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Atmospheric Measurement Techniques Discussions

Interactive comment on "High time-resolution chemical characterization of the water-soluble fraction of ambient aerosols with PILS-TOC-IC and AMS" *by* H. Timonen et al.

Anonymous Referee #1

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This paper presents data from a roughly 1 month study in Finland in which a variety of online instruments were deployed to measure aerosol composition. The authors present a unique approach to running some of the instruments and the data appear of high quality. The results are not highly novel, but are of general scientific interest, however, I wonder if a more detailed analysis could have been undertaken. The following are examples:

Many of the comparisons between measurements only give correlations, why not include other regression results (slope, intercepts).

The slopes for WSPOM vs Oxalate and POM seem surprisingly similar for biomass

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burning and non-biomass burning plumes, if true maybe discuss further?

Why is the slope for traffic sources so different (it's due to primary OA)?

Average diurnal profiles of WSPOM and other secondary compounds and T might also be of interest.

A positive correlation between the WSPOM to POM ratio and T is thought to indicate biogenic VOCs were involved in SOA formation because higher T leads to higher biogenic emissions. But why would higher biogenic emissions produce higher ratios? Is the idea that biogenic SOA is adding to the regional organic aerosol, some of which is not biogenic, hence the ratio increases? Higher ratios may just indicate more chemically aged aerosol; could that be linked to T?

Minor points.

The background is somewhat incomplete and more references could be added.

Page 1776, Line 10, change completed with compared ?

Page 1784, Line 15, should it read: filter placed on the oscillating element?

Page 1784, line 22, change smokes to smoke (also other places in the paper)

Page 1785, line 7, be specific, what secondary ions?

Any comment on why the ratio (slope in Fig 3) for WSOPM/POM seems similar for forest fire and other, even though very different sources.

Page 1787, line 21 states: This suggests that primary WSPOM is produced in biomass burning. (This point is also made in the Abstract). In a biomass burning plume, is a correlation between a species and CO sufficient to prove that the species is of primary and not secondary origin. It might be possible for a secondary species to also be correlated, at least to some extent. Maybe this could be tested. For example, K+ is clearly primary, maybe NO3- is mainly secondary? Is the K+ - CO correlation better

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or worse than WSPOM or NO3- vs CO? Later the correlation between WSPOM an oxalate is noted for smoke, since it is stated that WSPOM is primary does this mean oxalate is also primary? It would be interesting to compare oxalate – CO correlations. The point is that more thought should be given to the meaning of correlations with CO in the biomass burning plumes.

Fig 6, identify WSPOM/POM and T on the plot.

Fig 3, 4, 5, and 7, in some of these plots it would be helpful to give linear regression data (eg, r) on the plots.

In table 1, some other online WSPOM papers that could be included:

Hennigan, et al., Correlations between water-soluble organic aerosol and water vapor: A synergistic effect from biogenic emissions?, Environ. Sci. Tech., 42, 9079-9085, 2008.

Hennigan, et al., On the volatility and production mechanisms of newly formed nitrate and water soluble organic aerosol in Mexico City, Atm. Chem. Phys., 8, 3761-3768, 2008.

Weber, et al., A study of secondary organic aerosol formation in the anthropogenic-influenced southeastern United States, J. Geophys. Res., 112, D13302, doi:13310.11029/12007JD008408, 2007

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 1775, 2010.