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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Airborne determination of the temporo-spatial distribution of benzene, toluene, nitrogen oxides and ozone in the boundary layer across Greater London, UK" *by* M. D Shaw et al.

Anonymous Referee #1

Received and published: 1 December 2014

Shaw et al., present airborne measurements of the concentrations of ozone and its precursors in the boundary layer above London with the aim of quantitatively determining their vertical, horizontal spatial and temporal distributions. To my knowledge these are the first such measurements obtained over central London and will no doubt be of interest to the readers of ACP. Overall the paper is extremely well written with an appropriate level of detail provided throughout and a considered analysis of the data with the authors careful not to overstate or over interpret the observations. I would therefore recommend publication after the following minor comments have been addressed.

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I feel the authors have missed an opportunity to look at the evolution of the London plume in flights 7-10 where the flight track is parallel to the wind direction. Surely these data provide a unique opportunity to compare concentrations of ozone and its precursors both up and downstream of London from which an approximate emission estimate can be derived and compared to NAEI emission estimates? In my mind an analysis of this sort would offer far more scientific value than reporting the city cross section concentrations. At the very least it would be interesting to see how the T/B ratios change as the London plume evolves.

Minor comments:

Page 27338, line 3, change “is” to “are”

Page 27338, line 7. In the context of ground level ozone and its precursors I am curious as to why you focus on benzene and toluene and not for example isoprene which has a greater ozone forming potential. A short sentence to justify your choice of compounds is warranted. Furthermore, why do you limit the number of VOCs you measure to just two compounds surely you could have increased your duty cycle without compromising the spatial resolution of the data?

Page 27341, line 6, change to “...adding a small flow..”

Page 27341, line 14, Please could you clarify the rate at which data were acquired and used in the paper. Earlier we are told they it was 10 Hz but here it suggests it is 1 Hz.

Page 2734, line 2, change to “setup”

Page 27343, line 9. Please could you clarify your use of the humidity controlled zero air. Did you modulate the zero air humidity to reflect ambient RH conditions or did you calibrate over a range of RH to assess the effect on instrument sensitivity?

Page 27343, line 19. Please include the inner diameter of the tube – this is the value that actually matters.

ACPD

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Page 27345, line 9. I am pleased to see you have taken the time to compare the PTR-MS measurements with the GC-FID WAS samples. This always provides additional confidence in the reported measurements. I see the benzene bias is half of that reported by Jobson et al., 2010 at 8%. This potentially reflects the differing operating conditions of the two instruments, with less fragmentation in your system operating at 110 Td as opposed to theirs operating at 145 Td. The agreement for toluene is slightly less convincing with the PTR-MS underestimating by ∼20%. This warrants some further comment. Can you suggest why the disagreement is so large? Finally, a 20 % negative bias on the toluene concentrations and an 8% bias on the benzene measurements will undoubtedly have a significant impact on the T/B ratios reported which the authors should also comment on.

Page 27349, line 11. It would be interesting to see the same plot for a different flight leg (R6) where the wind direction and hence LHA plume are slightly shifted. This might help separate the influence of local emissions from the LHA plume.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27335, 2014.