

Interactive comment on “Wintertime radiative effects of black carbon (BC) over Indo-Gangetic Plain as modelled with new BC emission inventories in CHIMERE” by Sanhita Ghosh et al.

Anonymous Referee #5

Received and published: 30 December 2020

This work estimated radiative impact of black carbon (BC) aerosols over one of the most polluted region in the world, IndoGangetic Plain (IGP) using a high resolution ($0.1^\circ \times 0.1^\circ$) chemical transport model simulation. Authors assessed the five BC emission inventories (Constrained, bottomup - Smog , C mip , Edgar , and PKU) and through a detailed statistical analysis of simulated BC concentration with respect to measurements, and constraint the large uncertainty of direct radiative forcing (RF) of BC. My specific observations are following, hope these could help improve the manuscript and make it better suitable for publishing in ACP.

1) Since it is an offline modeling study, only direct RF can be assessed but not indirect

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RF. This point deserves a further stress in the title and abstract. Also, I think some discussion regarding the potential influence (direct/indirect adjustment) of BC on cloud and cloud associated RF is worth to be commented in the manuscript.

2) Details of each BC Emission inventory is missing. More details are appreciated here. Such as what emission sectors are considered, how seasonal variation are considered (only Dec. is simulated) and etc.

3) Could you explain what do you mean by “constrained emission inventory”? Do you mean top-down based on satellite observation? More details about this is also required in the method.

4) Maybe I overlooked, but, it seems authors forgot to describe the AERNET AOD dataset in the Method section.

5) line-159: SO₂ is not aerosol

6) As best as I know, the latest EDGAR emission is with 0.1*0.1 deg. resolution. Please double check and correct it accordingly in Table 1.

7) As best as I know, WRF model is km-based, not deg.-based, so how could possible to perform WRF simulation in 0.1*0.1 deg. resolution?

8) I think more validation of model results would be needed. I not sure if it is appropriate to validate the model (2015 simulation) with observations in about 10 years ago (2004. . .2011, in Table 2). If most of observations are available in 2004, why not perform simulation in 2004, what is your specific reason to simulate 2015? And, only validate the surface meteo. may not be enough, some validation of vertical profiles would be appreciated, since radiative transfer is a vertical path.

9) Why December is chosen for the simulation, any specific reason? I see in the introduction, authors mention that BC concentration is high in winter IGP, but this high concentration is with respect to surface concentration, however, this paper focuses on radiative forcing which is a vertical column transfer path. High surface pollutants

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concentrations in winter IGP could be solely due to shallow PBL in winter compared to summer (Chen et al., 2020), but not necessarily a high column loading which is more relevant to RF. Some comments of this could help strengthen the discussion.

Reference Chen, Y., Wild, O., Conibear, L., Ran, L., He, J., Wang, L., and Wang, Y.: Local characteristics of and exposure to fine particulate matter (PM_{2.5}) in four indian megacities, *Atmospheric Environment: X*, 5, 100052, <https://doi.org/10.1016/j.aeaoa.2019.100052>, 2020.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2020-511>, 2020.

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