

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-1098

Anonymous Referee #1

Referee comment on "Evidence of haze-driven secondary production of supermicrometer aerosol nitrate and sulfate in size distribution data in South Korea " by Joseph S. Schlosser et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1098-RC2>, 2022

This work details results of aerosol measurements during a pollution event in South Korea. Detailed size and composition measurements are presented from Incheon and Seoul showing increased levels of pollution and the possibility of secondary aerosol production due to high relative humidity. This event had higher than expected levels of aerosol nitrate in comparison to previously studied pollution events. This is believed to be the case because of low temperatures (along with the high RH). Previous pollution events studied have been in the summer when higher temperatures did not favor nitrate formation.

The analysis is sound, and this paper provides another example of secondary production catalyzed by high humidity during these pollution events. The results are not necessarily novel but they do lead credence to the role of secondary aerosol formation during these events – high relative humidity associated with these events exasperates air quality by increased oxidation of locally emitted NO₂ and SO₂. This also shows that this occurs in different seasons but can result in a different aerosol mix (higher nitrate).

Overall, the approach is reasonable. However, some revisions are needed before publication. A key flaw in the paper is one of its findings states that based on a size-resolved analysis of composition that composition of PM₁ can be used to understand PM_{2.5} composition. However, this is not necessarily true based on this analysis. This just shows for this one campaign where the aerosol composition seems to be atypical from other pollution events that it is a good proxy. In addition, as noted in the text, this is not the case for clean or "transition" periods. More analysis is needed to determine how prevalent these nitrate rich aerosol pollution events are, to see if this correlation between submicron and supermicron composition holds. Additional suggestions are provided below.

Minor Revisions:

- Line 106: Fig. S1 should be in the main text and a note on the distance between Seoul and Incheon. Figure S1 should include the location of the Incheon Met Site
- Line 151: I am not that familiar with the OPC-Grimm 1.109, but I believe it does not actually measure aerosol mass. It measures size distribution and then a density and calibration is used to calculate mass (from an online manual this may be done with dolomite dust). This should be addressed in the text. In addition, it gives a reason to use the Sungi PM2.5 in subsequent analysis as it is the same method as used in Seoul (which you typically did).
- Line 415-418: You state that the reductions in CO from the polluted to clean periods could be due to Chinese influence. However, if this were solely the case, wouldn't CO be higher (or comparable) at Incheon than Seoul. Seoul always has higher CO than Incheon. Maybe some of the enhancement is due to Chinese transport but in addition, the shallow boundary layer may allow for an increase in CO due to local emissions. Looking at CO/CO₂ would help in the future.
- Line 434-436: It is unclear what you are calculating by relative fraction in supermicrometer to all sizes. Does the 43% mean 43% of the sulfate is in the supermicron and 57% is in the submicron? Please clarify.
- Line 496-499: a statement is made that based on this campaign that PM1 composition measurements can be assumed to be the same as PM2.5 composition for modelling purposes. A cautionary note is included starting on line 501 stating that this is not the case for cleaner periods. While this is true for this case, it may not be the same for other haze events such as those mentioned when temperatures are higher and therefore the composition is different (less nitrate). It isn't clear how frequent these low temperature haze events are and in fact on line 524-527 you state this was a fairly uncommon event. The cautionary note here should be expanded to show the limitations of analysis from this one event for understanding the PM1-to-PM2.5 connection (here and also in the conclusion – line 557-558).

Typos/Suggestions:

- Table 1: include elevation; I think all are near sea level but it should be included
- Line 142: "Furthermore, the" should be "The"
- Line 199/208: BAM is an abbreviation for Beta Attenuation Monitoring
- Line 251: how can secondarily-produced species include primary organic aerosols? Should it be "includes primary organic aerosols and secondarily-produced species (i.e., SO₄²⁻ and SOA)"
- Table 3: caption repeats the dates related to the polluted, transition and clean time periods. This has already been stated so do not repeat it (it just makes the caption more cumbersome).
- Table 3 and following: ozone, NO₂ and SO₂ should be reported in ppb. CO can be in either ppb or ppm
- Line 284-285: For comparison. What is the Seoul - Sungi value for the entire polluted period, entire transition period and entire clean period.
- Line 360: The sentence starting with "Oxalate is produced" should be moved before the sentence starting "The strong correlation"
- Line 455-457: it is confusing to discuss both NOR and SOR value at the same time. Discuss nitrate in one sentence. And then sulfate.
- Line 558: "for haze pollution." should be "for this haze pollution event."