

# ***Interactive comment on “Aerosol emission factors from traditional biomass cookstoves in India: Insights from field measurements” by Apoorva Pandey et al.***

## **Anonymous Referee #1**

Received and published: 7 July 2017

This manuscript presents emission factors for three types of solid fuels from India based on real-world tests and associated chemical speciation analysis. The results are novel, and provide relevant information on EFs related with indoor combustion in the Indian (and the broader South Asian) context. The manuscript is written well, and provides succinct information on the sampling methods and previous work on the topic. The observations are described well, but the results are not discussed in detail.

Please include a brief description of the sampling location in the methods section- dimensions of the room where sampling was conducted, ventilation type (or lack thereof) in the sampling area, and representativeness of the sampling location.

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Lines 115-118: Please include limits of detection for carbon analysis using IM-PROVE\_A protocol, and average particle mass based on gravimetric analysis.

Each emission test included data on PM (size distribution, real-time PM data with Sidepak, sensors), and gases (CO, CO<sub>2</sub>) in addition to filter samples, but none of the data is discussed in the manuscript. Please consider including the profile of real-time emissions for a sample test for each fuel. Also, include average particle size, or representative particle size distributions for the different regimes (ignition, flaming and smoldering).

With respect to the differences between EFs for Bihar and UP dung, is it known if there are differences in composition of dung cakes?

Figure 5: It is interesting to note that the carbon profile for the present study is very different from the results in the CPCB (2011) report. It would be beneficial if the authors could comment on the reason for the differences (methods, sample type or something else?). Also, what could be causing OC<sub>3</sub> fraction to contribute nearly 50% of the total OC mass? Please elaborate.

Based on the observations from this analysis, what are the implications for the real-world? Are we able to derive substantial conclusions that could be relevant for mitigation policies, or exposure reduction measures?

Lines 154-157: Does this mean that one household could use different fuel types depending on the type of cooking activity being undertaken?

Figure S1: Using a plot of PM concentrations from the optical sensors will be much more useful compared to the raw signal data currently included in the supplemental data. Was a secondary check performed to compare average concentration of Sharp sensor for each cooking event with gravimetric filter measurement or Sidepak measurements?

Minor comments:

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Figure 2: Please change the resolution. The image gets blurred if the reader zooms in (same problem in case of Figure 4).

Revise the manuscript - words and/or punctuation marks are missing at some places (e.g., Lines 77, 79 and 138).

In the supplement, the sentence “MCE is typically treated as an identifier of . . . . . ” seems to be incomplete. Please edit.

Line 89: The thermal fractions “found in” rather than “found it”

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-291>, 2017.

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