

Interactive comment on “Possible climatic implications of high altitude emissions of black carbon” by Gaurav Govardhan et al.

Anonymous Referee #1

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General comments:

The authors first showed vertical distributions of BC simulated by WRF-Chem around Hyderabad/India for three different days (17 March 2010, 08 January 2011, and 25 April 2011). The previous study showed that the observed BC has prominent spikes in 4 km or higher altitudes. The authors identified that the source of the spikes of BC is the aircraft emission. Second, the authors confirmed that the BC particles from the aircraft emission reach in lower stratosphere by using both the simulation and observation of CALIPSO. The manuscript and logic are straightforward, but I would like to ask the authors to add more explanation for more deeply understanding. Especially, how much are the results representative in each season? Although the measurements are very limited, i.e., one day per one season, the simulation seems to be easily conducted in at least one month to generalize and strengthen the conclusions. In overall, the

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manuscript would be acceptable for publication if these comments can be satisfactorily addressed.

Specific comments:

1. P.2, L27: Could the authors clarify actual heights of “the middle troposphere” here?
2. P.5, L2: How do the authors classify the BC1 (hydrophobic) and BC2 (hydrophilic)? Do the authors consider atmospheric aging processes of BC?
3. P.7, L32-33: The modification is not clear. Especially, the readers cannot understand the vertical profile of the BC emission. Please clarify it.
4. P.13, L20-22: How do the authors determine the background tropical stratospheric AOD? Please add some references or evidence to the manuscript.
5. P.14, L10-11: The reason to eliminate the existence of dust is not unclear for me, because there is a possibility to existence of dust particles in the fine mode. The fine particles of dust perhaps exist in the 21-22 km layers. Could the authors add more evidence or include the possibility of mineral dust to strengthen the reason of the elimination of dust in your analysis?
6. P.15, L10: Do the authors have any evidence of the increasing aircraft traffic?
7. Fig 4: Is this weather a typical around Hyderabad. Also, how about the other days (8 January 2011 and 25 April 2011)?
8. Figs 5 and 6: What is the difference in NoACEM/Ctrl between Fig 5 and Fig 6? I am confused.
9. Fig 7: How much did the meteorological fields, i.e., air temperature, change by implementing the BC radiative impacts?

Technical corrections:

1. P.4, L23: How do the authors define the 70-vertical layer? Please clarify each level.

2. P.8, L21: The acronym “FINN” should be defined in L20 not L21.

3. Fig 3: The map is too simple to know the information about city name and topography. Most of readers are not familiar to this area.

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