

Interactive comment on “Influence of Intense secondary aerosol formation and long range transport on aerosol chemistry and properties in the Seoul Metropolitan Area during spring time: Results from KORUS-AQ” by Hwajin Kim et al.

Anonymous Referee #1

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While this paper constitutes part of a large, multinational study, it must be judged on its own merit. The methods employed here can't exactly be described as new and the findings aren't exactly unexpected; most of the results (but not all; see cooking comment below) would come as no surprise to a seasoned AMS user. However, given the diverse nature of pollution in large Asian cities, there is a need to provide a knowledge base for large but relatively poorly characterised cities such as this, so that aspect of its novelty means that this is appropriate for ACP. In addition, from a technical perspective it is also interesting to note the performance of the combined organic-inorganic PMF in

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this instance; while success in applying this to AMS data has (to this reviewer's knowledge) been inconsistent, it is important to document the cases where this works and most importantly, the authors report the corresponding organic-only version in the SI. That said, beyond resolving the contribution of $m/z=30$ to the organics, it is questionable how dramatic a contribution to the science this makes.

General comments:

I would question the suitability of ascribing the COA factor to cooking. Normally, when this has been reported in urban areas, it has been accompanied by a very characteristic diurnal profile (e.g. <https://www.atmos-chem-phys.net/10/647/2010/>). However, this is not present here and the reasoning for assigning the factor as such is based on mass spectral similarity alone. However, this particular mass spectral profile can be seen as indicative of monocarboxylic acids generally and there have been studies where the association of this profile with cooking has been questioned (e.g. <http://pubs.acs.org/doi/10.1021/acs.est.5b02922>). Without any further supporting data here, it would be far safer to refer to this as 'COA-like' and caveat the discussion accordingly.

Generally, the overall size of the manuscript would be considered too large for the scientific findings that are presented, with some areas (e.g. the peak by peak discussion of the HR mass spectra) I would consider trivial, given the current state of the knowledge. Given what is already known from the preceding decade's worth of AMS papers, it would be better if the authors could focus better on what is genuinely new and different about this work.

Specific comments:

Page 12, line 15: Given the kinetics of the SO_2+OH reaction are very well known, does the measured trend in fSO_4 correspond to a reasonable atmospheric OH concentration? If not, then a different mechanism must be responsible.

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Page 12, line 15: Which aqueous-phase oxidation process are the authors referring to? The peroxide pathway (generally thought to be the most important) will not be available at night and while O₃ and NO₂ are possible, they are both pH-limited, so it is questionable whether they are important in haze (as opposed to cloud) droplets.

Table 1: Minimums and maximums are probably not the best statistics to report, as these will capture spikes. 5th and 95th percentiles would probably be better.

Figure 10c: The sheer number of overlaying trajectories mean that this figure is impossible to interpret. Please revise with a smaller number of trajectories.

Page 15, line 30: The similarity in N:C and O:C diurnal profiles merely tells me that there could be a source of aerosols with a high H:C ratio that is prevalent at night. It would be inappropriate to speculate on secondary mechanisms based on this result.

Technical comments:

Page 3, line 26: Suggest rephrasing 'mainland China' to 'mainland Asia'. Some of the emissions affecting Korea can originate from other areas (e.g. yellow sand from Mongolia).

Page 4, line 23: Change 'specially' to 'specifically'

Page 11, line 12: Change 'outskirt' to 'outskirts'.

Page 13, line 10: Presumably, the use of the product of NO₂ and O₃ is a measure of the production of NO₃ and in turn, as a proxy for the formation of N₂O₅ and consequently HNO₃. This should be explicitly stated because there are a number of other factors at work in this chain that ensure that while it is probably sound as a qualitative measure of nocturnal nitrate production, it is unlikely to be strictly quantitative.

Page 14, line 4: Should be 'the southwest'

Figure 4: The figure caption shouldn't really include a formula. This should be worked into the text that refers to it and numbered accordingly.

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