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1. There is a wide diversity of stove and fuel types globally and the stove and fuel types explored here are but a small fraction of those used in real world. So while the gas-phase speciation offers a detailed view of the emissions from these stove-fuel sources, how are the stove-fuel sources in this work representative of the stove-fuel combinations in India and globally? More specifically, what fraction of the gas-phase emissions from cookstoves come from the stove-fuels described in this work? And depending on the answers to the previous question, how can this speciation, if at all, be used to inform the gas-phase emissions speciation in large-scale atmospheric models?

The objectives of the current paper are not to produce representative values for global emissions. Stockwell et al., 2016 and this work are the most comprehensive measurements to date in producing speciated gas phase emission factors. The stoves and fuels measured in the current study are predominant in the Indo-Gangetic plains. As a part of this collaborative project, the Seinfeld group at Caltech is modeling secondary organic aerosol formation using CMAQ to describe rural North India.

2. Since multiple tests were done with each stove-fuel combination, were other important variables recorded and/or controlled during the test? For example, fuel moisture content, environmental conditions (e.g., temperature, relative humidity), fuel size and burn rate, cooking pots, meals cooked. According to the authors, are any of these variables important in explaining the variability? Citing relevant literature on the factors affecting cookstove emissions variability would be helpful.

Moisture content of the fuels did not correlate with emission factors; in all cases the $r^2 < 0.3$. In one case, we did see a negative relationship between emission and moisture content (Benzene, dung-angithi, $r^2 = 0.718$). We did plot the emission factors as a function of fuel moisture content for a fraction of compounds as examples shown below. We found that meal cooked is not a significant variable ($p > 0.05$). We added the recorded data for each cook fire to the supplemental information, including variable information and emission factors so the dataset is publically accessible. The following statements were added to make this clear to the reader.
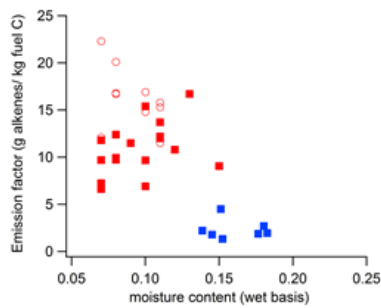
"Fuel moisture content, fuel mass burned, and meals cooked were noted for each cook fire, and can be found in the supporting information." *(Page 3)*

"There are many factors that may lead to variability in biomass burning emissions including pyrolysis temperature (Chen and Bond, 2010), fuel moisture content (Tihay-Felicelli et al., 2017), and the wind speed/direction (Surawski et al., 2015), among others. Relationships between emissions and fuel moisture content (Figure S1) or meal cooked were not found to be significant for any compounds (p< 0.05). This paper focuses on the relationships between emissions and fuel-stove combination." *(Page 6)*
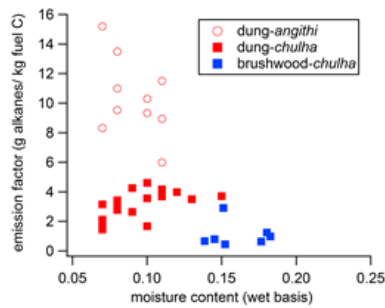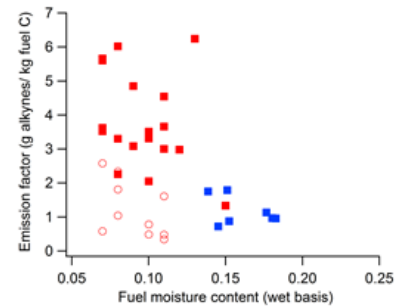
a) Total alkanes
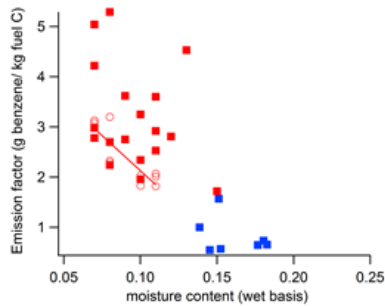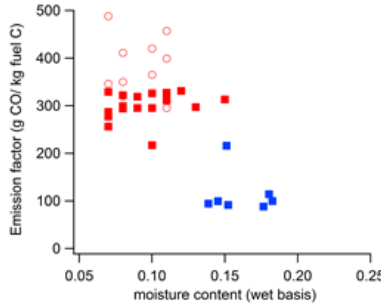**b) Total alkenes**
c) Total alkynes
d) Benzene
e) Carbon monoxide

3. Some more detail on the fuels and stoves for the less informed reader would be helpful. What animals was the dung from? Presumably cow? What is an Angithi stove? What is a chulha? How are these different? What is brushwood? Is the brushwood from a particular plant? The use of pictures could help.

The following was added to the introduction, to explain the hypothesis that these stove will lead to different emissions.

"The former is a primarily flaming stove with generally higher modified combustion efficiencies or concentration ratios of carbon dioxide to the sum of carbon dioxide and carbon monoxide (average dung-*chulha* 0.865), used to cook village meals. The *angithi* is largely smoldering with lower modified combustion efficiencies (average dung-*angithi* 0.819) and is primarily used for cooking animal fodder and simmering milk."

The pictures of stoves and the kitchen are already published in Fleming et al., 2018, but the reader is referred to this publication for more extensive information about the materials used in the cook fires. The following sentences were added to the experimental methods section.

"Animal fodder simmers upon smoldering dung in a clay bowl, referred to as an *angithi*. *Chulha* stoves are made from bricks and a covering of clay, and the availability of oxygen from the packing of biomass fuels results in primarily flaming combustion. The *chulha* is used to cook most meals for the family in this village. Buffalo and cow dung patties and brushwood, in the form of branches and twigs, were used in *chulha* stoves, and for the 13 mixed fuel cooking events dung and brushwood were combined in a ratio determined by the cook's preference."

4. In equations 1-3, how are mT and mT,c estimated/calculated?

"Fuels were weighed before they were burned, and the dry mass was calculated based on moisture content measurements. The ash was weighed after the cooking event and subtracted from the dry mass of the fuel giving the net dry fuel burned for the cooking event, m_T. When mixtures of dung and brushwood were used, both were individually weighed to more accurately determine the carbon mass burned. The fraction of carbon in the fuel used to yield $m_{T,C}$ was taken to be 0.33 for buffalo dung and 0.45 for brushwood fuels based on Smith et al. 2000."

5. Page 4, line 11: What is the limit of detection and limit of quantification for the filter measurement? The 0.75 µg blank seems quite low.

We determined the method detection limit to be 9.3 micrograms based off the standard deviation of the field blanks multiplied by 3. All masses for the sample and background filters are above this limit of detection.

6. Page 5, line 1: I am not sure what the point is of normalizing the SOA production from a species to that of toluene? Why not report the SOA production in absolute values of g/kg-fuel when the presentation of results in Figure 2(c) is done in relative format anyways?

The SOAP value for a particular VOC, as described by Derwent et al., 2010, is the change in SOA mass concentration when this VOC is introduced into the photochemical transport model, divided by the change in SOA mass concentration for when toluene is introduced. We use these SOAP values published in Derwent et al., 2010 to determine SOA forming potential for each quantified VOC. We are simply inheriting these data from the literature to estimate the relative magnitude of the effects, with the hope that more detailed modeling work would follow. The dataset is publicly available, so if one wished to generate absolute g SOA mass/ kg fuel burned by estimating the SOA yield for toluene in the plume, this is possible.

7. Why was the maximum incremental reactivity approach used to determine the ozone potential? If an alternative method was used (e.g., MOIR, EBIR), do the findings change?

Maximum incremental reactivity was chosen because a VOC limited regime gives us the highest sensitivity in ozone generation to cooking emissions. Carter 1994 concludes that the MIR scenario, since it is based on integrated ozone production rather than maximizing peak ozone levels (MOR scenario,) is generally less dependent on exact NO_x inputs. This high-NO_x scenario might be more realistic as well since we are focusing on smoke plumes. A sentence was added to the experimental section 2.7 summarizing this explanation for the use of the MIR scenario.

8. Can more details about the SOA formation be added? Were the SOA yields for low or high NOx conditions? What OA mass concentration as the absorbing mass was used to determine the SOA yield? Were they corrected for vapor wall losses? NOx and vapor wall loss corrections are species dependent (see Zhang et al., 2014) and may change the apportionment shown in Figure 2(c).

The photochemical trajectory model is outfitted with the Master Chemical Mechanism (MCM 3.1), which dictates gas-phase reactions and SOA formation. NO was initialized at 2 ppb, and NO₂ to 6 ppb. This is now described in section 2.5.

9. Page 7, lines 3-21: Are the brushwood results in this study comparable to the hardwood results from Stockwell et al. (2016)? If yes, why? It is unclear what the point of the comparison to the Stockwell study is since the manuscript only has a few sentences on this comparison. Is it just to show that the emissions in this study were lower than those in Stockwell et al. (2016). The explanations offered for the lower emissions was not satisfactory. Did the authors try to compare the emissions on a normalized basis with each other? Do they correlate?

Stockwell et al. 2016 and this paper are the most comprehensive gas-phase inventories to date in terms of cook fire emissions. It made sense to the authors to cross-compare the results. "Hardwood" and "brushwood" emissions appear to be more similar to each other than emissions from dung-based fuels. There are some real differences in emissions from cook fires between the studies, and we do our best to explain in the text these differences for the reader to compare. In S2.1 we plot the EFs in Stockwell et al. 2016 against those reported in our paper. In the review process, we've learned more about how the laboratory EFs were adjusted to field observations in Stockwell et al., 2016, and we believe these adjustments may have contributed to the discrepancy in the reported EFs. We've added Figure S2.2 where we plot the unadjusted emission factors in both studies, and there is an encouraging degree of agreement in the actually measured (unadjusted) values.

10. Section 3.3 for SOA: Was there an estimate for emissions of total non-methane organic gases (NMOG)? What fraction did the speciated compounds account for of the total NMOG? Were any intermediate volatility and semi-volatile organic compounds targeted? What fraction of the NMOG was unspeciated? What implications does this unspeciated fraction that may include lower-volatility vapors have for SOA formation?

We agree that there are semi and intermediate volatility compounds that were not measured in this study. Many of these will contribute to SOA and ozone formation. We did not attempt to quantify NMOG. This was not possible using our carbon balance method and an unknown dilution factor. We have added a "disclaimer" to the manuscript in the "SOA formation potential" section, printed below.

"We would like to emphasize that the SOAP-weighted emissions are reflective of only the measured VOCs, and there are likely semi-volatile and intermediate volatility compounds that are not measured but also contribute to SOA formation."

11. Section 4: Could atmospheric implications for SOA formation also be determined for this village similar to those for O3? How would the SOA formation compare to the primary PM2.5 emissions?

Unfortunately the SOA forming potentials generated using Derwent et al. 2010 only give relative amounts of SOA formation. From this analysis we concluded that a dung-chulha cook fire would produce almost 3 times more SOA than a brushwood-chulha cook fire. However, with this methodology we cannot compare the absolute magnitudes of primary and secondary emissions. Further work on addressing this exact question is in progress with the Seinfeld group at Caltech to model SOA formation in a simulated village.