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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "High Time Resolution Source Apportionment of $PM_{2.5}$ in Beijing with Multiple Models" *by* Y. Liu et al.

Anonymous Referee #1

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This study discussed source apportionment of PM2.5 in Beijing based on multiple models including PMF, footprint and NAQPMS models. However, there are too many studies with similar topics to resolve the sources of fine particle in Beijing. Nothing new findings and scientific questions were summarized in the abstract and conclusions even though it based on the high-time resolution data. Moreover, there are several questions and comments as follows needed to be addressed to improve the quality of this manuscript. 1. I suggest the authors to reconstruct the hourly PM2.5 mass based on the online chemical components (e.g. ions, OC/EC, and elements). The convert factor of OC to OM should be related to the sources rather than 1.8 which obtained in the regional parks in USA. Thus, the relationship between OC and EC should be discussed firstly. 2. I suggest the authors to present the data quality assurance and

control in more detail in section 2.2. For example, will the reconstructed PM2.5 match the measured PM2.5? What are the relationships between the elements (such as S, K, Cl and Ca) measured by Xact 625 and their water soluble form (SO42-, K+, Cl- and Ca2+) measured by IGAC. Moreover, the EC concentrations obtained by Sunset are also suggested to compare with the BC concentrations measured by MAAP or AE-31 etc. There are quite important for the source apportionment. 3. I suggest the authors to summarize the mass concentrations of chemical components in Table rather than in Figure for different episodes. It is important to judge the online data quality when compared to the previous studies in Beijing. 4. I don't think the explanation for the six factors in section 3.2 is reasonable, and it is not suitable for PMF to resolve PM sources with too many episode cases. The uncertainties of source contributions would be huge. For example, the contributions of the dominant sources during the episode cases would be overestimated during the non- episode period. 5. I also don't think the identified source profiles are reasonable. Firstly, K can be also originated from dust. Why not use K+? More fraction of K was showed in the industrial source rather than biomass burning. Secondary, industrial sources should be explained in detail. Why only little fractions of OC and EC in this source profile? Thirdly, why large fraction of Cl- was found in traffic emission? Lastly, why large fraction of Ni was found in biomass burning source? 6. What are the relationships between the tracers of identified sources and sources mass concentrations? 7. I don't think the discussions in 3.3 and 3.4 are necessary if the authors can't reply the above comments. 8. I suggest the authors to add more information about the spatial mass concentrations of PM2.5, PM10, SO2 and NO2 in Figure 7. Moreover, sources inventories used in this study would be suggested to add in the supplementary materials. The results resolved by the footprint and NAQPMS models should be discussed based on above information.

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