Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1162-RC1, 2020


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Study on variations in lidar ratios for Shanghai based on Raman lidar" *by* Tongqiang Liu et al.

**Anonymous Referee #1**

Received and published: 23 December 2020

The aerosol types are very important information in assessing the impacts of aerosols on climate forcing. Accurate measurements of Lidar ratios (LR) and better understanding of their variation characteristics provide a way from remote sensing for the scientific research on aerosol type information. This study addresses the results of a long-term observation of LR at 355 nm of Raman lidar in Shanghai from 2017 to 2019, and analyzed their variations and influencing factors. This kind of observations about LR are rare and worth encouraging, especially over Eastern Asia region. I recommend a minor revision before publication with ACP. The detail comments or suggestions are shown below.

My general concern about this study is that although the data results are rare and good, Some of the analysis on the reasons for LR vertical and temporal variations and



aerosol source influences are not straightforward. Several inferences and methods in part 3 seems too complicated and sometimes confused. First, it is difficult to conclude aerosol type according to the comparison of LR data with some literature reports in table 1 of the manuscript. Second, the results from the usage of cluster analysis of back trajectories in 3.2.2 are not significant without a statistical test. In fact, the back-trajectory analysis should be effective for long-range transport dominated aerosols. Over an urban area of Shanghai, I suppose the status of aerosols from long-time averaged perhaps are mainly dominated by local urban emissions. Dust fraction in the aerosols should be a main factor to affect the LR data and volume depolarization ratios ($\delta$), sometimes absorbing aerosols from primary aerosols may also cause an increase of LR in the surface layer. I suggest a study to check the dust fraction on LR and $\delta$ only with the surface PM2.5/PM10 data. The PM2.5/PM10 data should be easy to obtain over urban area of Shanghai. If the authors have more chemical composition observations, for examples, EC, that will be better.

And, I have some minor and technical comments for the authors to address:

1, the English of the paper should be improved, for examples, some definite articles 'the' be misused.

2, Line 43, "The P($\pi$) was related to sphericity of the particle which can be obtained from the polarization lidar", can it be obtained from the polarization lidar?

3, Line 46, "Moreover, the heating effect of the absorbing aerosol on the atmosphere results in an increase of atmospheric stability and a reduction of atmospheric vertical exchange, which further aggravates the accumulation of pollutants (absorbing particles) and a positive feedback is established". Whether this conclusion is right, I think it is depending on a suitable vertical distribution of the absorbing aerosol. If the absorbing aerosols are on near surface layer their heating effect will enhance the instability of the atmospheric boundary layer.

4, Line 92, "the Raman signals are very weak in the daytime", are the Raman signals stronger in the night time than in the daytime, I think they are same, but signal-to-noise ratios are different at daytime and nighttime.

5, Line 111-115, and 338-340, RH data from a model simulation or reanalysis data are not credible. I do not think this analysis is useful to the study.

6, In 3.1.1 General variation of LR. Some comparisons with those values in table 1, then conclude the aerosol type, I do not agree with this method. Dust fraction, or fine-mode fraction, maybe is more direct to help the analysis of the aerosol type.

7, Line 172, "Above 2km, the mean values of LR were usually less than 40 sr, which alluded to an increasing influence of background aerosol (i.e. less absorbing coarse-mode particle)", I think "coarse-mode particle" is not accurate. The coarse-mode particle will lead an increase of LR. Fine-mode secondary aerosols are dominated in long-range transport process over an urban area.

8, Results and analysis related to Figure 4, I do not think the current observation period of the data support a seasonal or monthly/annual change analysis. The observation period presented by Fig.1 only include some individual months over the 3 year respectively, it is not reasonable to combine the individual months from different years to an annual or seasonal change. The authors need longer and more continuous data.

9, Figure 5, I suggest date labels for the x-axis instead of sequence numbers.

10, Analysis related to Figure 7 with back trajectory cluster analysis, I do not think it is significant.

11, Analysis related to Figure 8 & 9, why not to check by some surface observations instead of only using AOD, for examples, PM2.5 mass concentration?

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