Atmos. Chem. Phys. Discuss., 15, C5238–C5243, 2015
www.atmos-chem-phys-discuss.net/15/C5238/2015/

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.

D. C. S. Beddows et al.

r.m.harrison@bham.ac.uk

Received and published: 27 July 2015

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.

Added both acronym and reference.

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

Changed to emissions.

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

Replaced repeat.

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?

This has been modified to cite a reference for greater woodsmoke concentrations at the weekend, but not to comment on causes.

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

Replaced repeat here and at other locations not mentioned.

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.

For marine air, 8m/s is a moderate breeze and relatively low.

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".

The reviewer has misunderstood the statement: Since the NK site is west of the centre of London, most of the city and its emissions lies to the east of the site.

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.

We are grateful to the reviewer for pointing out this discrepancy. The wind sector with which the fuel oil factor is associated contains a number of refineries as well as coastal shipping. The text has been modified to recognise that both are most probably contributing.

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. This has been shortened considerably. 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?

The reviewer raises an interesting point. We have consequently added the following sentences to address the issue of source contributions.

"At first sight, the combined PM10-NSD analysis is attributing different percentages to the components (e.g. urban background) which overlap with the individual analyses. However, the point needs to be considered that while the analysis of the PM10 dataset attributes PM10 to source factors and similarly the NSD dataset attributes particle number, it is unclear what the combined analysis is apportioning. Consequently, the apportionment results should be viewed with caution as they relate neither to particle mass nor to number alone."

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?

The explanation appears to be that the fuel oil factor was associated with by far the highest mean wind speed (Table 2). The extent to which the SO2 is lower is surprising, but all pollutants show very low concentrations associated with this factor.

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.

We have re-named it "urban background".

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: : :)?

No, Reche et al are referring to CPC measurements in 2009 N7-1000. Our annual

average for 2011/12 is also very similar at 12,776 +/- 4,210 cm-3 using the same instrument. The value of 5500 cm-3 in the paper is the average for the total SMPS count which is N16-604. This is measuring a fraction of the particles measured by the CPC.

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?

The SMPS system has not been modified. As the factor is a composite parameter derived from PMF, it is very hard to diagnose why the background should have changed. Given the nature of regional nucleation processes, a background of zero would be expected, and any background is likely to be an artefact of the data analysis process and a lack of totally clean separation of the factors. Why this should apparently change between years is most unclear.

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 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.

We partially agree with the reviewer over this, but not entirely. We do not see a justification for the statement that most of the pollution measured in London is from about 20 km around. Taking PM10, the polar plots tell us that the urban background and traffic factors are highest at low wind speeds which emphasises the local contribution. The

polar plots, however, do not give a quantitative differentiation between emissions on different distance scales, just an indication of where the highest concentrations might arise from. Hence, stating that at least 60% of the PM10 mass is from local sources is not an inference that should be drawn from the data in this paper. The statement that a significant proportion of the London PM comes from mainland Europe derives from the fact that numerous studies using local wind direction and air mass back trajectory data have found that the highest concentrations of particulate matter at sites in southern England are typically associated with air masses arriving from mainland Europe. It should not, however, be concluded from this observation that the largest contribution to the annual mean concentration arises from mainland Europe as winds in this sector are significantly less frequent than those from westerly sectors. Consequently, attributing PM10 concentrations to sources in London, the rest of the UK, and mainland Europe requires an entirely different kind of study from the one that we have conducted.

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