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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Receptor modelling of fine particles in Southern England using CMB including comparison with AMS-PMF factors" *by* J. Yin et al.

Anonymous Referee #3

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This paper describes CMB analysis of PM2.5 data collected in urban London and at a rural site in winter 2012, and compares the results with previous analysis by the same group and with results from other locations in Europe. The CMB results are also compared with PMF analysis of HR-AMS data for the urban site. The traffic source agrees well between the two methods. While there was reasonable correlation between CMB and PMF source attributions, CMB estimated more secondary and less biomass burning/coal and food cooking. This comparison of CMB and PMF is an important contribution. The paper is well-written and should be published with minor revisions in response to the comments given below.

1) Section 2.2.2: Why were the secondary biogenic markers not measured for the Harwell filters? The text says "(NK only)" without any explanation. It seems like this would be an important part of the comparison between NK and HAR.

2) Section 2.2 Methodologies: This section would flow better if you put the CMB analysis sections after the marker measurements and moved the AMS analysis to the end.

3) Section 2.2.3 AMS data analysis: The third paragraph about W-mode data and ME-2 seems unnecessary in this paper. Presumably these choices are discussed in detail in Young et al. (2014) and don't need to be rehashed here.

4) Section 3.1.1, first paragraph. Lists of numbers are difficult to read in text. Since the numbers are given in Table 2, maybe the text could just discuss the comparison.

5) Section 2.3.1. Are there markers for secondary anthropogenic aerosol that could be used in this analysis in the same way that markers for secondary biogenic are applied? It would be really interesting if you could get closure on the organic mass loading. Also in this section, is there a reason to prefer Hysplit backtrajectories (many of which originate at high elevations) to the NAME backtrajectories that were calculated for the ClearfLo campaign?

6) Page 24539, lines 5 to 9, "The derivations. . ..final choice." This sentence is long and confusing. Can you revise it into shorter sentences that make your points clear?

7) Section 3.3 Comparison between CMB and AMS-PMF estimates. "Comparison has been made. . .Fig. 5.": Reference to Table 5 should be to Table 4. I think Table 4 should have separate columns for PM2.5/OC and OM/OC since these are different conversions for some sources, e.g., traffic. The discussion of OC to OM conversion should be moved to this section from its current location in the third paragraph of Section 4. I found Section 3.3 to be very confusing until you explained what you meant by OM and PM2.5 in Section 4.

8) page 24542, lines 16 to 20: Is 0.78 a "stronger correlation" than 0.75? Looking at

ACPD
14, C9328–C9331, 2014

Figure 5, the data looks very similar.

9) page 24545, line 10: insert “such as” after “variables,” Do you have a reference for this sentence?

10) Section 4, third paragraph, converting OC to OM: One can get OC directly from HR-AMS measurements so why not compare the CMB OC directly to AMS OC? This would avoid some of the uncertainty in the OC to OM conversions.

11) page 24548, line 5: “other constituents” is really “refractory constituents”

12) page 24548, lines17-18, “It is conceivable that. . .”: This seems like an unlikely explanation since the difference in organic mass loading between PM1 and PM2.5 is usually only 10 to 20%. This doesn’t explain a factor of 3 difference between CMB and AMS.

13) page 24548 line 19: I would not say that the AMS CE is ambiguous. It is relatively constant over many field campaigns in many locations, although it can be different for particles with extreme compositions (e.g., very acidic).

14) page 24548, lines 25 to 30: I’m not sure what the point is of this discussion about some constituents of cooking aerosol being liquid at room temperature. The cooking aerosol agrees fairly well between PMF and CMB. The biggest disagreement between PMF and CMB for the primary particles is for biomass burning, but the liquid particle argument doesn’t apply there.

15) page 24549, line 26-27: Loss of levoglucosan from biomass burning particles during atmospheric transport is well-documented. Could this account for some of the discrepancy in SFOA between PMF and CMB?

16) References: There are a lot of typos in the references – please proofread them carefully.

17) Table 6: The first source is called biomass burning here, but SFOA or WS in other

ACPD
14, C9328–C9331, 2014

tables and figures. Maybe call it SFOA/WS for consistency.

18) Figure 3 and 4: The colors for Seasalt and Other OM are different between these two figures. Please use the same colors!

19) Figure 5: This figure is very hard to read. Please make the axis labels and legend much larger. It would help to number the panels and describe them in the caption. Why are the slopes in Figure 5 very different than the ratio of the numbers in Table 6 (e.g., slope of SFOA vs WS is 2.8, but the ratio from Table 6 is 1.9)? Also, the intercept of SFOA vs WS has a significant offset on the WS axis. Does this suggest an interference in the CMB extraction of the WS source?

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