

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 #2

Received and published: 10 May 2017

MS No.: acp-2016-1184

“On the multi-day haze in the Asian continental outflow: An important role of synoptic condition combined with regional and local sources” by Seo et al.

This paper discusses the results of a study of PM_{2.5} bound inorganic - organic species and gaseous pollutants on the haze events in February 2014 at the outflow regions of Asian pollution. Meteorological factors and synoptic conditions are also discussed along with chemistry for the same context. These data of Asian outflow were collected at a site in highly industrialized region of Seoul and at a background site, Deokjeok Island, over the Yellow Sea. Paper contents important data and interesting discussions on the topic, hence I recommend for the publication in ACP. However, I have

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several comments/suggestions, especially at several places where statements are contradictory which should be addressed before making a final decision. P1,L16-18: Not clear. It is an overall general statement for both the sites. Better to summarise according to individual site, as in the following sentences. P4,L3-10: Do authors think that it is logically correct to measure PM_{2.5} mass using low-volume sampler and compare PM_{2.5} chemical measurement using high-volume sampler. Possible biases should be properly addressed here with references (e.g. MAPAN, 2013, Volume 28, Issue 3, pp 153–166). P4,L12-13: The readability/ sensitivity of microbalance should be mentioned here. P6,L13-16: Back trajectory analysis should also be discussed in support. P6,L17-18: Not clear. P6,L33: “compared and characterized in Table 4” should be “compared and summarised in Table 4” P7,L3: Please mention (number) boundary layer height and wind speed here. P7,L7-10: Backward trajectory should also be discussed here. P7,L12-18: Section 3.2.2, this is in contrast to section 3.2.1 and following sections, e.g. P13,L10-12, and several other places (please see comments below). P8,L1-2: How about the contribution of marine aerosols, especially at Deokjeok? Mass concentration of PM_{2.5} and PM₁₀ should also consider while discussing PM_{2.5}/PM₁₀ ratios. P8,L15: For more clarity, it is better to also use nssSO₄²⁻ concentration in discussions. P8L20-21: OC/EC ratios in Deokjeok and Seoul are 7.4 ± 1.7 and 7.3 ± 1.1 , respectively in haze period. Do authors think “The OC/EC ratio is higher in Deokjeok than in Seoul during both haze and clean periods.” is a correct sentence if authors see the values in view of statistical significance? Similarly check such statements in other places as well. P9,L4-16: Discussion is contradictory in context of sources discussed before this para and later in the text. P9,L18: “total sugar” should be “total sugar identified” P9,L31: Reference is needed. P9L33; P10,L1-2: Again contradictory statements (as pointed out above). P10,L10: “OC/EC ratio” same comment as in P8L20-21. P10, Section 3.3.2: Result suggests that particles are fresh in Seoul and comparatively aged in Deokjeok. An analysis of fresh and aged PM_{2.5}, PM₁₀ should be incorporated. P11, L5-8: How about boundary layer height? P11,L10-22: I suggest to check and discuss the relation of RH, NO₂ with

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sulfate formation apart from photochemical formation. P12,L1-2: I suggest to check and discuss primary and secondary OC contribution in haze and clean days at both the site to justify this statement. P13,L10-12: Not clear.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2016-1184/acp-2016-1184-RC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1184, 2017.