

Interactive comment on “Measurement of aerosol properties during wintertime in Beijing” by M. L. Zamora et al.

Anonymous Referee #1

Received and published: 25 June 2019

The authors analyzed the aerosol properties in Beijing via a wintertime field campaign conducted in 2015, aiming to better elucidate the inherent haze formation mechanisms. In general, the paper is well written. However, some concluding remarks claimed by the authors are slightly overstated and many more details should be provided to enhance the readability of this paper.

Below are several specific comments for the authors reference.

Specific comments:

Line 7 of page 2: reference for the citation of Wang et al. (2016) was not provided in the reference list, please add.

Line 10 of page 2: the following references can be also added: Li, Z., Guo, J., Ding,

A., Liao, H., Liu, J., Sun, Y., Wang, T., Xue, H., Zhang, H., Zhu, B., 2017. Aerosol and boundary-layer interactions and impact on air quality. *Natl. Sci. Rev.* 4, 810–833. <https://doi.org/10.1093/nsr/nwx117> Miao, Y., Liu, S., Guo, J., Huang, S., Yan, Y., Lou, M., 2018. Unraveling the relationships between boundary layer height and PM_{2.5} pollution in China based on four-year radiosonde measurements. *Environ. Pollut.* 243, 1186–1195. <https://doi.org/10.1016/j.envpol.2018.09.070>

Line 29 of page 2: “. . .to better understand the haze formation mechanisms in different seasons” What do w and d stand for in equation 2? Please detail their meanings in the following main text to avoid readership gap.

Line 18-19 of page 4: How about the accuracy of the derived PBL from the HYSPLIT model? Why did not the authors use the radiosonde measured profile to retrieve PBL?

Line 30 of page 4: the full name of NPF should be provided here when the acronym comes out for the first time. “. . .with an new particle formation (NPF) event, . . .”

Line 1 of page 5: wind speed was not depicted in Fig. S1, please add a diagram showing the wind speed at the sampling site during the study period to support your claim.

Line 3 of page 5: is there any explanation for the observed data gaps in the total number concentration time series as shown in Fig. 1C?

Line 9–16 of page 5: the synoptic weather pattern should be provided as well to ease the readership.

Line 20-22 of page 6: is there any reference to support your claims? If so, please add the relevant references.

Line 33 of page 6: please clarify why stronger solar irradiation would result in larger particle diameter.

Line 1–3 of page 7: did the author meant to say that higher mixing layer would renders

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the particle growth and secondary aerosol formation more efficient? Any explanation?

Line 22–23 of page 7: “We show that the periodic cycles of haze episodes during the autumn and winter seasons in Beijing are closely linked to the meteorological conditions”. This claim might be not fully supported by the current results as shown in Fig. S1. It shows that the mixing layer height after September 26 varied with small deviations for the subsequent days, which suggests that meteorological condition is not the key factor in modulating the PM concentrations during this time period and thus the observed haze events should be attributed to other reasons.

Lines 26–29 of page 7: “Our results imply that an effort to solely control emissions of primary particles would result in only a minor reduction of the PM_{2.5} mass concentration, while the reductions in the emissions of the aerosol precursor gases, i.e., VOCs and NOX from local transportation and SO₂ from regional industrial sources, are critical for remediation of the haze pollution in Beijing”. The results in the current study may be inadequate to support such a pollution control strategy and more relevant results are required to bridge the gap.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-308>, 2019.

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