

Interactive comment on “Emissions of intermediate-volatility and semi-volatile organic compounds from domestic fuels used in Delhi, India” by Gareth J. Stewart et al.

Anonymous Referee #2

Received and published: 21 October 2020

Stewart et al. have developed a gas-chromatography-based analytical technique to speciate and quantify semi-volatile and intermediate volatility organic compounds (S/IVOCs) and applied this technique to measure emissions of S/IVOCs from domestic fuels used in Delhi, India. S/IVOCs are important precursors to ozone and aerosol formation in the atmosphere and there is need to develop robust analytical techniques to speciate and quantify their emissions. Biomass burning is an important source of global air pollution and the type of biomass burning studied here (i.e., biofuel combustion) is a particularly understudied emission source. Hence, the work described in the manuscript is well motivated. I should also commend the authors for a well written manuscript that provides all the necessary details to comment on the methods and the

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interpretation of the results. The analytical method development was well designed and the application was very well described, although I should note that I am not trained as an analytical chemist. This should serve as a useful resource for researchers doing similar work in the atmospheric community. The primary results of speciation and quantification are well described too but, given the large dataset that is being analyzed, only a small fraction of the data are actually presented. I recommend the publication of this study in Atmospheric Chemistry and Physics after the authors have had a chance to respond to my, mostly big-picture and minor, comments.

Big-picture comments: 1. The introduction seems too generic and long at the moment and needs to be realigned to describe the state-of-the-science and gaps as it relates to the key findings from this work. For instance, lines 53-68 discuss S/IVOC emissions generally but don't focus on those emissions from biomass burning. Text from lines 69 to 107 could be condensed into a few sentences. Earlier work relevant to this paper seems to be mentioned in lines 108 to 145 and needs to be highlighted, front and center. Another point that could be highlighted is that S/IVOC emissions are poorly, if at all, represented in emissions inventories and chemical transport models and their impacts on atmospheric chemistry and air quality are uncertain (with particular relevance to regions where this and similar fuel use is dominant, e.g., Asia). 2. Given the large variability seen in biomass burning emissions, say relative to internal combustion engines, the authors should comment on the single experiments done for most of the fuels. This could be done by analyzing the experiment-to-experiment variability for the fuels where multiple experiments were done (i.e., cow dung cake, waste), as well as through a review of similar literature. In addition, they should also comment on the differences in combustion encountered in their setup versus a real-world application. For example, most municipal solid waste is probably burned in a high-temperature incinerator where the combustion chemistry might be very different than the combustion simulated in this work. I do understand that 'backyard' low-temperature MSW fires are a major concern in India, including in Delhi. 3. I commend the authors on putting together this fantastic dataset of speciation and emission factors and I am fairly certain that this will serve as

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a comprehensive resource for years to come (from studying exposure to toxic pollutants to developing accurate emissions inventories for air quality modeling). [This must be an oversight but I did not see a 'data availability' section that describes how and where the data will be archived for others to use]. However, the manuscript seems to present only a 'snapshot' of the dataset, with a mix of higher-level observations and depth for only a subset of speciated organic compounds (e.g., PAHs). Correct me if I am wrong but there is so much more to the dataset than what is presented. If that is indeed the case, what I would have liked to see is a structured vision for how the data plans to be analyzed further (e.g., detailed source profiles, molecular markers for source identification, volatility distributions) and what open, pressing questions would this dataset help answer in the long run?

Minor comments: 1. Line 228: Explain what 'NIST library hit was >800' means. 2. Line 471-485: Clarifying questions. Is the low fraction of the speciation of the organic aerosol limited to not finding a match in the NIST or does it highlight a problem with the analytical method? In addition, how sensitive is the fraction speciated to the use of the filter media, i.e., better with PTFE versus quartz? 3. The composition section could benefit from findings from some recent publications that have studied SOA from biomass burning emissions or precursors, e.g., He et al. (ESPI, 2020) – alkylfuran mixture, Joo et al., (ESC, 2019) – 3-methylfuran, Ahern et al. (JGR, 2019), Akherati et al. (ES&T, 2020), and Lim et al. (ACP, 2019) – biomass burning SOA in laboratory experiments with an emphasis on understanding phenolic, furanic, and monoterpene VOC contributions to SOA. 4. Figure 7: Was total organic mass in the gas- and particle-phase measured another way, e.g., FID-gas, Sunset OC/EC-particle, to get mass closure? 5. Figure 8: Mention sample size for each fuel. Specify measurement uncertainty when $n=1$. Combine measurement uncertainty and experiment-to-experiment variability when $n>1$. 6. Figure 9: Was the gas/particle partitioning of PAHs analyzed further? Seems like an ideal dataset to study absorptive partitioning. 7. Lines 631-632: Why were certain samples fully speciated and others not very much? Was this relationship examined further with respect to its sensitivity to fuel, total organic mass

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captured on filter/disc, other variables?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-860>, 2020.

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