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Atmospheric Chemistry and Physics Discussions

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Interactive comment

# *Interactive comment on* "Rapid reduction of black carbon emissions from China: evidence from 2009–2019 observations on Fukue Island, Japan" *by* Yugo Kanaya et al.

**Anonymous Referee #2**

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This paper analyzed the declining trend in black carbon (BC) emissions from China, based on the long-term measurement data at a remote observation site in Japan. Combining air mass transport and air quality models, the authors made reasonable data filtering and simulation experiments. They drew a conclusion that China's BC emissions were clearly reduced in recent years, consistent with the big and continuous efforts of air pollution control by the government. In general the paper is well organized and written. Before it can be accepted for publication in Atmos Chem Phys, however, I have some concerns that should be more stressed or discussed. Some more detailed information should be provided as well, mainly in the measurement data and comparison between observation and modeling. The details follow.

1. The paragraph in Pages 3-4. The authors described the method of unifying the observations from COSMOS and MAAP, and stressed that the datasets correlate each other well. I suggest they provide the detailed correlation analysis between the two datasets with a figure, and indicated quantitatively the gaps between the two.

2. The third paragraph in Page 4. Does the author mean that the determination of influencing regions depend on the air mass transport modeling (HYSPLITT)? If so, I would suggest the authors provide the time-series or temporal variation of influencing regions within the research period, at least in the supplement. Some more discussions should also be given on the information.

3. Lines 11-16, Page 5. I cannot quite agree with the authors that the gap between observation and modeling indicates only the emission change, without the full evaluation of the model performance on 2008 (for which the emission data were applied). The determination of E(y)/REAS2.1(2008) thus seems problematic. How did the authors evaluate the model and recognize the modeling uncertainty for BC besides the wet deposition?

4. Figures 3 and 4. Why stress spring and select spring 2018 for comparing the modeling and observation results? Any special reasons?

5. Relevant with Question 3, I feel the authors need first to evaluate the model performance based on the observation, emission data and meteorology for the same year. The deviation between simulation and observation should be carefully studied to understand the uncertainty of modeling. Such bias should be excluded in the following step of determination of E(y)/REAS2.1(2008).

6. Small issue. What are the meanings of the dots with two colors in Figure 10b?

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