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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Summertime and wintertime atmospheric processes of secondary aerosol in Beijing" *by* Jing Duan et al.

Anonymous Referee #2

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This manuscript reported the composition of submicron aerosol measured by an aerosol chemical speciation monitor (ACSM) in summer and winter in Beijing. PMF analysis is performed for the sources apportionment of organic aerosol. Correlation analysis is used to investigate the formation mechanisms of nitrate, sulfate, and organic aerosol. The same measurements and data analysis have been repeatedly performed in Beijing as well as other megacities in China. This manuscript lacks novelty and in-depth discussions. Most of the conclusions are speculative. I am sorry that I cannot recommend publication in its current format.

Major Comments

1. Page 6 Line 20-23. It is stated that the higher COA concentration in winter than summer suggests enhanced primary emission during winter. However, the changes

are more likely caused by meteorological conditions than emissions, as the cooking activities are not expected to have seasonal variation.

2. Page 6 Line 24-26. In order to evaluate the effects of biomass burning control on air quality, the change in absolute BBOA concentration needs to be used, instead of the fraction of BBOA in total OA.

3. Page 6 Line 33-34. It is a bit surprising that two OOA factors are resolved in winter, but only one OOA factor is resolved in summer. This is different from the observation in Jimenez et al. 2009 Science and many others studies containing summer vs winter contrast. This doesn't necessarily mean that the PMF results in this study are wrong, but it would be interesting to understand why. The instrument resolution is not the only reason, as it doesn't change between seasons.

4. Page 8 Line 12-13. It is an interesting finding that "daytime oxidation formation efficiency of secondary aerosol during winter was comparable to that during summer", but the reason for this observation warrants in-depth analysis. This is just one of many examples that can substantially improve the manuscript.

5. Section 3.3. The rationale behind the scatter plot between NO3 and SO4 is not clear. NO3 and SO4 originates from different precursors, but this difference is ignored in the scatter plot. The conclusions from this analysis are also highly speculative. For example, the larger slope of NO3/SO4 is attributed to that high RH facilitates the gas/particle partitioning of ammonium nitrate. However, wouldn't the high RH (or LWC to be more precise) have similar effect on ammonium sulfate? The formation of nitrate depends on many factors, including NOx, OH, temperature, RH (potentially), etc. The authors need to express more caution in interpreting the results from the simple correlation plots. The conclusions such as RH enhanced nitrate formation in summer and photochemical process dominating nitrate formation in winter are not well supported.

6. Page 9 Line 17-18. It is written that "This suggests that RH has either no effects or complex effects on the OOA formation." This statement only illustrates that the previous

analysis is not meaningful.

7. Section 3.4. Similar to the discussions on the formation mechanism of sulfate and nitrate, the effects of OA precursors are completely omitted in the discussion. Temperature is another important factor when contrasting the OA between summer and winter, but it is also ignored in the discussion.

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