

Interactive comment on “Simultaneous factor analysis of organic particle and gas mass spectra: AMS and PTR-MS measurements at an urban site”
by J. G. Slowik et al.

Anonymous Referee #2

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Conceptually, there is nothing wrong with what this paper sets out to achieve, however the analysis and conclusions are not currently sufficiently supported. One reviewer (Pentti Paatero in RC C114) has already pointed out many of the issues associated with the numerical methodologies and their presentation, however I feel that in addition, there are shortcomings in how the approach is justified and interpreted in a chemical and atmospheric context. For the reasons outlined below (notwithstanding those raised in the other review), I would recommend that this paper be resubmitted after major changes.

Implicit in the combining of the two datasets is the assumption that the organic parti-

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cle/gas system can be described by chemical profiles containing coupled particle and gas phase tracers, with fixed concentrations relative to each other. While this could conceptually occur, at the same time there may be reasons why it would not. For instance, many gas and particle phase species are contained within generalised primary emissions, but may not be emitted at consistent concentrations relative to each other and will have different lifetimes within the atmosphere, so measured concentrations would not necessarily be expected to correlate exactly. A similar argument could be applied to the precursors, formation and destruction rates of secondary species, which would lead to the relative concentrations varying depending on the photochemical age and initial sources of the emissions. It could also be speculated that there may be a chemical coupling between gas phase VOCs and aerosol organic matter, e.g. through semivolatile partitioning. While none of these would necessarily invalidate the work presented, these issues and how they affect the assumptions, results and interpretation should be dealt with much more thoroughly than it is currently.

One specific example of a shortcoming in the discussion is in the comparison presented in figure 13. There are many reasons why it may be expected that the relationship between OOA and the benzene/toluene ratio may break down, an important one being the contribution from biogenic VOCs. It may be that instead of improving the fit to OOA, the inclusion of PTR-MS data has simply pulled the time trend of the factor towards certain combustion-related gas phase markers and further away from the concentration of ‘true’ OOA. Under these circumstances, the approach would in fact be detrimental to the accuracy of the results.

I would echo the other reviewer’s request that results be presented in the form of tables as it is difficult to see from the paper in its current form what the quantitative atmospheric implications of the results are. Furthermore, it is also important to communicate how much uncertainty the inherent ambiguities associated with the analysis introduce and how this could affect arising conclusions. While this is discussed, it is difficult to pull the information out of the text as it is currently.

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In a more general sense, the paper does seem to lack overall context beyond the analysis itself. While the introduction gives a very generalised motivation, the paper does not explicitly define how this work addresses the issues at stake. Additionally, the site description is very brief and does not contain any information regarding surrounding buildings, land use, how heavily used nearby roads are, etc.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 9, 6739, 2009.