**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Source apportionment of the carbonaceous aerosol in Norway – quantitative estimates based on ¹⁴C, thermal-optical and organic tracer analysis" *by* K. E. Yttri et al.

Anonymous Referee #2

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The authors use carbon-14 analysis together with a small number of specific chemical analyses to apportion carbonaceous aerosol from two sites in Norway to specific source categories. Although the sampling periods are rather brief and may be unrepresentative, the methods look broadly sound and the work merits publication subject to attention to a number of issues.

Specific points are as follows:

(1) Section 2.1 – details of the sampling sites are extremely limited and would ben-

efit from more information about possible local sources including road traffic. Is the rural background site influenced by emissions from local housing? How close are the nearest source emissions?

(2) The source apportionment is quite sensitive to the analysis of the specific chemical components and especially levoglucosan. While a reasonable level of detail is given over the analytical procedures, it is unclear how much quality assurance has been undertaken. In particular, the level of levoglucosan content of SRM 1649B is well known and evidence of measurement accuracy is required.

(3) Page 7386, LHS analysis – this seems a sensible way of dealing with uncertainty but two specific issues arise. Firstly, it is hard to believe that parameters are equally likely across the entire range between the low limit and the central value and the central value and the high limit. If these are truly limits (i.e. sharp shelves), it seems very unlikely that the probability of a value is entirely equal across the range. Surely, it is more probable near the central value? If this premise is accepted, then data should be input in terms of a probability density function which is highest at the central estimate and much lower at the limits. The second issue is that combinations of parameters leading to negative contributions have been excluded which will presumably lead to a positive bias in those outcomes. This issue needs to be reviewed and explained.

(4) Page 7390, last paragraph – the lack of partitioning of OC_{ff} into primary and secondary components is unfortunate. While the elemental carbon tracer technique is likely to prove highly misleading under these circumstances, the OC/EC ratio for road traffic emissions is rather well established and could be used to estimate a breakdown. The comparison between the urban and rural sites would be interesting. This would be likely to enhance the discussion in Section 5.1 on page 7394.

(5) Section 5.2 – why is OC_{BSOA} higher during daytime sampling and higher at the rural background site? This seems to suggest rather rapid formation. Is this consistent with the anticipated precursors? (6) Page 7400, first paragraph – it is now

revealed to the reader that the LHS processing gives a split between secondary and primary OC from fossil fuel sources. What is the mathematical basis by which LHS calculates this? How does it vary between the urban and rural sites?

(7) Why was PM1 studied rather than PM2.5? There is an argument that PM1 gives a cleaner split between the coarse and fine fraction but hard evidence of this is lacking. Since PM2.5 is far more important in a regulatory context, would it not have been better to study this fraction?

There are several minor corrections required.

(a) Page 7377, line 11 – should this read "terms of composition......"?

(b) Page 7395, lines 7 and 8 – this indicates that night was defined as 9am to 9pm. This is presumably an error.

(c) Legends to Table 7 and Table 8 – the word percentage is misspelt.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 7375, 2011.

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