Response to Reviewer 2 comments

At first thank you so much for your comments, which are all important for our paper and research. Below you find my point-by-point response to your comments, where I first repeat your comment in italic.

The accurate measurement of potassium (K) with an AMS is difficult. Potassium readily ionizes on the 600 C surface of the heater via surface ionization. Tuning/detuning of the heater bias in the instrument is important to minimize the surface ionization signal in the resulting mass spectrum since it is not quantitative. The difference in noted RIE for K in this work compared to other work is likely due todifferent tuning of the instruments and enhancements in surface ionization generated K in this work (page 6 lines 9-14). Ion signals resulting from K surface ionization at m/z 39/41 are more broad than other ion signals as a result of the ionization occurring in a spatially separate area of the ionziation region of the AMS compared to EI ionization. The authors are encouraged to look at and include the raw mass spectra signal for these ions in supplementary information (e.g. see supplementary section in Aiken et al. 2009).

We agree with your comments. We added the raw mass spectrum for K in the supplementary information (Fig.S3), and describe the method used to estimate the K mass concentration. At the same time, we used the RIE of K from our lab calibration instead of using the RIE of others (see Section 2.3).

Given the uncertainty in the K and the lack of details in the paper, the organic potassium discussion and ion balance may need to be removed or updated with additional analysis.

Thank you for your comments. We deleted the organic potassium discussion, and described the method to correct the SO_4 mass concentration based on K/S ratio from the Drewnick et al. (2015) paper. The corrected SO_4 leads better attribution for K, and does not include any "organopotassium".

On page 3 line 5, the authors state that potassium evaporates and ionizes slowly. This is not always the case, and is dependent on the anion paired with K. KNO3 boils at 400 C, lower than the vaporizer temperature of the AMS, while K2SO4 boils at nearly 1700 C, so will take much longer to come off of the vaporizer.

Thank you for your comments. We modified the statement to "The slow vaporization of potassium salt, especially for K_2SO_4 due to its higher boiling point (1689 °C) than the vaporizer operation temperature (660 °C), made the quantification of K mass concentrations difficult and prone to large uncertainties", and added the related information in your above statement into text.

The measurements were performed at 2 different sites approximately 9 km apart (Page 3 line 9). Another key difference in sites is the sampling heights which is mentioned, but not discussed in detail. The ASRC site measures at an altitude of 81m, whereas the ACHD site measures at only 8 m above the ground. Is there anyway to clearly identify that the plumes measured at the 2 sites are similar? Could local emissions from people at the ground and commercially available fireworks/sparklers be contributing to the lower sampling height ASRC?

Thank you for your comments. The corrected PM$_1$ concentration at ASRC was near to the PM$_{2.5}$ concentration at ACHD, which indicated that the instruments at ACHD missed the strongest aerosol plume. This would be caused by the lower altitude (8m) of this site, compared to that of the Empire State Plaza (48m). Due to short distance between these two locations place and weak easterly winds, the aerosol plume did not efficiently diffuse to the lower level. It seems like that the instrument at ACHD just measured the lowest part of the aerosol plume. We added detailed discussion of the transport in the text(Section 3). Due to the laws in the city of Albany, which did not allow people to light fireworks themselves around Empire State Plaza, and considering the long duration of aerosol concentration; and that this was the only big FW display in the vicinity, it is reasonable to believe the aerosol measured from ACHD and ASRC site were both from the plaza FW displays.

As the authors indicate, the lidar data suggests an elevated plume at 400 m, so are the lower altitude sampling locations more likely sampling ground level emissions mixing up, or higher altitude emissions mixing down? This is an important question since the chemical composition may either be for large fireworks or for consumer fireworks which may have significant differences (e.g. sparklers vs rockets etc.).

Thank you for your comments. We have rewritten the paragraph in section 3.3 to describe the transport process. Our best explanation is that the wind caused different types of transport at different altitudes (or layers in the collapsing nocturnal boundary layer). Above 200m, wind was weak easterlies, while below 200m, the wind was still disarrayed (or calm). It seems that the higher level wind transported the most intense high level firework plume (such as the big rockets) faster to the west, while the lower level firework plume will move slower, which would match the time difference between the occur of these two plumes. We think our instrument measured the lower level aerosol plume during the event period and missed the higher level one. And based on the Albany Firework law, there were no consumer fireworks allowed at Empire State Plaza. So we think these two plumes were both from the FW displays, while the higher part moved faster to west.

The PMF analysis of the data is interesting, but the authors provide no details in why the 5 factor solution was chosen over higher or lower factor solutions. Did the authors investigate any rotation of

the data using the fpeak parameters?

Thank you for your comments. We added the related discussion in the supplement to show why we choose 5 factors. And we did the analysis using different fpeak parameters with a step of 0.2. We choose 0.2 for fpeak value based on the lowest Q.

In the supplementary, the mass spectrum for the HOA spectra has a higher signal from m/z 55 than 57, this may indicate a mixed cooking and traffic factor (see Mohr et al. 2012). Figure 5 indicates that the difference between the FW and the LV-OOA is primarily due to lower m/z 44 (and by frag-table extension m/z 28) and BBOA markers 60 and 73. While this graphic is useful, it may be more helpful to show this as a % change: (LV-OOAFW- OOA)/LV-OOA * 100% (e.g. Alfarra et al. 2006). Further, given that BBOA is also elevated anytime the FW-OOA is elevated, could these two factors be "trading mass" to make up for non-static source mass spectra?

Thank you for your comments. We added the related information about the signal of m/z 55 and 57 into the text, and relative variation of the mass spectrum of FW-OOA and LV-OOA in the supplement. There is also information about the temporal behavior of the factors, indicating some difference between the BBOA and FW-OOA. There could be some mixing of these factors during the event, but this would change the FW-OOA by at most roughly 10%.

Specific Comments: Page 4 line 9: With a 9 meter inlet and a silica diffusion dryer, could the authors comment on the estimated line losses from their sampling inlet?

Thank you for your comments. We calculated the particle loss based the particle loss calculator (http://www.mpch-mainz.mpg.de/~drewnick/PLC/). The flow rate for the 9 meter inlet was roughly a few hundred LPM, so we found there was little loss for it. We calculated the particle loss for the other tubes and dryers.

Figure 7 should remind the reader of the EDT UTC time difference, or better, change the axis to local time (EDT).

Thank you for your comments. We corrected to EDT time scale.

References: Aiken, A. C., et al.: Mexico City aerosol analysis during MILAGRO using high resolution aerosol mass spectrometry at the urban supersite (T0) - Part 1: Fine particle composition and organic source apportionment, 9(17), 6633–6653, 2009.

Alfarra, M. R., et al.: A mass spectrometric study of secondary organic aerosols formed from the photooxidation of anthropogenic and biogenic precursors in a reaction chamber, ACP, 6(12), 5279–5293, 2006.

Mohr, C., et al.: Identification and quantification of organic aerosol from cooking and other sources in Barcelona using aerosol mass spectrometer data, ACP, 12(4), 1649 – 1665, 2012

Ulbrich, I. M., et al.: Interpretation of organic components from Positive Matrix Factorization of aerosol mass spectrometric data, 9(9), 2891 – 2918, 2009.