Comments by reviewer #1 are reproduced in the sans-serif font below. Our responses follow each comment in a blue, italicized, serif font. Text additions to the manuscript appear in the manuscript in red color. Deletions from the manuscript are described in the responses below.

In addition to the changes triggered by reviewer comments, other changes were made to improve readability. The significant additional additions or changes also show up in red color in the manuscript.

Pg 2, line 21: Please give a brief description of each stove type so that the reasons behind the differences in emissions are clearer.

The text was amended to clarify what the stoves are generally used for and the primary combustion type. Additions to the text are highlighted in red. The authors agree that these additions will assist the readers in making sense of the different emissions from the stoves that are described later.

"This time, we measured emissions in field conditions from two traditional, locally-made cookstoves, the *chulha* and the *angithi*. 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."

Additional text was added on page 3.

Pg 3, line 1: Was the dung:brushwood ratio known for each mixed fire to accurately estimate fuel C content?

Yes it was. The following sentence was added to clarify this point.

"When mixtures of dung and brushwood were used, both were individually weighed to accurately determine the carbon mass burned."

Pg 4, line 12: "The background filter mass was adjusted to match the flow rate of the sample filter by assuming the flow rate is proportional to the filter mass." This sentence is confusing. How would mass "match" a flow rate? Are the authors saying the sampled volume of the background filter was not always the same as that of the PM sample and therefore the background mass was scaled up or down according to the ratio of the sample volumes? Also, in Figure 1, the flow rate of the background filter is nominally the same as the PM filter, so were these marginal adjustments? Please rephrase this sentence.

We agree this was confusing as written. We clarified that Teflon A (sample) and Teflon C (background) of Figure 1 were reserved for gravimetric analysis, whereas Teflon B was not used in this paper. With this it becomes clear that the background mass needs to be adjusted for flow rate, which is now described as matching sampling volumes in the text. We also explicitly state our assumption that the amount of PM collected on the filter is proportional to the sampling flow rate through the filter.

Pg 7, line 12: In the comparison of EFs between this work and Stockwell et al., some consideration should be given to MCE. MCE could account for some of the EF differences for brushwood given the higher MCE of this study compared to Stockwell, although it is unlikely to explain the differences for dung. Is there sufficient information in Stockwell et al. (2016) to calculate EFs in g/kg C that could be overlaid on Figure 3 (or use g/kg in figure 3)?

Thank you for making this suggestion. We plotted their raw EF data in the supplemental information (Figure S2.2). There is more agreement in our datasets than it first appeared. This spurred a conversation with the authors of Stockwell et al., 2016 during which we learned that much of the discrepancy could come from adjusting their laboratory EFs to better match field observations. We also added text to explain this in the paper (page 7-8).

Section 3.2: The overall discussion section would likely flow better if the MCE section is moved earlier, as most of the preceding discussion requires consideration of MCE.

We believe that the emission factors are the most impactful contribution of this manuscript. Therefore, we decided to show first the emission factors in the chemical composition section. The statement that emissions generally have an inverse relationship with MCE is common knowledge, and does not warrant extensive discussion at the beginning of the paper. Rather, we focus on compounds/compound classes that have interesting relationships with MCE.

Pg 7, line 28: In Figure 3b, the EFs from the chulha stove appear to follow a clear linear trend with MCE regardless of fuel type. In contrast, emissions from the angithi stove show no apparent trend with MCE. The benzene case is also similar. Could these "complicated" MCE relationships be due to differences between the two stoves? The authors should discuss the different MCE trends observed between the different stove types.

We agree with this interpretation, and convey that the fuel-stove combination is important for determining emissions (not just MCE) in the second paragraph of the MCE section 3.2. A sentence was added to emphasize this point.

"Knowledge of the cook fire MCE alone is not sufficient to determine the EF of ethane. Combustion specific to the fuel-stove combination is a significant factor in cook fire emissions."

Pg 7, line 30: Alkyne emissions from the chulha stove clearly show increasing EF with decreasing MCE (negative slope). This contradicts the discussion on pg 6, line 34 that alkynes are predominantly emitted from flaming combustion, which would show a positive EF vs. MCE slope. Please reconcile those two points.

At lower MCEs there is a positive slope, this is largely encompassed by the dung-burning stoves. As there is more flaming combustion, we do in fact measure higher emissions of alkynes. However, it is well known that overall emissions are lower at higher MCEs. If one compares emission factors from ethane, alkenes, and alkynes in Figure 3, alkynes are highest at MCEs>0.95.

Pg 8, line 36: "aromatics make up on average roughly 95% of SOA precursors for all cook fires." This is not at all surprising considering that nearly all of the other compounds measured are either too light to contribute significantly to SOA production or don't have a reported SOAP value (Table S3). A simple disclaimer is warranted stating as such and that the contribution of aromatic hydrocarbons to SOAP here is likely an upper limit depending on the composition of the unmeasured fraction of VOCs in cookstove emissions. The authors may additionally want to compare to Stockwell et al. 2015 (Atmos. Chem. Phys., 15, 845-865), who measured laboratory cooking fires using PTR-TOFMS and observed many other compounds that could act as SOA precursors.

While Stockwell et al. 2015 did measure EFs from laboratory cook fires using PTR-TOF-MS, we chose to focus on Stockwell et al. 2016 because they use similar fuels and some of these measurements are in the *field. We do acknowledge that we did not quantify every SOA precursor which could add up to a significant amount of SOA. We clarified that the 95% comes from only species measured in this study on page 9.*

Pg 9, lines 7 and 12: Do these predicted ozone mixing ratios represent the excess ozone produced from only cooking fires? Clarify the text.

This was clarified by adding phrases "due solely to cook stove use", "from cooking.", and "excess" in front of the predicted ozone levels.

Section 4: The structure would make more sense if the paragraphs were swapped.

We chose to keep the order of the paragraphs in the "Atmospheric implications and conclusions" section. To us, the first paragraph gives us an implication for the work, which is the quantification of ozone production in a village from cooking. Then the last paragraph holds take home conclusions for the readers, the order given in the subtitle.

Technical corrections:

Pg 2, line 14: typo in 'Alternatvely'

Thank you for catching this. It is now spelled correctly.

Pg 2, line 17: typo in "a simulated village houses".

The grammar was corrected.

Equation 1: It's redundant to use the summation sign and '+' signs.

Equation 1 was fixed.