Review of “Primary and secondary organic aerosols in 2016 summer of Beijing” by Tang et al.

This manuscript quantified 144 particulate organic species and applied chemical mass balance (CMB) model to investigate the sources of organic aerosol at two different sites (CP and PKUERS) in Beijing. The authors found that the primary sources accounted for 42.6% and 50.4% of the measured OC at CP and PKUERS, respectively, which are larger than the contribution from secondary sources. Among the secondary sources, anthropogenic VOCs contributes more to SOA than biogenic VOCs. By comparing with previous studies, the authors showed that the PM and EC concentrations have decreased. The OC concentration from many sources have decreased, with the exception of OC from gasoline engine emissions. This comparison sheds light on the evaluation of regulation policies. At last, the authors investigated the relationship between SOC concentration with temperature, ozone concentration, aerosol water content, and particle acidity. Overall, the manuscript is well-written and the data analysis is solid. I recommend publication after minor revisions to address the main comments below.

Major comments

1. The uncertainty with tracer yield method should be better discussed. In that method, a laboratory-derived single-valued mass fraction from Kleindienst et al. (2007) is used. However, in the atmosphere, the mass fraction of molecular markers in SOA from a specific source is highly dependent on the oxidation conditions and the history of the airmass. How representative is the mass fraction value used in this study?
2. I find it very intriguing that while PM and EC concentrations have decreased from 2008 to 2016, the OC concentration is relatively constant (Table 1). As shown in Fig. 2, the contributions from many sources to OC have decreased, with the exception of vegetative detritus and gasoline engines. By eyeballing, the increases in vegetative detritus and gasoline engines seems smaller than the decreases in other sources. If so, there may be some uncharacterized sources that lead to the relatively flat OC trend. I suggest the authors to further explore the reasons for the relative flat trend of OC.

In Fig. 2, isoprene SOC decreases by 7% from 2008 to 2016. What’s the main cause for this decrease?

Minor comments

1. Line 63. “biogenic” SOC or total SOC accounted for 3.1% of the measured OC?
2. Line 319-323. What is the rationale to compare Beijing with Alaska?
3. Line 324-329. Why is the concentration of biogenic SOA in Beijing is even higher than some forest sites? Higher oxidation capacity in China is one possible reason, but the sources of biogenic VOCs are also critical. Have the authors compared the concentrations or fluxes of biogenic VOCs between Beijing and forest sites?
4. Line 438-440. In Offenberg et al., the sulfate concentration is a confounder. In other words, in Offenberg et al., it is unknown whether the enhancement in SOC is due to higher acidity or higher sulfate concentration or higher particle surface area. Have the authors investigated the relationship between apportioned SOC and sulfate concentration?
5. Fig.4. Why are data separated into day and night in panels (a)-(d), but not in (e)-(h)?