Atmos. Chem. Phys. Discuss., 13, C12165–C12167, 2014
www.atmos-chem-phys-discuss.net/13/C12165/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Primary and secondary biomass burning aerosols determined by proton nuclear magnetic resonance (H-NMR) spectroscopy during the 2008 EUCAARI campaign in the Po Valley (Italy)" *by* M. Paglione et al.

Anonymous Referee #1

Received and published: 13 February 2014

This is a very interesting paper looking at combining HNMR and AMS data to deliver a more complete picture of organic particulates in a polluted European environment. The real novelty of this work is in the combination of the independent factorisation of both datasets to deliver insights into the processes that might not be possible with only one of the datasets. The two data sources do not agree completely, however rather than try to establish which technique is 'right', the paper wisely focuses on chemical reasons for these discrepancies in an attempt to better elucidate the processes being observed.

The conclusions have a certain degree of ambiguity to them, but I feel that the insight provided is certainly novel enough to warrant publication regardless. The paper is very well written from start to finish. I recommend publication subject to the following (minor) comments:

General: Not enough information is presented regarding the factorisation of the HNMR data (the AMS PMF analysis is covered by the Saarikoski et al. paper). The authors state their reasons for not using the solution sets with 6+ factors in section 3.4, however these results are not shown graphically, which would be very useful to see. These could very easily be included as supplementary material. Additionally, the factorisation also presumably needed to employ an estimate of measurement precision but the authors give no explanation for how this data was generated. The fact the Q/Qexp values in figure 5 are very high (around 30) may suggest that these uncertainty estimates are too low and therefore potentially wrong.

P33354, L8: The use of an exclusively flaming fire presents an issue because real domestic fires produce emissions through a combination of flaming and smouldering. Were smouldering fires investigated? If not, why not?

P33362, L16: This concluding sentence seems to be a little pointless. The high degree of substitution in organic particulates is something that is already well established, so I do not see the need to view this as confirmation.

P33372, L1: The Jimenez and Ng references are perhaps not the most appropriate because they do not deal with biomass burning specifically. Jolleys et al. (doi: 10.1021/Es302386v) and Cubison et al. (doi: 10.5194/acp-11-12049-2011) may be more appropriate.

Figures 1, 3 and 8: The regressions presented presumably use standard least squares fits, however given that it is not apparent which of the two measurements is the most accurate, orthogonal distance regression would probably be more appropriate.

Figure 5: This figure appears to be of a low quality. The final version should be in a vector format (e.g. eps)

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 33343, 2013.