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Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Synergistic enhancement of urban haze by nitrate uptake into transported hygroscopic particles in the Asian continental outflow" *by* Jihoon Seo et al.

Anonymous Referee #2

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General comments:

Seo et al. combined measurements of PM2.5 mass and composition (from filter collection) in Seoul, Korea, with thermodynamic modeling in ISORROPIA II and back trajectory analysis. They find that particles influenced by regional transport from source areas in China have higher mass, higher inorganic aerosol content and higher water content. Specifically, the highest concentrations are observed in air masses under locally stagnant conditions affected by regional transport. They conclude that the synergistic effects of local stagnation and regional transport affect PM2.5 concentrations and composition.

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Discussion paper

Except for a few English usage issues, the manuscript is well written and within the scope of Atmospheric Chemistry and Physics. While the measurements are local, the main conclusion(s) are likely applicable to other regions and are therefore of broader interest. I have one major and several minor comments and concerns which should be addressed before publication.

Major comments:

In section 2.2, the author describe reconstructing concentrations of HNO3 and NH3 for their thermodynamic modeling analysis. I have two main issues with this:

1. In my opinion, the current version of the manuscript does not sufficiently justify the methods of reconstruction. For NH3, the authors seem to assume that the concentrations at the Gwangjin site are the same as the concentrations at the KIST site, and that they did not change between years. Please describe why these are reasonable assumptions. For HNO3, the authors seem to assume that the NO3/HNO3 ratio does not depend on the sum of NO3+HNO3 concentrations. Please justify this assumption.

2. The reconstructed concentrations certainly introduce uncertainty, which the authors recognize. For example, they comment (lines 122-123): "Although there are uncertainties in the reconstructed NH3 and HNO3 due to lack of direct measurements, their impact on the estimation of inorganic ALW and particle pH may be small enough." They follow this comment by a discussion on why the impact may be small enough. In my opinion, there is too much uncertainty here (in the data and the language, e.g. "may" and "would"), and I suggest that the authors conduct a sensitivity analysis on how uncertainty in the reconstructed concentrations of NH3 and HNO3 affects their conclusions.

Minor and technical comments:

line 12: replace 'stagnant' with 'stagnation'

line 17: replace 'group' with 'grouped' or otherwise revise as this is unclear lines 32-35: the sentence is unclear, especially the second half (... " and also a situation..."). Please revise (splitting into two sentences would probably help). Line 86: "The OM identified in this study is ∼5% of the total OM." I think I know what you mean, but this sentence is confusing to me. Perhaps rephrase as "The organic compounds identified in this study constitute ∼5% of the total OM."

Line 255: replace 'the more increase' with 'the higher increase'?

Lines 291-292: "Interestingly, SOR increase by temperature (and also irradiance) is not significant as much as inorganic ALW (Figs. 8c) despite..." please revise this phrase as it is not clear.

Lines 293-295: "This implies that the observed high SO4 in the S-T group was induced by the aqueous-phase oxidation of SO2 in the transported wet particles rather than the photochemical gas-phase oxidation." It seems appropriate to point out here that gas-phase oxidation likely also played a role (i.e. the data do not rule out gas-phase oxidation as a source of sulfate).

There are several instances where the article "the" is overused. As an example, last sentence in the abstract: "This study reveals the synergistic effect of remote and local sources on the urban haze pollution in the downwind region and provides insight into the nonlinearity of domestic and foreign contributions to receptor PM2.5 concentrations in the numerical air quality models". I would suggest removing "the" in front of 'urban', 'numerical air quality models'. This seems more consistent with common usage and would also further help to suggest applicability of the conclusions to other areas. I suggest the authors review the whole manuscript for use of "the".

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