

Interactive comment on “Haze pollution in winter and summer in Zibo, a heavily industrialized city neighboring the Jin-Jin-Ji area of China: source, formation, and implications” by Hui Li et al.

Anonymous Referee #3

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Li et al.,

In this manuscript, the author presents two field campaigns in Zibo, Shandong (China) during summer and wintertime (Jan 15-25, 2015 and July 14-31, respectively), and a series measurements were conducted including PM_{2.5}, sulfate, nitrate, OC, EC, as well as gases NO_x, SO₂, CO, and O₃; the authors also provided information on wind speed, wind direction, pressure, temperature and relative humidity. The aim of the authors listed in Introduction was to characterize PM_{2.5} including chemical composition, diurnal process formation, as well as regional contribution. However, in the title part, the authors promised the characterization of haze pollution, which should have focused

C1

on chemical composition of PM_{2.5} and the corresponding light properties. The authors also attempted to investigate seasonal variation during summer and wintertime, but two weeks (at most) observations were inadequate. Through the text, the authors tried to illustrate the effect of mixed layer height, photochemical activity, and relative humidity. Unfortunately, the conclusion of each part was not clear enough and somehow already well-known. The way of discussion in Section 3.3, and 3.4 were unacceptable. Hereby, the reviewer would suggest the manuscript be declined for publishing in a journal like ACP. The manuscript should be improved and submitted to a journal for air pollution characterization.

Major comments: 1. According to the content of the text, Title part should be modified to talk about the chemical composition, diurnal profiles, and formation instead of “haze pollution”. In addition, the discussion of “source” of the haze was very weak.

2. Abstract is lack of information. The audience would only know the average PM_{2.5} concentration during the summer and winter observation. No novel information could be obtained in the following part because the behaviors of SIA formation could be explained by known mechanisms.

3. Introduction. The summary of current understanding of air pollution and formation mechanism was insufficient, no scientific question was proposed, no hypothesis was drawn, and why the work should be done was unclear. The authors should at least give the information that why Zibo is important, and what scientific question(s) could be solved through this work.

4. Methodology. Description of quality control/ quality assurance could not be found in this part. The comparison between the online and off-line result would be interesting but they were not provided. Thus the reviewer could not determine the reliability and quality of data presented.

5. Section 3.1. Much information was provided in this part. However, it was no more than a report of local air quality.

C2

6. Section 3.2. The diurnal behaviors of major pollutants are interesting. However, the description and explanation in this part were too general and ambiguous. Indeed, well-known that mixed layer height would affect concentration of pollutants. The reviewer would suggest a study on the diurnal pattern of a ratio of a pollutant over an inactive primary pollutant, e.g. sulfate/EC for a clearer understanding the atmospheric process of pollutants. Moreover, there were numerous studies using aerodyne AMS on NCP, the referee would recommend comparisons between this work and previous studies.

7. 3.3 Sulfate. The referee does not understand the logic of this part. It is NOT surprising AT ALL for a weak correlation between EC and sulfate because one is primary and the other is mainly secondary. The effect of atmospheric dilution due to the shift of boundary layer height could be excluded only when a ratio of pollutant/EC (e.g. sulfate/EC) is adopted. A correlation analysis between sulfate and EC is not meaningful. Line 317-318 is not clear and lack of evidence.

8. 3.4 Nitrate. It is obscure and arbitrary to only use a ratio of $[\text{NH}_4^+]/[\text{SO}_4^{2-}] = 1.5$ to define an "Excess NH_4^+ ". Moreover, an "Excess NH_4^+ " is actually from the excess of NH_3 which can turn into particulate NH_4^+ , as a result, NOT a cause. It would go without saying that HNO_3 and NH_3 reacting in the gas phase and the subsequent partitioning is the major source of secondary NH_4NO_3 in the particle phase. Figure 7. Why don't you provide a NOR ratio against RH or temperature here? It would be clearer and more straightforward.

9. Section 3.5. Nothing special is drawn in this part.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2018-83/acp-2018-83-RC3-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-83>, 2018.