

Interactive comment on “Assessment of co-benefits of black carbon emission reduction measures in Southeast Asia: Part 1 emission inventory and simulation for the base year 2007” by Didin Agustian Permadi et al.

Anonymous Referee #1

Received and published: 18 June 2017

The authors developed an anthropogenic emission inventory based on various published emission data including their own. With the emission inventory, the WRF model simulation has been conducted for the year 2007 and the results are compared to in situ observations of PM₁₀, PM_{2.5} and BC concentration from various stations in Southeast Asia. Methodology and analysis are sound in general, but the authors mostly focused on comparing above-mentioned variables while meteorology components are almost completely excluded in the discussion on simulated biases. Including the analysis of the simulated meteorological fields will be beneficial to understand the similarity and dif-

C1

ference between observation and simulation. I recommend that the manuscript require major revision prior to publication.

Specific Comments:

- 1) This is the first one of two part papers. The title doesn't seem to represent this manuscript very well. It should be just two papers rather than two parts.
- 2) Page 5, lines 15-18: Surface concentration and PM may not be affected by this, but it may change total column concentration and AOD. What's the reason for this limitation? Is nudging filled the layers above 500hPa?
- 3) Page 6, line 31: Is this AOD for surface to 500hPa? Does this mean no long-range transported aerosol are considered in this study?
- 4) Page 7, line 10, "interannual variation in forest fire": Does the emission inventory used in this study include fire emission in 2007 (See also line 25 on page 3)?
- 5) Lines 27-32 on Page 8 & lines 25-26 on Page 9: It would be a good idea to show model simulation of winds (probably at 850hPa) and rainfall to discuss a possible impact of transport and scavenging effects to examine the discrepancy between model and observation.
- 6) Line 5 on Page 10, "caused by the limited monitoring data availability": Not clear. Not enough emission data? Could you elaborate this?
- 7) Lines 4-20 on page 12: PM_{2.5}/PM₁₀ ratio is used as a proxy to show the dominance of local sources. However, if the model top layer is 500hPa (Page 5, lines 15-18), aren't remote-origin aerosols suppressed in the model?
- 8) Lines 23-24 on page 12: All observations are from big cities. Can this ratio still provide any additional information?
- 9) Figure 6 & lines 30-34 on Page 13: All plumes seem to converge in the South China Sea. What's the role of meteorology, rain and winds?

C2

- 10) Lines 1-2 on page 14: It is not clear. Please elaborate.
- 11) Figure 7: Same color scheme should be used for better comparison.
- 12) Lines 7-8 on page 15, "better seasonal variability": Seasonal cycle in Phimai and Mukdahan are not simulated.
- 13) Figure S5: BC AOD in August is same as BC AOD in November. Please check.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-315>, 2017.