

[Interactive
Comment](#)

Interactive comment on “Source apportionment and the role of meteorological conditions in the assessment of air pollution exposure due to urban emissions” by K. Schäfer et al.

Anonymous Referee #1

Received and published: 21 March 2014

The paper presents the results of a comprehensive measurement campaign that took place in Augsburg in winter. An impressive array of instrumentation was used, monitoring meteorological parameters as well as gaseous and particulate species with ultra-high time resolution. The results were mostly interpreted in an overly simplistic way, i.e. by various complex statistical methods. This type of approach is certainly not the most elegant one, since it totally ignores well-known casual relationships that exist between various gaseous pollutants and aerosol sub-modes. However, it might be quite useful to find relations between certain meteorological parameters (e.g. wind direction) and pollution profiles. In spite of the deployment of extensive monitoring instrumenta-

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



[Interactive
Comment](#)

tion, most of the conclusions are rather trivial and have been established previously in many studies. The fact that low mixing layer height and low windspeed lead to elevated pollution levels and cause the exceedance of PM limits is an example of such trivial statements. Another trivial message is that variations in the level of pollution are mainly due to changes in meteorological conditions and not in emissions.

A critical issue of source apportionment measurement is whether secondary organic aerosol component could indeed be predominant in winter or it is just an artifact resulting from biased source profile assignment (e.g. due to improper consideration of temperature-dependent partitioning of organic compounds). This is a promising question that is not exploited in the paper despite the wealth of available data. Since semi-volatile VOCs are generally more abundant in emission plumes than their aerosol-phase counterparts, and condense readily at low temperatures followed by chemical transformations, they may add to the identified SOA components in the particles. This process may not show up well in a statistical study using real-time correlations, since an undefined time-lag exists for their formation depending, for example, on irradiation which was not monitored during the study. It can be stated that a good tropospheric chemical model backed up with the measured high-resolution chemical and meteorological data may have improved our understanding to a much larger extent than the current statistical exercise did.

However, there is some potential in the statistical evaluation of the data which is not exploited in the paper. For example, variations in the nitrate-to-OOA ratio may reveal some interesting features. In general, correlations between various derived parameters could have been more useful than between absolute values. There is a lot more potential in chemical data that would corroborate the somewhat arbitrary and fixed source categories set up by the authors for their statistical analyses. These source categories are in fact far from being confined, they do evolve into one another under certain conditions (e.g. fresh traffic emission ages into aged one with corresponding shifts in size distribution and chemical properties, etc.). It could have been interesting

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

to see focused case studies on specific nucleation episodes, etc. Overall, the paper is not very innovative in using the very valuable measurement database and its standard certainly needs major improvement before consideration for publication in ACP.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2235, 2014.

ACPD

14, C623–C625, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C625

