Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-675-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Formation mechanism and source apportionment of water-soluble organic carbon in PM_1, $PM_{2.5}$ and PM_{10} in Beijing during haze episodes" *by* Qing Yu et al.

Anonymous Referee #1

Received and published: 21 October 2018

This paper investigated the size distribution and sources of WSOC in Beijing based on the offline PM1, PM2.5 and PM10 samples collected during haze episodes in winter and early spring of 2017. Many traditional chemical analysis are conducted in this study, which looks a lot of work. However, this study disadvantaged in the few sample numbers, the lack of in-depth data analysis or convincing data interpretation. This study may meet the minimum quality standard of ACP paper if all the following concerns are well addressed.

Major concerns:

1. With only ~10 days measurement in each season, it was too board to attribute them

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Discussion paper

to “seasonal variations”. The authors may want to limit the topic to two haze episodes in winter and spring. Or alternatively, the authors should state why they think these episodes are representative for winter/spring.

2. The PMF analysis require a lot of samples, usually at least 90. With the sample number shown in this study, any PMF result cannot be within the uncertainty range. The authors should avoid using good methods inappropriately.

3. It’s surprising that the authors discussed about absolute tracer concentrations all the time. In winter and spring, the different boundary layer height and thus dilution effect could already result in the different pollutant accumulation rates, and thus much concentration differences. That is, even with the same emission rates, the primary tracers should also be higher in winter than in spring. The authors should consider scale primary and secondary tracers respectively to give a reasonable comparison.

4. In estimating the SOC through OC-EC methods, the primary OC/EC ratio should be estimated separately for each season, consider the seasonal variation in one major source of heating.

Minor suggestions:

1. Abstract and throughout: the term “diurnal samples” is strange. Consider change into “day/night sample pairs” or other clearer words.

2. Abstract and throughout: define “finer” and “coarser” modes clearly as PM1, PM1-2.5 or PM2.5-10.

3. Line 170: The difference between the two monoterpene SOA tracers, 3-hydroxyglutaric acid and cis-pinonic acid, should be clarified here.

4. Line 150 $\sim$ 152: The drop in WSOC/OC may not be due to the so-called “wind / rain cleansing”, but is simply due to the change is air mass origins. The authors should investigate into the air mass origins during these days, and separate the influence of transport and chemistry.

5. Line 152 to 155: due to similar reasons above, higher WSOC/OC ratio during the haze episodes cannot represent the enhanced secondary formation, especially when the detailed POC/SOC contribution of WSOC is not known here. Does the SOC/OC ratio increase in haze episodes? A SOC/OC ratio versus PM1 / PM2.5 / PM10 concentrations can help better illustrate these issues.

6. Fig. 1 & Fig. 6: There should be gaps in lines when the dates are not continuous. In Fig. 6, plotting the corresponding PM concentrations together can also help make this figure clearer.

7. Fig. 2: use boxplots instead of averages is more proper here.

8. Fig. 3: Does the authors mean that only cis-pinonic acid is related to gas-phase oxidation, while all other tracers are attributed to heterogeneous reactions? Are there any references to support these findings? Also, does the tracers arranged according to the contribution of heterogeneous / gas-phase oxidation? If so, is the 3-hydroxyglutaric acid contributed by heterogeneous and gas-phase oxidation half and half? Please be more accurate in presenting figures.

9. Fig. 4 to 5 and relevant discussions: please double check whether one can get satisfactory PMF result from so few samples.

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