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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics and sources of submicron aerosols above the urban canopy (260 m) in Beijing, China during 2014 APEC summit" *by* C. Chen et al.

Anonymous Referee #2

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This paper presents a case study of pollution and meteorology during the APEC summit in Beijing, with a specific focus on ACSM measurements on the Beijing Meteorological Tower. While the techniques and the processes under investigation are by no means cutting-edge, the facility in unique in its capacity to study pollution and dynamics in a megacity environment. Furthermore, the APEC case study presents a very interesting case that will allow new insights into air quality control strategies and source apportionment to be made. As such, I find this very relevant to ACP. It is worth noting that another paper from this platform and study period, Xu et al., (2015), is also currently under discussion, however having read both papers, I am satisfied that there

is not too much overlap because that mainly focuses on the detailed measurements of the HR-AMS. However, while this paper is well-written, I do find that some of the interpretation needs revision and there needs to be more accountability on the PMF and clustering analyses, so therefore I recommend publication subject to the following comments:

The authors need to include more evidence and reasoning in the supplementary information about their choice of PMF outputs used in the analysis. Specifically, why a 2 factor solution was considered the most reliable and why a nonzero value of fpeak was used.

The fact that the authors conclude that the cooking and biomass burning were contained in the HOA factor but could not be resolved begs the question of whether this would be possible using the ME2 algorithm and lead to an improvement in the quality and depth of the science. Have the authors tried doing this?

I do not agree with the conclusions reached regarding aerosol acidity. The correlations on figure 8 are very good and large quantities of nitrate were measured. This to me implies that the aerosol was consistently pH neutral, because an acidic aerosol would not be able to support nitrate in the particle phase. I think it is far more likely that one or more of the inorganic calibration values was wrong. What RIE values did the authors use and how were these determined?

The concluding line on page 22906 seems a little out-of-place considering the discussion that follows it concerning back trajectories and sources to the south. Would it not make more sense to talk about the regulatory implications later in the manuscript?

Insufficient information is given regarding the clustering process applied to the back trajectories. What clustering algorithm was applied? How did the authors determine the optimum number of clusters? The authors should also report the number of trajectories that contribute to each cluster, as well as the percentages, so that their significance can be assessed.

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Figures: With the vertical profiles of wind speed and direction, it is not completely clear whether this is from the LIDAR or the in situ measurements. This should be made clearer. Also, the white areas on the plots should be explained.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 22889, 2015.