Atmos. Chem. Phys. Discuss., 15, C2254–C2257, 2015
www.atmos-chem-phys-discuss.net/15/C2254/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Receptor modelling of both particle composition and size distribution from a background site in London, UK" *by* D. C. S. Beddows et al.

Anonymous Referee #1

Received and published: 5 May 2015

The paper by Beddows et al. shows the results of source apportionment on PM mass, number size distribution and a combined database of both measurements carried out in London over a two year period. They have used PMF to find the main sources of atmospheric aerosols. Overall, they have extracted 6 factors contributing to PM mass, 4 explaining number-size distributions, and 5 for the combined set of metrics. I recommend the publication of this article. It contains valuable results on source apportionment of both mass, number and combined mass + number size distribution datasets. But before publication, the authors need to consider additional discussion on different issues, as well as to reduce the length of specific points, mostly minor comments.

1) In page 10131, line 9. A brief description (at least the acronym) of the ClearfLo campaign should be provided, or some reference.

2) In page 10132, line 4. I suggest replacing the word “sources” by “emissions”.

3) In page 10133, lines 15-16. In the same line it appears two times the word “associated”.

4) In page 10135, lines 17-18. Recreational wood burning? Is there any reference to justify this? Could it be just that people on weekends stay during more time at home and consequently biomass domestic heating increases?

5) In page 10136, line 17. In the same line it appears two times the word “associated”.

6) From where have you identified low wind speeds associated with the Nucleation factor? From the values given in Table 2, it is just the opposite.

7) In page 10138, lines 25-26. The sentence “greatest density of London sources” is inappropriate. I don’t think all London sources are enhanced in the west of central London but probably the area is more affected by nearby vehicular emissions and/or by the” London plume”.

8) In page 10139-10140, lines 26-30 and 1-2. In this sentence, the fuel oil combustion factor is roughly attributed to shipping emissions, while before (for PM10), the interpretation was mainly focused on stationary sources (maybe power plants or industries): “The only constituent to show higher concentrations in the warmer months of the year is the Fuel Oil source. This might be attributable to emission from high chimneys with more efficient mixing to ground level during the more convective summer months”. Please, clarify this item.

9) In page 10142, lines 3-25. Although I agree with what is written there, I have found the discussion about CMB (a technique not used in this article) too long. I strongly suggest summarizing this couple of sentences.

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10) In page 10143, lines 4-7. I partially agree with this sentence, but the authors have to consider that the outcome obtained from the combined dataset is not irrelevant. Actually, from those results they (and the potential readers) are able to identify those chemicals driving each of the NSD factors. Furthermore, in terms of source contribution concerning common sources extracted by the single and combined NSD datasets, the divergences are not negligible. In this line, there is a lack of discussion in the manuscript considering these issues. Which one of the outcomes should be considered as more realistic?

11) In page 10150. It is surprising for me that the fuel-oil factor encompasses the lowest SO2 concentrations? Can the authors give an explanation about that?

12) In page 10150, Table 2. From the values reported there, I would be more convinced if the "diffuse urban" becomes "urban background, or urban plume". The word diffuse drive me to think is somewhat diluted, but conversely the factor encompasses the highest pollutant concentrations.

13) In page 10154, Figure 2. The total number of particles is around 5500 cm-3. In Reche et al. (2011), http://www.atmos-chem-phys.net/11/6207/2011/acp-11-6207-2011.pdf, the annual average reported for the same site was 12100 cm-3. Can the authors comment about these differences? Is this a product of abatement strategies? Is this a consequence of changing the instrument (CPC3022 to 3775)? Are other reasons behind (meteo, etc. . .)?

14) In page 10160, Figure 8. It is surprising the prompt increase in background concentrations of the "nucleation factor" from one year to another. From nearly 0 particles in 2011, with peaks during events, to a regular background around 500 particles in 2012 and a more noisy plot. Can the authors comment on it? Was the SMPS system modified in any way?

15) From this study one can conclude that most of the pollution measured in London is local in origin (from about 20 Km around). Roughly, at least 60% of the PM10 mass

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is from local sources (traffic+diffuse urban+mineral-road dust+fuel-oil). Obviously, part of the secondary factor is locally produced. To what point these results are in line with previous ones? In many cases I have heard and I have read that a significant proportion of the London PM comes from mainland Europe. But this study just demonstrates the opposite. I never trusted totally that history about such a dominant role of transboundary PM pollution except for specific cases. With these results the authors have the opportunity to highlight from where the problem is coming.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 10123, 2015.