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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Top-down estimate of black carbon emissions for city cluster using ground observations: A case study in southern Jiangsu, China" *by* Xuefen Zhao et al.

**Anonymous Referee #2**

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In this study, Zhao et al. uses ground-based elemental carbon (EC) measurements from two sites in eastern China to evaluate and constrain black carbon (BC) emissions from two bottom-up inventories: a national/regional inventory for China (MEIC) and a high-resolution inventory for city clusters in southern Jiangsu Province. Both inventories include emissions from transportation, industry, power generation, and the residential sector. The authors show that the posterior emission estimates, constrained by ground measurements, are much smaller than the prior emission estimates, suggesting that pollution control measures by the Jiangsu government have effectively reduced emissions of BC. They also show results from various sensitivity tests, including those on the number of observation sites, spatial representativeness of observation sites, a



priori emission inventories, and wet deposition. Overall, this is an interesting study that can be potentially useful for air quality modeling and management, emission inventory development and evaluation, and also studies on regional aerosol effects. Through several fairly detailed sensitivity tests, the authors also demonstrate that the differences between a priori and posteriori emission estimates are robust. However, the paper is overly long (and needs some improvement in presentation quality) and some reorganization may help. And there are also some concerns about the methodology that need to be addressed before this paper can be published in ACP.

Major comments: It is not quite clear whether emissions outside of Jiangsu Province (but within the model domains) are scaled or not. Given the location of the sites, they could be strongly influenced by emissions from nearby provinces. If different local governments implemented different pollution control measures but the same domain wide scaling factors are used for emissions, that may lead to biases in the final estimated emissions for southern Jiangsu.

The lack of biomass burning emissions can be concerning. Could the model underestimates of BC in July and particularly October be caused by the biomass burning (particularly agricultural fires)? How does the lack of biomass burning emissions affect the estimated emissions for other sectors?

The paper is overly long and can be better organized. In particular, if spatial representativeness and wet deposition are important, can the authors focus on the top-down estimates that consider both of these factors? Description of the other sensitivity tests can be brief. Also writing needs to be improved.

Specific comments: Figure 3 and the paragraph starting from line 389: given that the scaling factor for April and Oct. are more uncertain (in terms of their statistical significance), are the seasonal patterns in the posterior emission estimates significant?

Figure 5a – what may have caused the model overestimates in mid-January at PAES? How does this period affect emission estimates? Can the authors exclude this period

and compare the top-down estimates?

Lines 456-461: again, could the model bias be due to the lack of biomass burning emissions?

Tables: There are already many tables in the paper (and maybe not everyone is absolutely necessary). But a table that summarizes the different cases may be helpful for readers to keep track.

Table 4 and related discussion on case 3: would the authors expect somewhat different driving conditions and emission factors for automobiles in urban and suburban settings? If so, is it still a valid assumption to assume the same scaling factor between NJU and PAES for transportation?

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