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Interactive comment

Interactive comment on "Characterization of organic aerosols from a Chinese Mega-City during winter: predominance of fossil fuel combustion" *by* Md. Mozammel Haque et al.

Anonymous Referee #2

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Review for "Characterization of organic aerosols from a Chinese Mega-1 City during winter: predominance of fossil fuel combustion" by Md. Mozammel Haque et al.

This study collected one-month of wintertime PM2.5 samples in Nanjing, China and analyzed the molecular composition of organic aerosols in the samples. Finally, PMF statistic method was used by the authors to perform the source apportionment. The paper was well organized, but the main shortage of this work is the insufficiency of novelty. Similar work has been published many times. From the references listed by the author, we could see that in the past decade several papers have published the results on the molecular compositions of organic aerosols in Nanjing and other Chinese mega-

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Discussion paper

cities. They also did some work on the source apportionment. Due to the significant changes in air pollution emissions from the traffic and the industry in Nanjing during the past decade, characteristics of the current organic aerosols in the city such as concentrations and compositions are expected to be different in comparison with those around ten years ago. Thus, I think authors should address this issue. Moreover, some statements in the text are not reasonable enough and should be revised (see detailed comments below). Generally speaking, this manuscript could be accepted after a major revision.

Detailed comments:

1. Line 109-113, experiment section, why authors choose this one month of time (11 December to 11 January,) to study the winter aerosols? Is this time long enough for figuring out the winter characteristics of air pollution in Nanjing?

2. Line 219-220, this conclusion needs more evidence, the current data do not support such a conclusion.

3. Line 239-241, this conclusion is inconsistent with authors' previous statement that NO2 is largely emitted from vehicular exhausts, here authors claim that both coal combustion and vehicular are the major sources.

4. Line 279-286, although CPI reported from this study is similar to that reported by Wang et al ., 2005 , the composition of n-alkane differs from that in 2005 in Nanjing, of which the highest n-alkane was dominated by low molecular weight congeners C22/C23, but here the maximum is C29, could author give some explanation?

5. Line 368-369, why current concentration of lignin and resin acids in Nanjing is much lower compared to those reported by Wang et al., 2006?

6. Line 454-464, here author stated that PAHs are from coal combustion, again imply that vehicular exhaust is not the major source of NO2 in Nanjing.

7. Line 587-590, why secondary oxidation products are formed during long-range

transport? Author should give more solid evidence to demonstrate that SOA in Nanjing in winter is mostly derived from long-range transport rather than derived from local emissions?

8. Figure 6. Nap is very volatile, and thus its concentration in aerosol phase is hard to be accurately measured. What is the recovery of Nap in this study?

9. Line 643-651. This paragraph is somewhat confusing to me. The sentences of the line 643-645 clearly say that here is the source contribution to the amount of OC, but the Figure 9 caption and the line 648-650 say that the numbers are the contributions to PM2.5, which is correct, please clarify. Moreover, many source apportionments have been done for PM2.5 and organic matter in Nanjing and other cities in China in the past decade. So, is there any difference in the source contributions to PM2.5 in Nanjing compared to those in the past decade. I think such comparisons are important for readers to understand the changes in aerosol chemistry along with the economy development in China.

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