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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Air quality and emissions in the Yangtze River Delta, China" *by* L. Li et al.

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Reference No.: acp-2010-615 Authors: Li. Li, Chang.hong Chen, Cheng. Huang, Yangjun. Wang, Haiying. Huang, Joshua S. Fu, David G. Streets, and Carey J. Jang Title: Air Quality and Emissions in the Yangtze River Delta, China Date: 18th October 2010

The paper is scientifically sound and worth publication. It presents a useful update of emission inventory for YRD and reveals the air quality of YRD using chemical transport model.

In the past decade, modeling of air quality of China using 3-D mesoscale chemical transport models has been developing very fast. It is quite well known that the emission inventory (EI) of China is considerably under-estimated. There is a pressing need to compile a reasonably accurate EI especially in the China northeast industrial region, the YRD and the Pearl River Delta region. This paper reported the latest improvement of the emission inventory of the YRD region and this should be the main contribution of this manuscript. However, there are a few omissions and ambiguities. The English standard of this manuscript could be further improved.

Major comments: (1) The English standard should be improved to remove ambiguities in a few places. A few improvements have been listed in the specific comments below. (2) There are two models used in this study – MM5 and CMAQ, the authors should clarify which model they are referring when they are reporting their inputs, boundary conditions and initial conditions in section 2. Another point is all 3 domains of MM5 are clearly defined but the domain and vertical levels of CMAQ were not mentioned. (3) Sections 2.3, line 3 stated that the new emission inventory consists of biomass burning and biogenic sources but no more details were given afterwards. This is a major omission. (4) Some discussions about the level of pollutants are stated without support; some are too subjective which makes the arguments speculative. (5) The labeling of the 16 cities in the figures is quite confusing. There are two Taizhou but they are not properly differentiated in the figures. (6) The term "average" is used very loosely throughout the manuscript.

Specific comments: (1) In the abstract, only the monthly averaged SO2 is reported, it is recommended that the modeled NO2, PM10, and Ozone should be given too. (2) Pg 2, last paragraph: the line "driven by a new, comprehensive emission inventory." should be rewritten. In this manuscript, only the YRD inventory is updated, all the other regions are not "new". Neither is the term "comprehensive" appropriate. (3) It is mentioned in pg 3, section 2.1 that "The driving meteorological inputs are provided by MM5", presumably, the authors are referring to CMAQ input? Also what inputs are used to drive MM5? (4) Section 2.2, 1st line: "The model domain" should be changed to "The

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MM5 domain"? (5) Figure 1: There is one city between Huzhou and Changzhou that is not labeled. Including this city, there should be a total of 17 cities. Are the emissions of this city not included? (6) Section 2.2, 2nd line: I don't understand the term three-way nested, I have heard of one-way and two-way interactions but not 3-way. (7) Section 2.2, line 10: How boundary condition is considered was not shown, and if the study only use the default BC given in the CMAQ model, it is not helpful to give an ideal and reliable initial condition only by running the model five days ahead of start date with clean initial conditions because the default BC is also clean and without day-to-day variation. (8) Section 3.1.2, 3rd paragraph: PM2.5 and NH3 emissions were stated for stationary sources but they are not stated for mobile sources. In line 6, the words "larger share" is wrong choice of words. (9) Section 3.2.1: Hourly concentrations for SO2, NO2, PM10 were selected during January 11-20, 2004, and July 11-20, 2004. Since the comparison of observed and model results focus on this time period, why Table 2 only assess the MM5 performance of Jan 01,02,20,25,28, and Jul 01, 02? Why these days are selected and why not shows the results of January 11-20, 2004, and July 11-20, 2004. (10) Section 3.2.2: The term "coefficients" should be defined more properly. (11) Section 3.2.2, figure 6: Is the NCEP data FNL data? Is it re-analysis data? The source of NCEP data should be given. Also, if the NCEP data is used to drive the MM5 model (my speculation), then it is not appropriate to compare the model results with its input data. It is more appropriate to compare model outputs with observational data. (12) Figure 7: the labels of the x-axis basically have no meaning. (13) Section 3.2.3, pg 8, line 7: The phrase "true SO2" is a strong phrase. Suggest to replace the phrase by "reflect the general SO2 concentration in the YRD region." (14) Section 3.2.3, figure 8: The legend of figure 8 stated monitoring average and model average. There are a few monitoring stations in Shanghai, how the monitoring results are averaged? Also, how the model results are averaged? Which grid (all grids in Shanghai) or which vertical layers (only surface layers or all layers in BL) are included? (15) Section 3.2.3, NO2: "the NO2 simulation results are not as good as for SO2". After looking at figure 8 and figure 9 and Table 3 as well, I quite disagree with this statement.

Table 3 shows that the hourly average of NO2 in both January and July are quite similar. More information should be given to indicate why NO2 simulation is not as good as SO2. (16) Section 3.2.3, PM: “Results show that CMAQ can reflect the trends of PM10 in January, while in July the model tends to underestimate the PM10 concentrations.” My reading of Figure 10 indicates the opposite. The model underestimated PM in January in both Nantong and Ningbo. (17) Section 3.2.3, Factor 2 analysis: Since the MRF scheme is not satisfactory, then why not use another scheme? (18) Section 3.2.4, Equation 7: I think there is an error in equation 7. The symbol oi is not defined. The i should be a subscript. Also bar o is defined as average observed ozone concentration, the word “ozone” should be deleted? (19) Section 3.2.4, pg 11, line 9: I disagree that “generally wind speed is low in winter” in YRD. Wind speed can be quite high during winter monsoon season in East China. (20) Section 4.1, line 4: “The NO2 subsequently reacts with O3 and reduces the O3 concentration.” This sentence is over simplified and needs to be rewritten. (21) Figure 14: The title needs to be changed. A scatter diagram cannot reveal the relationship between O3 and NO2, it can only show its correlation. The corresponding discussions in section 4.1 should be corrected as well. (22) Figure 15: Specific selection of July 5 and not other days should be justified. It would help the interpretation if wind arrows are added to this figure. (23) Section 4.1, 2nd paragraph: The sentence “high O3 gradually diffuses from Zhoushan” should be re-written. The description is over simplified, apart from advection, there could be dispersion and photochemical production. Furthermore, the high O3 first appear over Zhousan is very interesting but there is little discussion about this ozone evolution. Figure 15 indicates that Zhoushan is a coastal area. Under southeasterly winds, there is no land based emission sources upwind of Zhoushan, why there is considerable O3 over Zhoushan first? (24) Section 4.2, equation 8: definition of extinction coefficient should be given. (25) Figure 19: One city label is missing in Figure 19. Figure 19 basically duplicate Figure 13. Also, number of cities in Figure 19 is different from that in Figure 18. (26) Why monthly average of dcv is presented and discussed instead of PM10? (27) In the conclusion, line 1, “first time” is a sensitive phrase. Recommend to

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delete “for the first time”.

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