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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Impacts of Household Sources on Air Pollution at Village and Regional Scales in India" *by* Brigitte Rooney et al.

Anonymous Referee #3

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This manuscript presents a multi-resolution modeling study of ozone and particulate matter in India, with a focus on the impact of residential combustion on ozone and particulate matter concentrations. The authors apply the CMAQ model at several different horizontal grid resolutions for several different time periods in 2015/2016. They then analyze the total concentrations of ozone/PM2.5 as compared to observations and examine the contribution of residential combustion of sources for cooking to the total PM2.5 and total SOA. Overall, the manuscript is generally well written. However, I agree with the points made by the other anonymous referees, and concur that the manuscript would benefit greatly from some major revisions. I will not repeat the general suggestions made by the other referees here, only to say that I agree with their major points. I will however provide some specific comments in addition to those made

by the other reviewers.

General Comments: Very briefly, I agree that the authors spend a lot of time discussion previous work upfront. It would be nice more details of their own analysis could be provided and then compared to the previous studies as appropriate. More details and analysis of both the WRF and CMAQ simulations is required, particularly the WRF simulations. This point was made by the other referees, but I think it is necessary to reiterate here the importance of providing an analysis of the performance of the WRF simulations. Hopefully there are observations available to provide such an analysis. Giving the reader a clearer picture of the strengths and weaknesses in the WRF simulations would be very beneficial to supporting the analysis of the CMAQ simulations and the conclusions the authors are attempting to make. Related, there are large biases in the CMAQ simulations that at bring into question some of the conclusions made by the authors, particularly those attributing percent contribution of residential cooking to total PM2.5 and total SOA. Providing a clearly picture of the CMAQ model performance would help support these conclusions (I refer the authors to the comments provided by the other referees). The authors should provide summary statistics (e.g. RMSE, bias, correlation) of the various simulations.

Specific Comments: Page 7, line 30: How different are the profiles for wood and dung? Why not use a combination of the two if the percent contribution in a grid cell is known?

Page 8, line 21: This appears to be sentence fragment to me. Please fix.

Page 10, line 26: As mentioned the other referee, it's a strange setup to be using the same month/day of the GEOS-Chem simulation to support the various CMAQ simulations. Why was this done?

Page 11: What input data did the authors use to drive the WRF simulations? Reanalysis data (if so, what was the horizontal resolution)? Were the same data used for all three horizontal domains? How many vertical layers were used in the WRF simulations, and what was the height of the lowest layer? What Cumulus Parameterization

(CP) scheme was used? Was a CP scheme applied for all the simulations?

Page 11, line 18: What version of MCIP was used? Also, please provide a relevant reference for MCIP.

Page 11: What was the CMAQ configuration (e.g. vertical layers, mixing scheme, etc.)? As mentioned by the other referee, it does not appear that a spin-up was applied. I agree with the referee that it would be beneficial to provide a spin-up period, even if not starting from profile ICs.

Page 15, line 25: As mentioned by the other referees, the author's statement that the "overall comparison of predictions and observations would appear to be driven by the accuracy of the meteorological fields" is on point, however no analysis of the accuracy of those fields is provided, so the reader has no idea what role the meteorological fields are actually playing in the analysis. So, these comparisons need to be provided since they are no doubt very important to the analysis and conclusions the authors are making.

Page 16, line 13: The statement that "The closeness of the 4 km and 1 km simulations reflects the closeness of the respective inventories" seems to only capture part of the story. What about differences in the meteorological performance between the two simulations? It would seem that the reader is intended to assume that the meteorological fields are also very similar between the 4km and 1km simulations. Is that really the case?

Page 19, lines 5-20: I'm confused whether the authors are describing new results from their own analysis or simply summering the results of Fleming et al.? Please clarify whether the statements are new results or simply summary of results from Fleming.

Page 20, line 10: The statement that "overall good agreement between observed and predicted levels O3 levels" is a bit generous. Based solely on the time-series plots and without any summary statistics, I think the authors at best could call the results rea-

sonable. There are certainly frequently very large biases that make calling the results "good" difficult to support. However, I believe that even reasonable results are sufficient to support the conclusions the authors are making regarding ozone performance.

Page 21, line 5: The statement regarding the importance of replacing household combustion devices with modern technology is not strongly supported by the analysis presented. If anything, it would seem that reducing the impacts from agriculture burning would go much further in improving people's exposure to PM2.5 in this region. That is not to say that improving technology is not beneficial, it's just not the message I think that is delivered by the analysis as it is currently presented.

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