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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol composition, oxidative properties, and sources in Beijing: results from the 2014 Asia-Pacific Economic Cooperation Summit study" *by* W. Q. Xu et al.

Anonymous Referee #1

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This paper reports an observational case study of pollution in Beijing around the APEC period, chiefly with an HR-AMS. The techniques used are fairly well-established, but the results presented are extremely interesting because this presented a unique opportunity to study the different sources of megacity pollution in isolation. Several interesting observations are made regarding the different sources of PMF-resolved organics and the changes in the oxidation state of the organics and the sizes of the particles. It is generally well written and very relevant to ACP, so I recommend publication after the comments below have been considered.

This is not the only paper covering this case study; the paper Chen et al. (www.atmos-

chem-phys-discuss.net/15/22889/2015/) also covers this from the perspective of the measurements made on the Beijing Meteorological Tower. While I have read both papers and I am satisfied that there is not too much overlap between the two papers, I find it strange that this paper makes no reference to the Chen et al. paper, especially as the two papers share the same corresponding author. It would be useful to discuss the relevance of the findings presented here in the context of the observations of the other paper.

General: The use of the term 'oxidative properties' seems a little odd. This is normally used to refer to the properties of oxidizing agents or their precursors (e.g. NOx, O3), but here it is the oxygen content of the organic aerosol that is under investigation. I think it would be more correct to refer to the 'oxidation properties' throughout the manuscript (as it is done in a few instances).

Page 23411, line 5: The statement that 'the oxidative properties of aerosol particles remain largely unknown' is a little odd because there have been a large number of papers focusing on this exact topic in the last seven years using the techniques used here. While it continues to be a subject of much interest, I don't think the statement as it is written really stands.

Page 23415: The terms 'A-A' and 'I-A' should be defined.

Page 23418: The discussion regarding aerosol acidity is almost certainly not correct. If the aerosol was acidic, it would be unable to support nitrate, which would partition completely to the gas phase as nitric acid. Reports of acidic aerosols in the atmosphere measured using AMS (e.g. in marine locations) always feature very little or no nitrate and show much worse correlations than is presented in figure 2. It is far more likely that in this instance, the aerosol is pH neutral and the sub-unity NH4 meas/pred value is because sulphate RIE used here is inaccurate. This parameter can and does vary from the default value of 1.2 and should have been calibrated along with the RIE of ammonium.

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15, C7525–C7527, 2015

Page 23420: Could the variation in the HOA/BC ratio also be caused by changes in the relative contributions from the local combustion sources such as biofuel, coal and traffic?

Haze event: Please use a different numbering convention than 'S1', 'S2', etc. for the stages because these are also used for the supplement figures and I found this confusing.

Figure 13: The vertically-resolved wind speed and direction data needs to be properly introduced. Is this the same data as was used in figure 14a of Chen et al.?

Figure 14: The caption needs to be specific about what event is being shown in this figure

Supplement: Are the ion tracers referred to in S1 and S2 derived from AMS data? If so, these are internal, not external tracers.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 23407, 2015.