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Interactive comment on "Sources of Submicrometre Particles Near a Major International Airport" *by* Mauro Masiol et al.

Anonymous Referee #2

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The manuscript describes the results from a monitoring station close to LHR airport and uses two different analytical techniques to apportion or explain the particulate matter observed at the site. The data was collected during two intensives, nominally a warm and a cold season. The two analysis techniques are used to estimate the influence of the airport on the local particulate concentrations. It is clear the authors have analysed the data in detail and the use of the two techniques to interpret the data is good, as well as acknowledging the limitations.

Overall, I think the work is suitable for publication. I have 3 main suggestions/questions and some detailed comments below.

Main suggestions/questions:

1) I do not think the introduction is suitable as it does not provide a good background to

C1

the manuscript (see details below). In addition, neither the introduction or the abstract mention the use of the regional nucleation event, which is really important for understanding a potential limitation in the two analysis techniques 2) I think a lot of the NOx and O3 details and graphs can be removed. The authors acknowledge that NOx has been studied in detail at the site before and the contributions from LHR to the site have been published. The paper itself is about the particulates, not air pollution in general. This would streamline the manuscript. 3) What version of AIM was used to analyse the data? If it was version 10, then no action is required. If it was version 9 however, then some work is needed. There is a significant difference between V9 and V10 in the diffusional correction algorithms. V9 over estimates the size of the correction needed and can lead to an over estimation of the nanoparticles, especially below 50nm. As the authors are dividing the particles into Aikten and Ultra fine modes (and the characteristics of these modes change with season), there is a need to check as it could lead to artefacts in the clustering.

Minor question:

Has the Aethelometer data been corrected according to Collaud Coen et al., 2010? I note the reported protocol as defined in Petzold et al., 2015. If not, it needs correcting especially if the authors want to draw BB aerosol information from it.

Details:

I question the relevance of some of the introduction to the manuscript. Lines 75 to 83 detail the pros and cons of a third runway, which I do not think adds to the manuscript as the study is about the impacts of Heathrow. It is fine to say the airport is planning to increase capacity, but as to why is not for this manuscript. In line 82, the use of the term 'Despite this' could be misinterpreted as questioning the UK government's decision.

Lines 85 – 104 Again, I think the context of this paragraph is not appropriate for the manuscript. The manuscript is about characterising the particulates close to Heathrow and not about questioning EU air quality standards as there is nothing in the manuscript

C2

that takes this data and compares mortality or morbidity predicted from these measurements with those based on the EU standard method, for example. This section should be that despite the UK meeting the PM standard, it has been show UFP have health issues etc and because there is no network of measurements, campaigns such as this are key to assessing the sources and potential impacts of airports on the UFP. The last sentence should be removed from this section. As the authors point out (line 98), there is limited knowledge in this area, so what should be regulated and how? EU has standards on particulates from cars. New aircraft will soon be regulated on particulate number and mass. EU law requires restricting exposure to nano-particles in the work place. The UK has the clean air act. So there are policies in place, but more info in needed before guidelines and strategies can be applied.

Section 2.1 – study area and dates. With the exception of the first sentence, this section should be in the introduction. It is background and context material.

Lines 150-151, no need to repeat the dates here as they were just in the previous section.

Line 151-152, data from the site is QA etc. Which data? The authors or the data from the AURN site?

Lines 288 – 303, this reads like an introduction and is better suited in the introduction.

Section starting at line 309. This is not an acceptable way to present data and I don't see what ranking them in order shows. This also seems to repeat a lot of information already contained within figure 2a. Suggest removing most of that section and simply referring to fig 2a, M&H 2015 and that the data is representative.

Line 318, Suggest changing 'Since' to 'Analysis of the data showed it was not…..' and provide a reference for the Kruskal-Wallis analysis.

Line 324 and figure 3. Firstly, if the PNSD are shifting towards coarser modes, how can the PVSD be almost constant? The author's choice of wording is contradictory. Sec-

ondly, in the summer it appears the mode during the day could be anywhere between 20 and 50nm and during the night it is around 35nm. Is the statement backed up by fitting a double log-normal to the data? Thirdly, why aren't the percentiles for the PVSD presented if they are shown for the PNSD? Finally, why are the medians presented and not the means, which is more common? Does the later analysis use the means or medians? If the former, figure 3 should show means.

Line 330, meteorology plays a role.

Line 345, constant to 11pm (same value pre noon in summer)

Line 349, the accumulation mode peaks at around 10pm, which is inconsistent with the statement that it follows traffic which peaks at 5-6pm, for the M4 at least.

Line 351, I don't think the use of the word intermediate is appropriate. It is either traffic or aircraft or mixed, perhaps. Furthermore, this is interesting. Why is there a difference in winter and summer? If Version 9 of AIM was used, could this change your results if you try version 10?

Lines 355 – 381. I do not think this section needs to be in the manuscript. The title of the manuscript is about the sources of the particles near an airport. The NO2 is a) not showing any directionality and b) has been extensively studies already. The same is true for figures 2 a and b, the NOx data can be removed from there to streamline the paper.

Line 424 – I think cluster 2 can be bi modal as well, it certainly shows in the percentiles.

Line 429 – suggest changing to 'the POTENTIAL role…..'

Line 445 - again, is it mono-modal or bi-modal? The small bump at 14nm is in the median and percentiles.

Line 457 & figure 4. The hourly count for cluster 4 is very noisy. Where is the morning peak? The one just after 6am? I also disagree it is the mirror image of clusters 2 & 3

counts. It shows a broadly opposite trend. Suggest rewording.

Cluster 1 & 5 in the cold season – It seems a little odd that the count profile of cluster 1 matches more the profile of LHR than cluster 5, yet the suggestion is that cluster 1 is an aged LHR aerosol, and cluster 5 is fresh because short transit times (high wind speed). Surely a fresh emission will match more the LHR profile, while the process of aging will remove or diminish the effects of source? I don't think the author's conjecture is correct for cluster 5 being fresh emissions.

Cluster 3 cold season – can you back up your BB conjecture with the Aeth delta C data averaged over the same periods? If not, you should remove it.

Line 509 replace hump with mode.

Line 581 – The contribution to the NO2 levels at LHR are quoted here as 25-30%, but in the previous section (line 367) is was found to be 15-17% from another study. Was this study different to previous ones?

Line 637 – 50 – 200nm is not consistent with the authors definitions of UFP (<100nm)

Line 694 – 'to' missing from second sentence.

Line 711 – 3% is not modest, it is minimal.

Line 848 – suggest removing Anomalously . Being downwind of an airport is expected to lead to higher loadings.