**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 #2

Received and published: 21 May 2010

This paper presents a new approach which combines three analytical techniques (PILS, TOC, IC) to obtain high time-resolution data of water-soluble OC. The technique is highly interesting and useful for atmospheric aerosol research, and it is therefore of interest for the readers of AMT. In addition, the results presented show a very good agreement with AMS data. Unfortunately, the manuscript tends to over-interpret the data with regard to the discussion of the sources of the aerosol, which I believe should not be the purpose of the manuscript. The authors seem to extract more conclusions about sources than are really evident from the data presented, due to the fact that the dataset is probably too small. I would recommend that the authors put more focus on the technique and the comparison with the AMS results than on the interpretation of

the data on sources of WSPOM.

Specific comments:

- page 1776, line 23: "suggest" should be "suggests"

- page 1778, line 13: "water soluble fraction" should be "the water soluble fraction"

- line 14: "during measurement campaign" should be "during one measurement campaign"

- line 18: it would be interesting to briefly mention the online WSOC studies in other regions of the world (e.g., US, referenced in Table 1)

- line 22: please describe the "measurements", provide the dates, duration, time resolution, etc.

- page 1779, line 4: it would be useful to add mean PM values to provide an estimation of the aerosol load.

- line 23: "has been" should be "have been"

- page 1780, line 3: were any tests carried out to verify the denuder efficiency? And to ensure that there was no particle loss in the denuder system?

- page 1784, line 9: please provide reference for the 20% uncert, e.g., Peltier et al. (2010)

- line 26: "the ion concentrations" should be "the ion concentrations were"

- page 1786, line 17: AMS total mass is higher than the TEOM mass in Figure 2 (top). Despite the uncertainty of the measurements, AMS measures only non-refractary material and therefore the total mass calculated by the AMS would be expected to be lower than the total mass provided by the TEOM. How do the authors explain this?

- Figs 3 and onwards: it would be necessary to add regression equations and R2 values for all the plots. In their current form, interpretation by the reader is highly impaired.

- page 1787, line 4: what kind of samples were those described as "rest"? What were the main aerosol sources? And the r2 value obtained?

- page 1787, line 2: how is it possible that the correlation for all samples between WSPOM and POM results in R2=0.88, and once the dataset is divided into 3 sub-sets the highest R2 value is 0.85? If all the other R2 values are weaker, this cannot be possible.

- line 3: the absence of correlation between WSPOM and POM during traffic periods could be interpreted as the influence of traffic emissions from traffic (and therefore no WSPOM)

- line 18: are these subsets the same as those described above? If so, simply state "the same subsets". The use of the term "similarly" is confusing here.

- Fig 4: please add R2 values and equations. In addition to the dispersion of the data (r2 values), it would be interesting to highlight in the text the slopes of the regression lines, for example: for the forest fires the slope is XX (>1) indicating primary WSPOM emissions with increasing CO, whereas during traffic episodes the slope is YY (close to zero) meaning that there is no WSPOM emission. The same for the "rest" episodes.

- line 24: how can oxalate be a major component if it contributes with only 1-3%? PLease clarify or re-write.

- page 1788, line 23: "the WSPOM/POM ratio was found to have a similar temporal pattern..." I do not really see this pattern. I think the authors overly-interpret the data here. This should be removed from the abstract as well.

- page 1789, line 3: please inverse the order of 18:00 and 6:00

- line 6: "had a correlation" should probably be " had a certain correlation", the correlation is not that good.

- line 7: "typically below 50%..." I don't see this either. Please support this statement

with the mean ratios + std dev for <12°C and 12-18°C, or else remove it. If the ratiosn do not confirm the statement, the authors could simply write that they observed a "certain" correlation and that the order of magnitude is similar to that oublished previously by Jaffrezo.

-line 19: please add (BVOCs) after the definition of the term.

-page 1790, lines 1-15: this is the main ogoal of the paper, and I believe the authors succeed in it. However, they should avoid over interpretation of the data regarding the sources of WSPOM; the dataset is probably too small for this purpose.

- line 26, "SOA formation also" should be "SOA formation might also"; this hypothesis is not fully confirmed by the data.

- Lines 2-5: this last sentence should be removed, the data do not prove it.

- line 6: "system provide" should be "sysmet may proide"

- line 11: "observations were valuable" should be "observations may be valuable"

_____

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