Atmos. Chem. Phys. Discuss., 15, C11235–C11238, 2016
www.atmos-chem-phys-discuss.net/15/C11235/2016/

Atmospheric Chemistry and Physics Discussions

Interactive comment on “Observationally-constrained carbonaceous aerosol source estimates for the Pearl River Delta area of China” *by* N. Li et al.

Anonymous Referee #1

Received and published: 7 January 2016

The carbonaceous aerosol is an important component of aerosols, not only impact on air quality, but also on climate change. This study is interesting that the authors use the simulated and observed results to quantifying carbonaceous aerosol source. As a researcher on air quality modeling, includes the modeling of elemental carbon (EC) and organic carbon (OC) aerosols, I found that the simulation and its verification in this study was week, and had some fatal errors on simulation period, model domain setup and others, the simulation must be re-do before it has been accepted as a research article in ACP. I recommend to reject this version of the manuscript for a publication in ACP.

General comments:

1) In this study, the authors use “four 7 day periods” to represent the four seasons, and the first day use as spin-up, “results from the second to seventh day were analyzed”, while the observation in most sites cover the whole year. That is a joke in the numerical study. As we known, the simulated periods must be match to the observed, that the model would reproduce the concentration in the observation period with the match meteorological field and emission inventory. As shown in Table.4, the observation period cover the year 2000-2003 and 2006-2009, if the authors want to reproduce the EC/OC concentration with the model, they should simulation the whole observation period. But the authors simulate “four 7 day periods” and make conclusion based on the simulation, this is a fatal error and the results are unacceptable.

More, the one day “spin-up” maybe too short, especially in the heavy air pollution days. In the future one seasonal simulation, the authors can use more days for “spin-up”.

2) The model domain setup is unreasonable. The model domain at 9km and 3km resolution is too small (shown in Fig. S1 in the supplement), and the station is too close to the boundary of the model domain at 3km resolution, which domain provides the simulated results for comparison. In CMAQ model, the small model domain would underestimate the peak concentration of the air pollutant in the simulation in 7-day periods, due to the influences of the boundary condition when nesting. That might be the important reason why the “top-down“ emission of EC/OC estimated by the multiple regression method is higher than the other studies (shown in Table.1). That is another fatal error in this study.

More, as described in page 33588, “We simulated three nested domains covering East Asia, Southern China, and the PRD area, with horizontal resolutions of 27, 9, and 3 km, respectively”, the second domain with horizontal resolutions of 9km should cover “Southern China”, but in the Fig.S1 in the supplement, the second domain just cover the Guangdong province. The authors can provide the parameter of model domain setup, which would help the reader to make clear about the domain setup. In my option, the model domain at 9km and 3km resolution should be expanded in the future simulation. The model domain at 9km resolution could cover “Southern China”, not only Guangdong province, but also its surrounding provinces, including Fujian, Jiangxi, Hunan, Guangxi and Hainan provinces, and the model domain at 3km resolution could cover PRD area and its surrounding areas.

3) Section 2.2 emissions. In this section, the authors should explain why the spatial distribution of “Bottom-up EC/OC emissions” from power generation and industry in Hong Kong is different from other areas shown in Figure 2 and Figure 3, and why the OC emissions of the power generation in Hong Kong is blank (Figure 3) while the EC emissions of power generation in Hong Kong is colored (Figure 2). Is the power generation emission in Hong Kong existing, or not?

4) In page 33594 line 10, “We averaged the seasonal measurement at the 9 location in Hong Kong to represent hereafter a single urban site, HK”, as I known the “Tap Mun” station is at the northeast of Hong Kong and far away from the urban, also be described in the note below Table 4, “gThese sites were report as reported s background sites in. . .” while the “g” includes the “Tap Mun” station in Hong Kong. Thus, it should not been included in the urban-averaged sites.

5) In page 33594 line 18-23, “These concentration are similar to the seasonal means typically observed in Shanghai . . . and Beijing. . .” this sentence includes so many references, using table to list these study would be more clear.

6) In page 33595 line 22-24, “At all sites where observation were available, the fraction of SOC in total OC were higher in summer (57%) and low in winter (46%)”, actually in observation list in Table 4, the fraction of SOC in total OC were higher in winter in some sites, e.g. Zhaoqing and Baplist University. In Zhaoqing station, the fraction of SOC in the total OC is 3.2/9.4 = 34% in winter, and 1.2/5.7 = 21% in summer.

7) There are something mistake in Figure 8: the EC and OC emission of “Biomass

burning” in the east of Hong Kong is empty, and the boundary of emissions is straight line.

8) The model evaluation is week, only include the mean value verification of EC and OC concentration in different sites in PRD area, the statistical parameters (e.g. mean bias, mean error, root mean square error) and plots (e.g. time series) on model performance is missing in the discussion paper. The statistical parameters and plots will help reader to make sure whether the result is reasonable or not.

9) Which datasets has been used at the initial and boundary condition for the regional meteorological model WRF? The WRF model is regional model, and it needs the global dataset to drive it, as the initial and boundary condition.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 33583, 2015.