

Interactive comment on “Size segregated particle number and mass emissions in urban Beijing” by Jing Cai et al.

Anonymous Referee #2

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This study combined particle number size distribution with chemical speciation source apportionment methods and compared their results in a comprehensive manner, which provides more detailed information about primary sources in Beijing for the sampling period. Overall, it is an interesting manuscript relevant to source apportionment of atmospheric particles in megacities. The authors also provided detailed supporting information on the method and its validations. In general, the paper is well written and fits well to the scope of ACP. I would like to recommend that the manuscript can be published on ACP after the following minor aspects are fully addressed.

1. Since Size-PMF was much less applied than OA-PMF, adding a summary table about the previous studies using this method in the introduction section (at least in the supplementary information) could help the potential readers better understand its

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applications.

2. The noon peak of Traffic-ultrafine was explained by wind changes in the paper. However, it is also possible that some occasional NPFs and their following growth might also affect during this time of the day, even though NPFs days were fully excluded. The authors should also state this kind of possibility. Besides, it would be interesting to compare with the previous particle number PMF studies in Beijing.

3. Figure 2. The information of PNSD is not easy to follow when NPF and haze days are marked in blue and grey region. Maybe it could be clearer to use non-filled boxes. Besides, the legend of the subpanel (c) should be “T” rather than “TW”.

4. Figure 4 a, c. Please also add time scale in those two sub-panels as the x-axis.

5. Figure 8, there are too many sub-panels providing similar information. The authors should make this figure easier to understand.

6. Line 113 – 118, it is the first time in the paper that the authors declared that there was no strong coal combustion and biomass burning emissions during their observation period. Yet, more explanations seem to be given at the section of 3.2. Some of the descriptions should be moved here.

7. Line 174 – 176, The CE of ACSM also depends on ambient RH variations. If a dryer was applied, it should be clearly stated in the Method section. If not, the authors should explain the possible influence of RH on CE and how to exclude it.

8. Line 230 – 231, cold front is a meteorological definition. If the authors declare that cold fronts are occurring, more evidence of meteorological parameters should be provided. In my point of view, those shape decreases of PM in summer were mostly caused by precipitation rather than cold fronts, which was shown in figure 2 (d).

9. Line 120, in the method section, the authors used the term of ToF-ACSM for the short of Time-of-Flight-Aerosol Chemical Speciation Monitor. Yet, in the following sections, the authors also used the term ACSM instead (such as in Line 167 and Line 176).

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To make it different from Q-ACSM, it is better to always use the term of ToF-ACSM throughout the paper.

10. Line 369, the background for the nighttime Traffic-ultrafine type seems much higher than the simulated gasoline emissions. Except for diesel truck emissions and nighttime cluster formation listed, lower boundary layer during the nighttime would also be an important factor.

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