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Interactive comment on “Sources and contributions of wood smoke during winter in London: assessing local and regional influences” by L. R. Crilley et al.

Anonymous Referee #1

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Referee Comments:

This paper deals with the study of carbonaceous aerosol in the great London area, by parallel sampling and measurement at 2 central London urban background sites, one on the top of a high tower, and at two rural neighbouring locations. Twenty four hours integrated aerosol samples were measured for carbon (OC, EC, 14C), levoglucosan and K⁺; BC was also measured semi continuously with multi-wavelength aethalometers. Using measured data and an array of source apportionment methodologies the carbonaceous component of the aerosols were quantitatively attributed to traffic emissions, biomass burning or gas to particle transformation.

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The paper contains a large collection of experimental information, which is thoroughly treated, compared and discussed, in order to justify and demonstrate the results and interpretations provided.

The paper is quite extended and long, and a bit tiresome to read and follow. Most of the sections in the paper are justified but I wonder if some of the sections could not be shortened to make the manuscript more linear and easy to read and interpret. I agree with most of the arguing and conclusions but some of the arguments are somehow repeated across sections. Also some of the discussion involves direct mentioning of figures and tables in an Annex section which in my opinion should be avoided (if figures or tables are part of the discussion they should be included in the main manuscript). As an example of sections that could be shortened without much loss of information is the discussion about the BT tower measurements.

I agree with most of the discussion and interpretation of data and data treatment results which are well explained and well demonstrated. Only a few points were less clear for me. I have some doubts about the correctness of the approximation to OC SOA being only originated from biogenic VOCs gas to particle conversion. Although that may be reasonable in non-urban areas, at urban central areas contribution from fossil fuel burned VOCs may not be negligible, principally taking into account that Primary OC/ECff ratios used for primary ff source evaluations were on the lower range of bibliographic data, more characteristic of tunnel conditions, not taking into account the relatively rapid condensation processes (with VOCff ??) that follow these fresh emissions which bring urban OC/EC ratios to 0.7-0.8 values. In my opinion this approximation should be discussed a bit further, now that in some points of the paper it was used for explaining discrepancies between results (for example paragraph 5 in page 27484). Figure 4 shows that OC SOA average concentration estimated at NK is almost the double of average values calculated for the rural sites. If OC SOA is only from biogenic origin which is the explanation for this fact? Higher biogenic VOC precursor concentrations in the urban centre? More effective gas to particle transformation

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processes in the urban environment? Or more effective gas to particle conversion of fossil fuel VOCs?

Another small point that is less clear for me- last sentence of paragraph 5, page 27482- I did not understand the explanation for the high correlation observed between BC_{tr} and levoglucosan in NK. Do you mean that the dominance of traffic emissions degrade the capability of the evaluation methodology to separate between BC_{tr} and BC_{wb}? Another possible explanation could be that peak concentrations of pollutants (which mostly influence correlation values) are mostly dependent of low dispersion periods of inversion.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27459, 2014.

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