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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the multi-day haze in the Asian continental outflow: An important role of synoptic condition combined with regional and local sources" *by* Jihoon Seo et al.

Anonymous Referee #1

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The paper by Seo et al. summarizes observations at a background and an urban site (Deokjeok Island and Seoul) during a multi-day haze episode in east Asia. The measurements are focused on aerosol composition and auxiliary gases. Meteorological conditions and weather systems associated with this event are also discussed. The main conclusion of the paper is that in Seoul, both regional transport and local emissions contribute to haze formation when meteorological conditions are favorable. The paper is overall well organized and well written. I support publishing the paper after the following minor comments are addressed:

1. p. 3, L 16: PM10 was also measured at both sites, correct? 2. p. 4, L7: what precautions were taken to reduce gas-phase artifacts in the OC filter measurements?

any back-up filters used? If not, please comment on the possible effects on the reported results. 3. p. 4, L 21: what was the total volume of the solvent mixture? Also, rephrase as "....and methanol (3:1; v/v) twice, each for 30 min." 4. p. 4, L23: Please comment on the possible evaporation artifacts when the samples are heated to 40 C. 5. p. 7, L 10: Considering the distance between Deokjeok Island and Seoul and the wind speed of 2 m/s during the haze period, it seems there would be a 12 hr transport time for plumes to travel from Deokjeok Island to Seoul. Couldn't secondary formation of aerosols during this time also contribute to higher pollution levels in Seoul compared to Deokjeok? 6. p. 7, L26: I don't think comparing CO and PM2.5 is correct since CO is a primary pollutant and PM2.5 is predominantly a secondary pollutant. 7. p.7, L30: PM2.5/PM10 values are not significantly different at the two sites, so I would remove the 2nd sentence in section 3.2.3. 8. p.8, L7: Contribution of HNO3 to NOR is not considered (possibly because the measurements were not available). Since temperature and RH affect partitioning of gas phase nitric acid to aerosols, without HNO3, NOR as defined is not that useful. I suggest removing NOR from the discussions in the text and tables. 9. p.8, L10: SIA fraction in Deokjeok is 51%, not 57%. 10. p.8, section 3.2.3 and 3.3: Although it's true that Seoul measurements include contributions from local emissions, I think it's also important to indicate that differences in boundary layer heights in Seoul vs. Deokjeok can also contribute to some of the observed differences in Fig. 4-5. 11. p. 9, L4: what are petrogenic sources specifically and how are they different than petroleum combustion processes? 12. p.9, the paragraph on the fractions of PAHs and emission sources is lengthy and at the end, it seems each pair of PAHs suggest one type of source impact. I suggest rephrasing and shortening this section. 13. p. 9, L22: As somebody who's not familiar with seasons in Seoul, it's surprising that measurements in Feb. would indicate biogenic alkane emissions from plant waxes. Aren't trees dormant in Feb in Seoul? Also related to the biogenic alkanes... it seems only C20-C36 alkanes that are found in biogenic emissions are characterized here. This certainly skews the results since midsize semivolatile alkanes are mostly associated with vehicular emissions (see e.g. Gentner et al., EST, 2016). This needs to

be addressed. 14. p. 10, L4: which carboxylic acid can originate as a primary aerosol component from fossil fuel combustion? The low O/C content of POA in many urban environments suggests primary OA does not contain such oxygenated compounds like carboxylic acids. This is again repeated in p. 12, L3-5. Higher acidic components in Seoul would suggest local SOA production rather than contribution from primary emissions. 15. p. 11, L18: To fully understand aerosol nitrate formation, some exercise with a thermodynamic model is needed because equilibrium partitioning of HNO3 to NO3- is RH and acidity dependent. Therefore, the high correlation of NO3- with RH doesn't necessarily mean NO3- was produced aqueously by uptake of N2O5. If the authors mean uptake of HNO3 instead (L20) then the process is not aqueous chemistry, but rather shifts in equilibrium partitioning and dissolution of HNO3. 16. p. 11, L26: it was indicated on L15 (P11) that NO2 decreased during 2/27-3/2 with high wind speeds, but that's not similar to the observations in CO that showed little difference with wind speed. Please clarify. 17. p. 11, L31-33: It's unclear why trends in organic tracers suggests regional transport of SO2. 18. p. 12, L7: Why is the O:C of carboxylic acids only explored? The conclusions drawn are acceptable if O:C represented values for all components of OA. 19. p. 12, L12-13: I disagree with the conclusion here. If RH and OC are not well correlated while RH and OC/EC is, this suggests to me that EC and RH are somehow correlated, but still EC is solely a primary tracer, I think the correlation is merely due to meteorology and cannot suggest anything about aq-phase production of OC.

Figures Fig. 2- please add the sampling site locations to one of the panels. Fig. 3- I understand that OC was the parameter directly measured, but since the pie charts represent total aerosol mass from each species, why not convert OC to OM, using appropriate, representative ratios of OM/OC? Fig. 6- I suggest marking the political boundaries with a different colors than the contours. Also, the wind arrows don't show up well. Consider making the panels larger. It would also help if the locations of the sampling sites are marked on one panel.

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