

Interactive comment on “Gaseous VOCs rapidly modify particulate matter and its biological effects – Part 2: Complex urban VOCs and model PM” by S. Ebersviller et al.

Anonymous Referee #2

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Review for gaseous VOCs rapidly modify particulate matter and its biological effects – Part 2: Complex urban VOCs and model PM S. Ebersviller, K. Lichtveld, K. G. Sexton, J. Zavala, Y.-H. Lin, I. Jaspers, and H. E. Jeffries

This manuscript follows part 1 by the same authors using a more complex, ambient atmosphere-like experimental design.

The following should be addressed before publication should be considered.

1. Each experiment was only performed once. I strongly suggest that repeat experiments are performed to obtain information of the variability of the results presented here.

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2. p.7375, line 12: The compound mixture used here seems to be derived from studies performed some 25 years ago. Although it is mentioned later in the manuscript that the composition of urban VOC mixtures did not change over this time, no evidence for this statement is given. This should be added.

3. p. 7376, line 18: It is argued that the seed particles used here (MOA) are a good surrogate of ambient PM. This statement should be supported with data or additional arguments, also in this manuscript. Ambient PM is a highly complex compound mixture with a large number of mostly polar compounds. Thus, I am not sure the apolar composition of MOA can be called a “good surrogate for ambient PM”.

4. The most puzzling result is that in experiment F (aged with MOA) the “gas-only” exposure generates the most pronounced result (Figure 10a, 11a). Very vague arguments are given on p. 7391: Line 11: Why should carbonyls explain this observation? They should be present in both experiments E and F. Line 13: The same is true for “undetected species”.

Two possible alternative explanations: A. The two exposure systems do not separate gases and particles so clearly as it is argued. B. Ozone/OH radicals present in the smog chamber in experiments E and F could alter the composition of MOA (generating an oxidized MOA). This could possibly partly explain why experiment F shows the most pronounced effects in the “particle-only” exposures. Generally I suggest that data of more experiments (repeats) and more control experiments should be shown.

5. Section 4.3: This is not a comparison with other work. Are there no other studies investigating particle effects with online deposition chambers?

6. Section 4.4: I suggest merging the “summary” with the following conclusion section.

7. Figure 8: Typos: The same experiments are labelled with “SE...” and “ST...”. I strongly suggest re-labelling the experiments and simply number them 1, 2, 3, etc. This would strongly simplify reading the paper.

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8. Figure 12 is hardly discussed. Considering that this is one of the main Figures (besides Figure 10 and 11) it should be either discussed or omitted.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7365, 2012.

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