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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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In the manuscript "Aerosol emission factors from traditional biomass cookstoves in India: Insights from field measurements" the real-world emission factors of PM components from different biomass fuels in food cooking are studied in an Indian household. The findings increase the understanding of real-world PM, OC, and EC emission factors for a wide range of combustion of solid fuels from different regions. The results are compared with previous findings in standardized tests, and in general, the results of the manuscript show higher PM emissions and OC/EC ratios existing in the literature. In a big picture, the main findings are novel and significant enough for an ACP paper but the quality of presentation needs improvement (major revision). On the positive side, I like the title, abstract and conclusions parts of the manuscript.

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Discussion paper

Specific comments

Table 2 lists the experiments performed, indicating the number of reps and the foods that were cooked. The variation of the foods cooked was a bit random; apparently, the family was just living ordinary life. In the analysis, the food is not considered as a factor instead it is just causing deviation to different EFs for each fuels. Authors should analyze the role of the food cooked while discussing the results. Is the food itself emitting something, or is it only so that different foods require different amount of heat or time?

Authors have used several instruments in the tests: PAMS, Sidepak analyzer, gas analyzer, minivol sampler and wireless optical particle sensors. First of all, locations of wireless optical sensors are not shown in Figure 1. Also it is not clear why minivol sampler could not use the “eight armed sampling probe” and adjacent tubing to be in parallel with other instruments. Figure 1 would be more informative if the whole room or house would be also sketched to have an idea how the exhaust flows in the space. I assume there is not a dedicated stack for exhaust gas ventilation.

The authors have corrected values from different sampling locations using wireless sensors and correction factor of about 1.04 (data shown in Figure S1). About half of the time both sensors were showing maximum concentrations, so that time they were not showing any meaningful results. Was the maximum concentrations also included in the regression analysis? I see that at many times the difference between sensors was much more than 4%. Also you should include all the experiments with wireless sensors to this regression analysis.I think getting this factor right is very crucial thing because gas analyzers measured from different location than the minivol sampler. It would be also interesting to see how gas concentrations change during combustion/non-combustion phases to see how much is the increase in CO and CO2 concentrations in the measurement locations. Authors could include a plot of this in the supplementary information.

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Discussion paper

The manuscript in current form presents only data from filter collections (Minivol sampler) while the authors also list other instruments that were used. For instance, authors should show time series of some individual test how the PM emission behaves during different phases of the combustion. Overall the role of combustion phase is highlighted in the results. Authors should also show and analyze particle size distributions measured with the PAMS instruments. This would help to e.g. identify whether the PM mass is in the particle size range of PAMS or above it.

The results are focusing on the effects of combustion phase and fuel quality of the EFs. The discussion and conclusions of the results are so far in form where authors just focus on the measured values. The cause and effect analysis is much lacking in the current version of the manuscript. For instance, why different phases of combustion show different results? Why are the deviations in results such high? The authors should dedicate more effort in this.

Minor remarks

Figure quality. You should remove all borderlines from the figures. Font quality is bad in Figures 2 and 4.

Row 79. Full stop apparently missing.

Rows 131-133. You basically don't have to use value of 2.4% here since you have measured OC/EC yourself, or you can also compare the amount of non-CO or non-CO2 carbon based on your results to this value.

Row 135. "Figure 2 compares EFs for the different fuels." Are these EF's from the "whole burning event" including all the phases from combustion.

Row 156. "is used". Should this be "was used"?

Figure 5. I imagine you could have all the bars in the same plot.

Row 185. "Fuel-wood and dung EFs. . ." You could rephrase this to be in more accurate

form, it is not emission of fuel-wood or dung after all.

Row 188. OC3 does not look to be more than 50% in Fig. 5.

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