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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Continuous measurements at the urban roadside in an Asian Megacity by Aerosol Chemical Speciation Monitor (ACSM): particulate matter characteristics during fall and winter seasons in Hong Kong" *by* C. Sun et al.

Anonymous Referee #2

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This manuscript describes continuous ACSM measurements at a roadside urban site in Hong Kong. In addition to mass concentrations of inorganic and organic species species, this manuscript also uses PMF to further deconvolute the organic aerosol composition into four different organic components. The potential source regions (including locally produced vs. regional transported) of the observed organic aerosol are discussed. The measurements and manuscript are reasonable and subject matter is

appropriate for this journal. However, I think that several aspects of the analysis need to be revisited and clarified before the manuscript can be accepted for publication in ACP.

Main Comments

1) In page 19410, line 8-9 and in the PMF component spectra it is clear that the observed organic aerosol contains significant levels of m/z 60 and 73. The large body of AMS literature has shown that these ions are typically indicative of influence from biomass burning organic aerosol. These ions have also been used in ACSM studies to show biomass influence (A simple internet search with the keywords "biomass burning ACSM factor", for example, brings up several of the pertinent literature publications). Thus, it is very surprising that the authors do not mention this possibility in the organic aerosol analysis. Why is biomass burning discounted as a source? Some ideas on how the authors can check for the influence of biomass are:

-The timetrends of m/z 60 and m/z 73 can be analyzed and compared to each other as well as external burning tracers such as EC and CO. Plumes of EC and CO that do not correlate with NOx can be used as possibly indication of biomass burning influence

- A manuscript by Cubison et al. has reported f60 values in various airmasses with and without biomass influence. The observations from Hong Kong can be compared to those values.

- Comparisons of the observed SV-OOA spectrum to "standard" BBOA spectra in spectral databases

- Since the SV-OOA component concentration is largely influenced by regional continental transport, and appears to be particularly importaant in high concentration pollution events, it may be possible to see whether there is any correlation between SV-OOA and other data regarding fires in the region.

- The residuals in the PMF analysis of m/z 60 and m/z 73 can be investigated to see

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if they perhaps get a lot smaller at a larger number of factors and if perhaps a clean biomass burning factor splits from the existing SV-OOA component at larger factor numbers.

2) It is clear from the observed SV-OOA time trends (particularly peaks at 12 pm and 6 pm) that the SV-OOA factor contains some influence of the COA factor. Unless the SV-OOA species are secondary species formed from the cooking process (or other co-located SOA source), there is no other simple reason why SV-OOA should contain this diurnal trend. The authors do not clearly address how they tried to deal with this mixing. This is important considering that the SV-OOA is a significant fraction of the OA. Some questions pertinent to this are:

- Did the authors try to go to a much larger number of factors and investigate what happened to the COA loadings and correlation with the SV-OOA-like factor?

- Did the authors investigate the effect of fpeak on the time series correlation between SV-OOA and COA. What do the results look like at the fpeak setting where this correlation is minimized?

- Even if they are unable to use it for this manuscript, the authors should at least mention that ME-2 based analyses like possible with the SOFI tool could be a means of dealing with this.

- Relevant methods similar to those used by Aiken et al. to evaluate biomass burning in Mexico city (http://www.atmos-chem-phys.net/10/5315/2010/acp-10-5315-2010.pdf) could be attempted.

3) One weakness of this manuscript is that it reads like a report of AMS/ACSM measurements at yet another field site. It would be useful for the authors to provide as much intercomparison with other previous measurements as possible to provide a larger context within which we can understand these measurements. For example:

- The authors mention that transport from PRD can be a source of some of the ob-

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served aerosol at the Hong Kong site. How do the loading and composition of the aerosol particles observed at the current site differ from those previously observed in the PRD region? Is it possible, for example, that BBOA from the PRD is a source of the observed m/z 60 and m/z 73 in the ACSM spectra at this site?

- These measurements were conducted in the winter and fall. How do the results (absolute concentrations and relative compositions) differ with previous studies at the same site or similar site that were conducted at the same or other seasons?

Other comments

4) Section 3.2: When the various OA compoennts are described, it would be useful to have the brief description of their mass spectra (which is currently in the supplementary) included in the main manuscript to reinforce the key mass spectral features used in the factor assignments. Similarly, the discussion of the COA that is currently in the supplementary could be moved to the main.

5) Page 19415, line 22-19416, line 5: this section is a little long winded. It would be better to cut out a lot of the dicussion of the correlation coefficients, which are not really that useful, and instead point out the differences in actual mass concentrations for the different periods that are in the table. While it is useful to have done the HiOx and LOx comparisons, I think it can be summarized in a couple sentences and it is not clear to me that this extended discussion provides any more useful information about the SV-OOA than is possible from the diurnal cycle. So, I would get rid of this aspect of the discussion. It would be better to focus on the types of diagnostics suggested in comment #2 above in the main comment section.

6) Page 19418, page 3- Isn't the diurnal variation in aerosol components like Chloride also driven by the boundary layer? Are their measurements of CO that show how much the dilution is during the day? If so, it would be useful to show the CO diurnal trends as well for reference.

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7) Page 19420- what is the wind direction classification for C2? Is the main difference between C1 and C2 the fact that it rained? If so this should be stated. Also, it would be nice if in the discussion of Table 4, the periods that should be directly compared with each other due to similarity in source regions or other conditions are explicitly stated. Otherwise, the reader has to try and summarize for themselves the results from the analysis of table 5. It would also help if in Table 4, the source region classification (i.e. continental, coastal etc.) of each period was provided.

8) Figure 3: THe COA and SV-OOA MS correlations with the reference spectra look quite scattered. This is likely indicative of the fact that the SV-OOA has COA mixed into it. It would be useful in supplementary to show hte correlation spectra for SV-OOA and COA with markers corresponding to m/z so that hte masses that have discrepancies are more easily identified. Another option would be to draw the reference spectra behind each of the component mass spectra in gray so that the comparison can be readily made by the reader.

9) It is not clear that Figure 7 adds that much to the discussion. The conclusions from these figures could be stated in words in a few sentences.

10) Figure captions for Figure 9 and Figure 10. Please clarify what you mean when you say that the data is binned with a range of 7 ug/m3. This is not clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 19405, 2015.