Atmos. Chem. Phys. Discuss., 6, S5838–S5840, 2006
www.atmos-chem-phys-discuss.net/6/S5838/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source apportionment of submicron organic aerosols at an urban site by linear unmixing of aerosol mass spectra" *by* V. A. Lanz et al.

Anonymous Referee #3

Received and published: 30 December 2006

This is a very interesting paper and certainly deserves to be published on ACP. Source apportionments of organic aerosol are highly topical and even though the authors' approach is fairly technical, they do a commendable job of explaining their methodology and results to a relative outsider. The manuscript is suitable for publication as is; the comments below are merely suggestions for minor improvements or clarifications:

1. The observation that the fraction of OOA decreases as more factors are included in the analysis is very interesting and, at the very least, shows that all of these estimates have significant error bars. To get an independent idea of the uncertainties in the source apportionment, I was wondering if the authors could do the following

analysis: given published mass spectra of the 6 factors they identified, can they run a multivariate regression on the data? I guess, this would be similar to Zhang's approach, but extended with 3-4 more factors. I would be interested to see what fractions are attributed to the different sources, and how they compare with the PMF results.

2. Page 11684, lines 1-7: it is suggested that the OOA-HOA analysis is an "established" approach to estimate SOA, which is an overstatement that the Zhang-Jimenez study is careful not to make.

3. Page 11685, lines 1-14: a reference to Quinn et al. (JGR 2006) could be added, who used principal component analyses using AMS data, some of the organic masses and many other gas- and particle-phase measurements to study the sources of OA.

4. Page 11689, lines 1-19: an assumption is made here on the weights that are used with the data. I can imagine that the results are somewhat dependent on this choice, and I was wondering if the authors tried other options and, if so, how similar or different the results were.

5. Page 11690, line 23: "During several of the evenings Ě charbroiling had been observed". Am I to understand that the charbroiling mass spectra are derived from the data set itself? If that is the case, isn't this somewhat of a circular argument: you derive a mass spectrum for charbroiling from the data set, do a PMF analysis, and voila a large fraction of data looks like charbroiling. Also, if the charbroiling spectra were not pure, but a mixture of charbroiling and something else, then I guess the charbroiling source would be overestimated in the analysis? So the main question is: how can the authors be so sure that the spectra they observed in particular cases are 100% from charbroiling. There may be a good reason, but as a reader it is hard to tell. Finally, the measurement site was evidently quite close to a charbroiling source, which suggests that the sampling site may have been biased towards such sources?

6. Page 11702, line 2: the main source of NOx apart from traffic is power generation. With regard to the latter: are there any power plants in the vicinity of Zurich, and

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are their emissions occasionally observed in the data? They should be easily distinguished from urban air: high NOx, possibly sulfate if there has been time to process the emissions, but no CO (or VOCs).

7. Page 11704, line 11: HOA emissions versus NOx are compared with published values. NOx, however, is very reactive and the HOA/NOx ratio given might be an overestimate if some of the NOx is removed before air is sampled at the site. In the absence of NOy measurements, this point should probably not be overemphasized.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11681, 2006.