ML" (the same page, line 18).

R: Thank you for your comments, here we discussed the relationship between PRL derived MLH and vertical profiles of anthropogenic aerosols and dust. The high dust mass concentration usually distributed near the ML (1.4-3.4 km) during the whole observation period, and high dust mass concentration usually distributed near the ML (1.6-3.4 km) in spring. We have specified the “near the ML” and “around ML” in the manuscript. Please refer to Page 18 Line 3–7.

(12) Page 18, line 20-21, the authors should clarify why "the bottom dust mass concentration is mainly influenced by transport" instead of by local sources.

R: Thank you for your comments. Our observation site is located in the urban area of Beijing, where soil dust, construction dust, coal dust and motor vehicle exhaust are the four main sources of dust in Beijing (Wang et al., 2015), accounting for 38.50%, 22.25%, 14.06%, and 20.82% of the total amount of dust, respectively. The bottom dust mass concentration discussed in the manuscript is located at 0.25 km, while soil dust, construction dust, coal dust and motor vehicle exhaust usually concentrated within tens of meters (Noh et al., 2021). Thus, the bottom dust mass concentration is mainly influenced by transport, and we have added these statements and specified “bottom (0.25 km)” in the manuscript. Please refer to Page 17 Line 2–4 and Page 19 Line 9.

(13) In caption of fig 2, Check about the conversion factors of dust and anthropogenic aerosols. It's too low for dust and a little high for anthropogenic aerosols (see ref.[1-2]).

R: Thanks for your suggestion, we also found this phenomenon when retrieving the dust and anthropogenic aerosol mass concentrations. We have carefully checked the sun‒photometer derived aerosol optical parameters and found no problem. This phenomenon is very interesting, and it is worth further exploring why the conversion factors of dust in Beijing is low, and the conversion factors of anthropogenic aerosol aerosols is high.

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