

1. The authors' reply to my comment #1 added information about the tubing materials and the Go:PAM reactor geometry, but information about the inlet tubing length, diameter, and residence time were not given, and are still needed.
2. The reply to my comment #2 concedes my point that the different residence times complicate direct comparisons between cooking and vehicle exhaust measurements. This needs to be stated in the methods section, so that readers understand the importance of maintaining a fixed residence time and apply it in their own work. Similarly, the concluding section would benefit from a sentence or two that is more committal to future better-quality OFR experiments than "we really hope we could strictly control the temperature, RH, and other conditions". The authors argue that that the relative humidity, although different between in the two sources, is low enough in both cases to minimally influence heterogenous chemistry. This is a fair point, and one that should be added to the revised manuscript.
3. In response to my comment #3, the authors added fractional contributions of alkanes, alkenes, aromatics, O-VOCs, and X-VOCs to the measured VOC emissions from each source, along with kOH values for the major components of each of those compound classes. This added information is appreciated and is likely sufficient to give the reader a general idea of which compounds contribute to the calculated OH reactivity values. However, in my opinion, the discussion of which emitted compounds contribute to the measured SOA formation is an important outcome of this study whose discussion remains unclear/incomplete. For example, the SOA/POA ratio was ~100 for GDI emissions and ~2 for cooking emissions, but only 29-35 ppb VOCs were speciated in GDI emissions and 124-189 ppb VOCs were speciated in cooking emissions. It is clear from these numbers that the aggregate SOA yields of cooking VOCs are lower than GDI VOCs. It is not clear how much of the observed SOA can be explained from the speciated VOCs (and laboratory SOA yields obtained in chamber studies), and whether an unresolved complex mixture of (I)VOCs is plausibly an important class of SOA precursors, particularly for the GDI emissions. To increase the scientific significance of this manuscript, I feel strongly that additional analysis needs to be done in this regard, such that we can gain insight into contributions of measured VOCs to observed SOA formation in both sources. See, for example, Figure 3 of Liao et al., ES&T, 2021, reproduced below for reference – it should be possible to perform a similar analysis from the data obtained here.

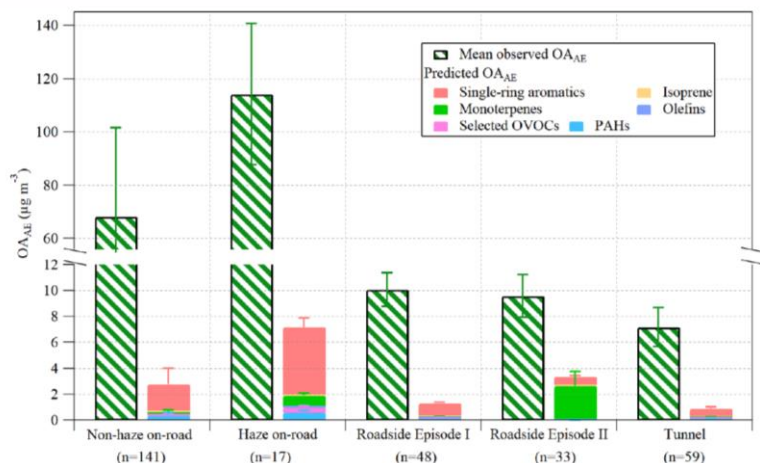


Figure 3. Mean observed OA_{AE} corresponding to the equivalent photochemical age of 1.5 ± 0.2 days and the predicted OA_{AE} contributed from OH oxidation of precursors measured with PTR-QiTOF. Error bars show the standard deviations of the data for each case with the sample size shown as *n* below. VOC categories are described in the Materials and Methods section. Selected OVOCs include CH₃OH, C₂H₆O, C₄H₈O, C₄H₈O₂, C₅H₈O, C₆H₈O, and C₈H₁₀O.

4. OK
5. The addition of Table S7 is useful and appreciated. However, the discussion about non-OH artifacts misses the point : ozonolysis is (likely) important for unsaturated fatty acids that were probably emitted from the cooking sources, and perhaps measured (e.g. oleic acid parent and dehydration peaks at AMS $m/z = 282$ and 264), but have not been analyzed. At the least, a couple sentences should be added to the discussion indicating that contributions from ozonolysis of unsaturated fatty acids to SOA/OPOA are not constrained in the data that has been analyzed but may be important.
6. I suggest paraphrasing this response and adding it to the revised manuscript.
7. This response does not address my comment. Cleaning the (dark) Go:PAM by flowing dry air and ozone through it (very likely) does not remove all of the residual organics that are reactive towards OH but not O₃. Thus, the next time the lights are turned on to make OH, some of the SOA formed is almost certainly associated with residual organics that were not sufficiently removed from the previous experiment by only flowing dry carrier gas + ozone through the dark OFR between experiments. As far as I can tell, and based on my own experience, it is impossible to rule this out the way the experiments were performed. This is a flaw in the cleaning protocol that has to be expressed in the methods section - i.e. that the SOA that is formed in any experiment with OH as the oxidant represents an upper limit because the background is not well constrained. This should be directly addressed in future studies by adding humidity to the carrier gas and turning on the lights during the OFR cleanout stage.
8. Thank you.
9. I disagree that discussion of acetylacetone is useful when there is no acetylacetone in the emissions. Please focus the discussion of non-OH artifacts on the alkanes, alkenes, aromatics, O-VOCs, and X-VOCs that were measured, and the co-emitted compounds that were emitted and (likely) measured but not analyzed (e.g. fatty acids).
10. No additional comments
11. See response to comment #3. The authors argue that it is beyond the scope of this paper to discuss the SOA formation potential of individual measured VOCs. I disagree – in my opinion, that is one

of the potentially most novel results for which data is already available. Further analysis and discussion is required (e.g. Liao et al., 2021).

12. Thank you.

13. Figure 2 would be much easier (for me) to read if it were split into two panels or a single “split” y-axis, one with an SOA/POA axis ranging from 0.4 to 4, the other with an SOA/POA axis ranging from 40 to 200. The maximum photochemical ages (~3 and 6 days) are close enough that, in my opinion, they can both be plotted satisfactorily on the same x-axis, here and also in Figure 3.

14. Thank you.

References

Liao, K., Q. Chen, Y. Liu, Y. J. Li, A. T. Lambe, T. Zhu, R.-J. Huang, Y. Zheng, X. Cheng, R. Miao, G. Huang, R. B. Khuzestani and T. Jia. Secondary Organic Aerosol Formation of Fleet Vehicle Emissions in China: Potential Seasonality of Spatial Distributions. *Environmental Science & Technology*, <https://doi.org/10.1021/acs.est.0c08591>, 2021.