

Interactive comment on “Secondary organic aerosol formation from gasoline vehicle emissions in a new mobile environmental reaction chamber” by S. M. Platt et al.

Anonymous Referee #1

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This paper presents results from the detailed characterization of a mobile smog chamber developed to simulate the aging of organics from primary emission sources. System characteristics such as leak rate, wall loss rate and actinic flux are measured and these data presented. The paper presents results from two vehicle emission aging tests conducted with this chamber in a regulatory emission testing lab with a Euro 5 light gasoline vehicle (LDGV). These experiments' results indicate that the SOA produced during aging of gasoline vehicle emissions greatly increases their overall PM emission factors. Accounting for this secondary production increases LDGV emissions factors to be on the same order as those from a Euro 3 diesel vehicle, which are mostly composed of primary organic aerosol (POA) and black carbon (BC) components.

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This paper is well written, detailed and clear and the work upon which it is based appears to be carefully conducted. The chamber is a useful, if not entirely novel, piece of experimental apparatus that has been well-characterized. While I do think that this is the most 'fully featured' mobile chamber that I have read about, this set of activities alone is not of sufficient interest to warrant publication in ACP.

On the other hand, the LDGV experiments do show new results that are of great interest to the community of researchers working on vehicle emissions, evolution of organics in the atmosphere and urban and regional air quality modeling. However two shortcomings seem somewhat glaring when comparing this paper with others of its kind, including those from the same group: 1) there are only two experiments with one vehicle and they do not 'cover much ground' in terms of the variety of conditions (e.g. vehicle test cycles, VOC to NO_x ratios, vehicle type and age, etc.) that may influence the overall contribution of these vehicles to ambient PM loading, and 2) the experimental results (e.g. evolution of aerosol mass spectra and gas phase species) are not very thoroughly (or not at all) presented or analyzed. The former point is especially glaring when you consider that a previous study by this group of diesel vehicles (Chirico et al. 2010) included results from 20 experiments with 3 different vehicles and much more in-depth analysis. The latter point is important so that the results of this study can be interpreted or expanded upon by other researchers. It's not clear to me why a fuller exposition of these experimental results is not included.

In general, I think many of the details on the chamber and its characterization are suitable for an online supplement to a research article. Were this the case, there would only be enough material in this paper for a short research communication. Therefore, while the work is of high quality and of interest, I question whether it is of sufficient novelty and substance to warrant publication in ACP in its current state.

I will not give an exhaustive list of the types of further experiments or analyses that I feel would be appropriate for a description of this research (above I suggest several different experimental configurations or analyses that could lend further insight). Rather, below

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I highlight some major points and a few minor errors or clarifications that should be addressed regardless of whether the authors choose to more fully present these or related experimental results.

Major points

P28354, L2: The fact that the lower edge of the transmission window for this lens is somewhat concerning given that vehicle-emitted aerosols are often right in this range or below. SMPS or AMS particle time of flight data should be used to indicate that mass measurements are not biased. More information on the particle size distributions and how they evolve with coating would in general be a good thing to include/discuss.

P28354, L3-9: No results based on this methodology are presented later on in the paper, though there are a large number of potential issues. This should be included in discussions of the results. For example, does the correspondence between mobility diameter and equivalent diameter change as the particles are coated? The initial OA/BC ratio is $\sim 1:1$, but this changes by more than a factor of 10 as the SOA formation occurs. How do the AMS collection efficiencies compare with what has been observed in other studies? Why are SMPS data not shown, described or used? More information on spectral characteristics of the POA and SOA are also appropriate given the focus of the paper.

P28362, L11-13: If it is the case that you can identify much of the VOC reacting in the chamber, why are none of these data shown? This would seem to be a very important piece of the puzzle, yet no portion of it is included. Again, given the fact that there are 2 experiments worth of data, I would expect more time would be spent actually delving into this data.

P28363, L28-29: You suggest here that the addition of propene should not affect SOA measurements to a large extent, though it is well recognized that NO_x levels can have a controlling effect on SOA yields (e.g. (Ng et al. 2007; Presto et al. 2005; Presto et al. 2010)). I take the statement to mean that the propene doesn't form SOA, which I

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expect is probably true, but the VOC-NO_x ratio may have a substantial effect on yield and chemical composition of SOA.

Minor points:

P28345, L22-25: Awkward sentence and not fully justified point. Authors should specify from the beginning that this is referring to Europe and then justify using some fuel use data.

P28347, L12: Missing punctuation? Should be a new sentence.

P28347, L13: 'chamber' misspelled

P28352, L26-28: Very awkward sentence.

P28360, L13: missing a 'h'

P28360, L22-23: I think this should indicate that lower yields are observed at lower OH exposure intensities – this sentence makes it sound as if SOA evaporates when the OH intensity is reduced.

References

Chirico, R. et al., 2010. Impact of aftertreatment devices on primary emissions and secondary organic aerosol formation potential from in-use diesel vehicles: results from smog chamber experiments. *Atmos. Chem. Phys.*, 10(23), pp.11545–11563.

Ng, N.L. et al., 2007. Secondary organic aerosol formation from m-xylene, toluene, and benzene. *Atmospheric Chemistry and Physics*, 7(14), pp.3909–3922.

Presto, A.A. et al., 2010. Secondary Organic Aerosol Formation from High-NO_x Photo-Oxidation of Low Volatility Precursors: n-Alkanes. *Environ. Sci. Technol.*, 44(6), pp.2029–2034.

Presto, A.A., Huff Hartz, K.E. & Donahue, N.M., 2005. Secondary organic aerosol production from terpene ozonolysis. 2. Effect of NO_x concentration. *Environmental*

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Science and Technology, 39(18), pp.7046–7054.

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