

# ***Interactive comment on “Space-time variability of ambient PM<sub>2.5</sub> diurnal pattern over India from 18-years (2000–2017) of MERRA-2 reanalysis data” by Kunal Bali et al.***

## **Anonymous Referee #1**

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Air pollution, especially PM<sub>2.5</sub>, in India is a hot topic. However, studies are hampered by limited availability of data in India. As correctly pointed out in the Introduction that satellite or ground-based measurements have limitations in temporal continuous or spatial coverage. This paper analyses long-term reanalysis MERRA-2 datasets of PM<sub>2.5</sub>, tries to overcome the limitations and perform analysis of the space-time variability of surface PM<sub>2.5</sub> over India during 2000-2017. However, as pointed out in the pre-review by the two referees, the datasets (and quality control) used in this study are not detailed introduced. In this ACPD version, I see little improvements of the description/discussion of the datasets, quality control and validations of the used model results, and representativeness of the observations. I feel less confident about the

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drawed conclusion, if the authors do not understand that how are the datasets produced, how is the quality controlled and what are the limitations and representativeness of the data used. I would recommend this manuscript for publication in ACP only if the following concerns can be nicely addressed.

1) Satellite observed AOD550 with MISR, MODIS and AVHRR, plus ground-based AOD data from AERONET were assimilated in the MERRA2 reanalysis dataset. However, all these datasets are column parameters. I understand the assimilation of these datasets can improve the radiative forcing simulation directly. But, how does the assimilation of column parameters improve the simulation of surface PM2.5 concentration in MERRA2? This is not discussed in the paper. More discussion (preferable with some quantifying values) about this would provide more confidence of the model data used, especially for India where very limited surface observations are available or assimilated.

2) Following the above comment, I think the impact of AOD assimilation on surface PM2.5 concentration would be strongly depended on PBL simulation, which is also a key topic analysed in this study. However, how is the PBL simulated and how is the top of PBL defined in the MERRA2; what kinds of meteorological datasets are assimilated in the model to improve the PBL simulation over India; and how good is the performance of the PBL in MERRA2 compared with observations or improved by assimilation, . . .etc? All kinds of these questions are not discussed in the paper.

3) I agree with the second referee that most of the CPCB monitoring are in the urban area: the representativeness of CPCB dataset needs to be carefully discussed. Due to CPCB observations possibly represent the urban condition, how suitable they are for direct comparisons with the MERRA2 global model results with relatively coarse resolution? Based on this thinking, I am kind of agree with the second referee that “the bias correction/calibration methodology is overfitting the model data”. This question was not discussed in the ACPD version, I feel we need to think about it more carefully in the next revised version.

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4) Furthermore, based on my limited experience with CPCB dataset, it seems the quality of PM<sub>2.5</sub> concentration from CPCB is questionable. How is the CPCB dataset quality controlled, this question was raised by the second referee, however, still not addressed. There are some Indian cities have PM<sub>2.5</sub> observations from US diplomatic missions, which are generally believed to be of high quality and could help with quality validation of CPCB observations over these cities.

5) MERRA2 dataset is simulated with EDGARv4.2 global emission inventory, as described by the paper. I suppose the EDGAR inventory for year 2012 (the latest one) was used. How well the 2012 inventory represent the condition of the period 2000-2017? As reported by lots of studies, between 2012 and 2017 the Indian emissions have changed a lot. And as described by the MERRA2 aerosol dataset developer (Buchard et al., 2017) that assimilation cannot correct for deficiency due to missing emissions. The uncertainty of emission inventory would propagate to the MERRA2 reanalysis data. And biomass/agriculture burning is believed to be a large contributor of surface PM<sub>2.5</sub> over India. How is this burning source considered in the MERRA2 simulation?

6) As described in the paper that OC are secondary aerosols in MERRA2 dataset. I would like to know how is the secondary organic aerosols simulated or represented in the GEOS5/MERRA2 model. Since, OC contributed about half of fine particles mass in Delhi (possibly other IGB regions as well) based on recent observations (Gani et al., 2019). The correct simulation of OC secondary formation processes would be critical for the accuracy of MERRA2 aerosol dataset. Some comments on the validation of OC simulation within MERRA2 would be helpful.

7) Some technical correction: a) line 128. 'three different sensors' should be four sensors in total if count AERONET monitoring as well. b) line 108. I feel the word 'propose' might be inappropriate. This assimilation/reanalysis approach has been widely used over other regions, the contribution of this study is used a reanalysis dataset to analyse the spatial-temporal variation of surface PM<sub>2.5</sub> over India.

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