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# *Interactive comment on* "Monthly and Spatially Resolved Black Carbon Emission Inventory of India: Uncertainty Analyses" *by* U. Paliwal et al.

**Anonymous Referee #2**

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I have only good words to say about this article. It is one of the easiest articles I have ever read and it flows very well for the reader. The science behind is novel and it would be a great tool for any modeller wants to include emission data from India to any transport, chemistry or climate model.

1) I am sceptic about the methodology used to create this spatial resolution, although the input was at a district level, hence, in course resolution. However, if we exclude inverse modelling approaches that would be able to create a fully resolved inventory, I admit this approach is the best we have.

2) Another problem is the emission factors used in the study. I expect that the authors will justify more their choice of emission factor selection, since there are already-known and validated emission factors in the literature used for such purposes (showed also in



Table 2). I am not sure if taking the mean emission factor would make sense since in some cases it varies a lot. In addition, I do not think that emission factors taken from Andrae and Merlet's paper (used in most models) can be given the same weight with other emission factor studies. Of course, since they do not validate their dataset, they do not need to evaluate the emission factors they used.

3) I think that a missing point is evaluation of the database created from this work. I would highly recommend to the authors to try including this dataset to a global inventory and perform a few runs comparing the surface concentrations of BC from their model and the dataset with observations in Southeastern Asia. There are several measuring stations there and such a comparison would help all of us understand how valid this dataset is. At the moment, a poor discussion is performed for such an interesting topic. I believe that including the dataset to a model and performing the aforementioned analyses would expand the discussion a lot.

4) My highest concern is if the present manuscript falls within the scope of the journal. According to the main page of the journal, it is "dedicated to the publication and public discussion of studies investigating the Earth's atmosphere and the underlying chemical and physical processes in an altitude range from the land and ocean surface up to the turbopause, including the troposphere, stratosphere, and mesosphere. The main subject areas comprise atmospheric modelling, field measurements, remote sensing, and laboratory studies of gases, aerosols, clouds and precipitation, isotopes, radiation, dynamics, biosphere interactions, and hydrosphere interactions. The journal scope is focused on studies with general implications for atmospheric science rather than investigations that are primarily of local or technical interest. In my opinion, the present study comprises a rather statistical methodology than a modelling, measurements, lab-based methodology (perhaps a bit of GIS) that the journal requires. Furthermore, without the evaluation using model-simulated concentrations and comparison with measurements of the Southeastern Asia, the study is of local interest.

A general comment is that the Editor should have probably rejected the present manuscript in its current form. I have seen several more subject-related manuscripts being rejected. However, I am glad he did not and the authors have the chance to provide a very useful database to the modelling community (after evaluation of their results, of course).

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