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Atmospheric
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Discussions

Interactive comment on “Quantitative evaluation of emission control of primary and secondary organic aerosol sources during Beijing 2008 Olympics” *by* S. Guo et al.

Anonymous Referee #2

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The paper by Guo et al. examines sources of primary and secondary aerosol in Beijing using measurements from two field sites (urban & rural) and basic source apportionment methods. To look at the impact of control measures they compare their results to previous research and by separating their measurements into sampling periods when different control strategies were enacted in the Beijing area. They conclude that control measures have worked for PM2.5, but secondary organic aerosols now comprise a larger fraction of fine mode aerosol. Clear evidence is presented showing that coal combustion has been effectively controlled.

The paper addresses an important topic as part of the CARE-Beijing campaign and

presents interesting new data. There are several major concerns explained below. Once these are all addressed, I recommend publication.

Major comments:

1) In general, careful revisions by a native English speaker would greatly benefit the quality of this paper. There are numerous grammatical errors (examples: pg. 32884 ln. 11 “of vehicle emission”, ln. 18. “regional” should be “rural”; pg. 32885 ln. 8 “other” should be “rather”, ln. 25 remove “the”; pg. 32886 ln. 17 “precious” should be “previous”). There are also numerous poorly formed or awkward sentences (examples: pg. 32885 ln. 1; pg. 32886, ln. 20-22; pg. 32887 ln. 8, ln. 16, ln. 19; pg. 32889 ln. 3-5). These examples are just a sample; a thorough review of the whole paper should be carefully done.

2) pg. 32888 ln. 12: It is very unlikely that there was no cooking aerosol in Beijing during the study. Cooking aerosol is present in most studies in urban areas and comprises a major fraction of aerosol in Beijing in Wang et al. (2009). The majority of the observed OC at both sites was not apportioned. It is likely that cooking SOA is in that “Other OC” group and is still important for urban and rural OA.

3) Table 3: Why are the SOA sources not included in this table?

4) Table 4: It would be very helpful if the excess non-apportioned OC was shown in this table as well. In line with the comment above, Tables 3 & 4 need to be more consistent. Same with figure 2; are the percentages shown of everything or just the apportioned aerosol?

5) p. 32891 ln. 15: The author uses carbon predominance index (CPI), which is not used in this field and the term is not explained in the references given. I suggest removing this comment. Additionally, It is worth mentioning recent work on primary emissions of VOCs from gasoline and diesel vehicles (P.N.A.S.) may point to the importance of diesel emissions with the high concentrations of C12-C22 alkanes.

6) Sec. 3.4: In general there are too many numbers in the text that are either shown in Table 4 or could be shown graphically with greater efficiency. Reducing the incidence of numbers will greatly improve readability.

7) pg. 32897 ln. 19: As the authors say earlier on the page, SOA formation is very complicated. Due to the interconnectivity of anthropogenic/biogenic oxidation and SOA formation, simply comparing the ratio of anthropogenic to biogenic SOA is insufficient to assess the effect of control measures. The ballpark estimate does not consider the impact of differences in temperatures over this period (a driver of biogenic emissions); changes in NOx emissions (NOx levels affect the rate of oxidation & SOA formation); and the authors have not apportioned the majority of the OA. Whether this "other OC" aerosol is anthropogenic or biogenic could have a major effect on this basic calculation. Furthermore, toluene SOA is just a marker for one type of anthropogenic SOA from toluene and potentially similar aromatics. SOA from alkanes is not considered and cannot be assumed to scale with the toluene SOA marker, especially considering the differences in SOA from gasoline vs. diesel and the variable decrease in emissions from the two vehicle types during the study. So, it is inaccurate to say that the anthropogenic SOA is “constrained” during the control period. More advanced techniques and modeling needs to be done to study this. The authors should consider removing this portion of the paper.

8) Table S2 gives PM (size cut needs to be specified) concentrations for a number of species that will be largely partitioned into the gas phase, if not entirely in the gas phase. These intermediate volatility organic compounds (IVOCs) needs to be better labeled in the table to indicate that the reported concentrations are underestimates and that some of the reported mass may actually be vapors that condensed on the filters during sampling. Examples: naphthalene, methylnaphthalenes, dimethylnaphthalenes, acenaphthylene, acenapthene, & n-alkanes (12-22).

9) It seems inaccurate to call the first period the “non-control period” since control measures were in place.

Minor comments:

1) It may be more appropriate to use ”fine mode” rather than just “fine” when referring to particulate matter. Furthermore, “particulate matter” or “aerosol” is more appropriate than “particles”.

2) When referring to the extra material not in the paper, it is more appropriate to write “the supplementary material” rather than just “Supplement”.

3) pg. 32890 ln. 18-21: this is repeated from end of sec. 2.2.

4) pg. 32888 ln. 21: I would not call the tracer-yield method “very useful”. It is a basic method that offers a very rough way to estimate SOA.

5) pg. 32891 ln. 18: The statement about PAHs from volcanoes does not seem very relevant.

6) Tables 1-2: The format should have the month first before the year.

7) Fig. 1: Label control periods

8) Table S1 is almost completely redundant from the text in the paper; I suggest removing it.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 32883, 2012.