

Interactive
Comment

***Interactive comment on* “Functional group composition of ambient and source organic aerosols determined by tandem mass spectrometry” by J. Dron et al.**

Anonymous Referee #1

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General Comments

The authors present results of measurements of carbonyl, carboxyl, and nitro functional groups in primary (wood and vehicles) and secondary organic aerosol (xylene oxidation) and in ambient aerosol, made using a new tandem mass spectrometric method the authors developed previously. This is a promising approach to functional group analysis and seems to work reasonably well for these complex samples, as indicated by comparison with expectations based on literature reports by others and concurrent analyses of some tracer compounds by GC-MS methods. The study is technically well done and the interpretation of the results is sufficiently thorough. There is a need for

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new methods of organic aerosol analysis, and functional group methods inhabit a useful middle ground between the OOA-HOA-BBOA-type information obtained by the AMS and the molecular speciation obtained by GC-MS. I suggest the paper be published in ACP after these minor comments are addressed.

Specific Comments

1. Page 9255, lines 23-27: Are you sure there is as much difficulty differentiating carboxyl and carbonyl groups by FTIR as you imply here? The Russell group has published a number of recent papers on these functional groups in ambient organic aerosol. Please be sure this comment is not out of date by looking at more recent references.

2. Section 2.2: I suggest adding a few sentences to describe what evaluations have been done to test and calibrate the methods, rather than expecting the reader to go back to the original references. For example, important issues include how well the derivatization and CID work for multifunctional compounds such as those found in SOA, and how robust calibrations are for such compounds. The discussion here suggests they work perfectly well for all kinds of acid and carbonyl containing compounds regardless of structure and have no interferences. This seems unlikely. Measurement uncertainties and potential problems should be discussed in the context of the complex samples being analyzed.

3. Page 9264, line 27: The Camredon et al. 2007 reference is for oxidation of alkenes not aromatics. The chemistry is different for these VOCs, so references on aromatics should also be used; perhaps the book by Calvert et al., 2002, *The Mechanisms of Atmospheric Oxidation of Aromatic Hydrocarbons*.

4. Page 9265, lines 19-25: How can you use the low carbonyl content to conclude that SOA is minor in the ambient aerosol, when carbonyls were also low in Mexico City and there SOA was significant? It would not be surprising if this is true, given your wintertime conditions. Nonetheless, the conclusions being drawn here about SOA are

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not necessarily consistent with functional group analysis, since they are based on the analysis of SOA from a single reaction system.

5. Page 9269, lines 29-30: The statement “consistent results” seems to be a bit overly optimistic considering that the functional group method concludes that SOA is negligible in the ambient samples compared to 20% for the tracer method.

Technical Corrections

1. Page 9257, line 25: Do you mean a heated tube? A rod is solid, so nothing would flow through it.

2. Page 9260, line 27: I suggest defining “functionalization rates” here instead of waiting until page 9262 since this is an unusual term and is used a few times before it is currently defined.

3. Page 9261, line 10: Should be "solvent" not "solvents".

4. Page 9267, line 13: I suggest replacing "plain lines" with "solid curves".

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9253, 2010.

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