Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-892-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Open Access

EGU

ACPD

Interactive comment

Interactive comment on "Emissions of non-methane volatile organic compounds from combustion of domestic fuels in Delhi, India" *by* Gareth J. Stewart et al.

Anonymous Referee #2

Received and published: 10 November 2020

In this paper, Stewart et al. present emission factors (EFs) for non-methane volatile organic compounds (NMVOC) produced during residential solid fuel combustion for heating and cooking in Delhi, India. Common cooking fuels were collected from across New Delhi and burned under controlled laboratory conditions. NMVOC emissions were measured using multiple gas chromatography based systems and a proton-transfer-reaction time-of-flight mass spectrometer. Species-specific and total measured NMVOC EFs are reported in the paper/supplement for each fuel type burned and the results are discussed in the context of similar laboratory studies of residential biomass burning emissions. The authors find that for most fuels oxygenated NMVOCs account for the largest proportion of the total NMVOC emissions. Additionally, they

Printer-friendly version

Discussion paper

report that the speciation and total measured NMVOC emissions vary widely between different fuel types, highlighting the need for a more complete understanding of residential biomass burning EFs. The study focuses on fuels that are specific to India and New Delhi, making their results relevant to local and regional chemical models. Emission factors from this study are also more broadly applicable to other regions where residential solid fuel combustion is used as the primary means of cooking and heating.

This clearly written manuscript addresses an underrepresented area of biomass burning emissions with a very comprehensive NMVOC EF dataset. I particularly appreciated the inclusion of LPG emissions to demonstrate its potential as a 'cleaner' alternative. I recommend this paper for publication after addressing the minor issues discussed below.

1) My main concern is the representativeness of the burning chamber used in this study to the common stoves used in residential settings. Although the chamber description is referenced, I feel that it is important for this study to include a more detailed description of the stove/combustion chamber itself along with how it was operated to replicate real-world conditions as the combustion effieciency is well known to influence NMVOC EFs.

2) There is very little discussion about the error associated with the reported EFs beyond that associated with each instrument, nor is the EF variability between repeated burning experiments of similar fuels included. For example, the mean EFs in the supplement and Table 2 should be associated with the fire-to-fire variance, such as the standard deviation of the burns. Similarly, what is the error associated with the stack flow-based method for determining EFs?

3) It is unclear whether CO and CO2 were measured during the experiment, but if available their inclusion as EFs and MCEs for each burn would greatly help anchor this study in the context of NMVOC emission literature as the authors discuss in the conclusion.

Additional comments by line:

110 – Add missing word: due 'to' vehicular emissions.

198 – Repeated word 'given'.

414 – Should this be referencing S4? S2 appears to show how EFs were calculated. Additionally, S2, describing how EFs were calculated, is not referenced in the text and should be added. This also goes for the tables in supplement S4, they should be referenced in the text and would benefit from having the individual tabs labeled (Sx).

498 – Discussion of total emission factors would be more accurately discussed as total 'measured' emission factors as the techniques used in this study likely miss a portion of low volatility species, which could be lost in sample inlets and chromatography columns.

502-508 – It is unclear if you included the GC measurements in your total emission factors from this discussion. Is that the purpose of discussing proton affinities here? If so just state that alkanes and alkenes measured by the GC's were included in the total EF as appears to be described in S3.

509 – Should this be S3? Or maybe relevant to both S3 and S4 'EF g kg' tab?

512 – Include the mass of benzaldehyde.

544 – Stockwell et al. (2015), which the authors compare results to, define IVOC/SVOC as species with a molecular weight greater than toluene. Is there a reason the authors instead choose to define IVOCs as those with molecular weight greater benzaldehyde?

587 – Figure 9A – The different studies in the plot are very difficult to distinguish and it is unclear what the authors are trying to convey with it due to the potential comparison of unrelated EFs. For example, what does the inclusion of 'all species reported in review and comparable studies' include? Are wood emission factors from this study compared to garbage burning or peat EFs for Stockwell et al. (2016)? Are EFs from

western US wildfires (Liu et al., 2017) and southeastern US agricultural fires (Muller et al., 2016) relevant to this work? Further, EFs also vary between fuels due to differences in combustion efficiency (e.x. dung, peat, and trash will smolder more than wood) whereas this figure implies fuel type is the only difference. It would make sense to have this as a more direct comparison between related fuels (i.e. just literature fuel wood, cow dung, etc.).

650-657 – As mentioned, if available, this study would also greatly benefit from reporting CO, CO2, and MCE values for each burn. As the authors state, this would allow their EFs to be evaluated based on the impacts of MCE. Additionally, reporting MCE would allow these results to be more accurately compared to other studies, while CO and CO2 are themselves important inputs for climate models.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-892, 2020.