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***Interactive comment on “Source apportionment of the carbonaceous aerosol in Norway – quantitative estimates based on  $^{14}\text{C}$ , thermal-optical and organic tracer analysis” by K. E. Yttri et al.***

**Anonymous Referee #1**

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The manuscript “Source apportionment of the carbonaceous aerosol in Norway - Quantitative estimates based on  $^{14}\text{C}$ , thermal-optical and organic tracer analysis” by Yttri et al. uses a set of different chemical tracers and radiocarbon analysis of TC for the attribution of emissions of carbonaceous particulate matter. The approach of Gelencsér et al. (2007) was slightly improved and applied to PM<sub>10</sub> and PM<sub>1</sub> samples from an urban and a rural station in Norway under summer and winter conditions. Final results contribute substantially to the understanding of emission and formation of carbonaceous aerosols so that I recommend the study to be published in ACP. However, revisions are

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necessary.

Main comments:

1. The Monte-Carlo analysis in the way it is applied is not sensitive to the problem, whether a special fraction is significantly lower than the detection limit or not. This deficit mainly arises from the exclusion of combinations of parameters producing negative contributions (7386/9-10). This procedure produces a positive bias to those fractions, which are calculated by subtractions or complex combinations of equations. OC(BSOA) is an example of such a fraction, especially when estimated for winter conditions. It is determined from Equation (11) by subtraction of parameters with large uncertainties, which implies that some of the LHS runs resulted in negative values. By deleting these solutions, a positive bias is produced. However, by omitting to discard the negative solutions and statistically investigating the detection limit of this fraction, the outcome will be more reliable. Consequently, unbiased (i.e. negative) values should be given in Tabs. 6-9 and S2-S5 as well and bars and pies should be removed from Figs. 1-3 for those fractions, for which the null hypothesis that no significant difference exists between the background and the calculated signal cannot be rejected. Insignificant results should be discussed accordingly (e.g. in chapter 5.2).

2. Tab. 7-9: It is stated for many samples that the precision of OC(pbs) and OC(pbc) is too low to be shown. To me, this implies that the results are below the limit of detection as well so that this issue should be addressed in the same way as above, including the removal of the bars and pies from Figs. 1-3. If the limit of detection is not undercut, it is at least also not justified to present results for OC(PBAP), as this fraction is the sum of OC(pbs) and OC(pbc).

3. The ranges of the emission ratios of (OC/TC)<sub>bb</sub> for PM<sub>10</sub> and PM<sub>2.5</sub> in Tab. 3 seems to be too narrow. I acknowledge the big advantage that the emission ratios applied in this study were thoroughly determined by the laboratory of the authors itself so that the method of OC/EC measurement can be ruled out as a source of er-

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ror. Nevertheless, different ambient burning conditions in the Norwegian households (such as types of fire place, flaming/smoldering, usage of wet or dry wood, ignition procedure,...) will cause a much larger variability in the EC formation than shown in Tab. 3.

4. Emission ratios for biomass burning in Tab. 3 are given for PM<sub>2.5</sub>, whereas PM<sub>1</sub> filters are sampled. The difference should be considered in Tab. 3 by increasing the range between low and high values and discussed in the text.

5. SOA from bb should be addressed as well, at least in the discussions. This SOA fraction adds to OC(BSOA) according to Equation (11): the (TC/LG)<sub>bb</sub> emission ratios (Equation 4) and thus the term TC(bb)\*F<sub>14C</sub> (Equation 11) do not allow for the SOA fraction of bb, as these value were determined in fires place experiments. Consequently, the definitions of ASOA and BSOA (Tab. 1) are misleading, as bb is an anthropogenic source but appears in the biogenic part of the calculations.

Further comments:

6. 7380/17-19: The back filters serve for two purposes; they help to correct for the positive sampling artifact and for the filter blank in one step. It seems to me that the front and back filters are treated differently, one being conditioned the other one not. This is problematic as the conditioning introduces a large blank to the front filters, which cannot be corrected for using the back filters. Consequently, the additional blank contribution must be considered independently to exclude errors of the OC/EC concentrations used for the source apportionment.

7. 7386/11: It should read “Table 2”.

8. 7387/26-28: This discussion it not clear and the numbers deviate from Tab. 3.

9. 7393/15-16: The results of Glasius et al. (2011) are not available yet so that numbers should be given for this comparison. Furthermore, the recent compilation by Hodzic et al. (Atmos. Chem. Phys., 10, 10997-11016, 2010) should be consulted as well.

10. 7396/9: The equation for determination of OC(PBAP) should be included in Tab. 2.

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

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