

Review of “Source attribution of black carbon and its direct radiative forcing in China” by Yang et al

Yang et al. investigated the BC source attributions (or more specifically, region attributions) in China with a source-tagging technique by employing a global climate model, NCAR’s Community Earth System Model. They found out that BC emissions from local (inside China) and non-local (outside China) are both generally important contributions to air quality in different regions of China, BC outflow from East Asia and direct radiative forcings. Overall, this paper is a helpful addition to our community that attempts to improve our understanding of BC source-receptor relationship. This paper generally reads well and is within the scope of ACP. However, before it can be accepted for publication in ACP, I have several comments that need to be properly addressed.

Major comments:

An important part of this study was to quantify the BC source contributions to trans-boundary and trans-pacific transport. In terms of model performance evaluation, this study only validated model simulations with observed BC surface concentrations from CAWNET and AAOD from AERONET over China. We don’t know the efficiency of BC outflow from East Asia. In this paper, it obviously missed the evaluations of model simulated vertical profiles of BC against aircraft campaign observations, e.g. A-FORCE and HIPPO, which should be employed to compare with model simulations.

Other minor comments:

Line 124: the reference Hoesly et al., 2016 is missing in the reference list.

Line 162: A brief description of dry/wet deposition scheme for BC in CAM is lacking here, especially the wet scavenging and how it is improved following H. Wang et al. (2013).

Line 334-336: This sentence should be corrected as “AI derived from Total Ozone Mapping Spectrometer (TOMS) measurements also shows similar pattern as simulate AAOD (Fig. S2).”

Line 339-344: What’s the assumption here? Why the ratio of AAOD to AI should be the same between western and eastern China? What is the role of dust here in assisting the speculation?

Line 424-426: I think BC emissions from SC are also important for the column burdens over continental China in some seasons (e.g. JJA and SON), which needs to be outlined as well.

Line 443: “Figure S4a” should be replaced with “Fig. S5a”.

Line 443-463: It is helpful for the authors to make supplemental plots showing the anomalous winds during polluted days that favor the accumulation of pollutants over each region.

Line 505-509: Why the authors only choose the latitude range along longitude 150°E, not a domain covering East China Sea and West Pacific to quantitatively assess the BC contributions from China and outside China, similar to that impact over West United States?

Line 509-510: I get lost here. It is not clear to me that 58% contribution from China emissions is for outflow or something else. Authors need to clarify this.

Line 531-538: I think the authors should list a table to compare your results with other studies, including annual BC emission budgets, burden, lifetime, DRF and DRF efficiency.

Line 654-655: Other modeling studies also found model low bias over China using CAWNET, e.g. Huang et al., 2013; Wang et al., 2014, which can be referenced here.

Line 669: “and” is missing between “modeled” and “observed”.

Reference:

Huang, Y., S. Wu, M. K. Dubey, and N. H. F. French, Impact of aging mechanism on model simulated carbonaceous aerosols, *Atmos. Chem. Phys.*, 13, 6329–6343, doi:10.5194/acp-13-6329-2013, 2013.

Wang, Q., D.J. Jacob, J.R Spackman, A.E. Perring, J.P. Schwarz, N. Moteki, E.A. Marais, C. Ge, J. Wang and S.R.H. Barrett, Global budget and radiative forcing of black carbon aerosol: constraints from pole-to-pole (HIPPO) observations across the Pacific, *J. Geophys. Res.*, 119, 195-206, 2014.