Atmos. Chem. Phys. Discuss., 11, C162–C164, 2011
www.atmos-chem-phys-discuss.net/11/C162/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The study of emission inventory on anthropogenic air pollutants and VOC species in the Yangtze River Delta region, China" *by* C. Huang et al.

Anonymous Referee #1

Received and published: 11 February 2011

General comments: The manuscript The study of emission inventory on anthropogenic air pollutants and VOC species in the Yangtze River Delta region, China by Huang et al. present a very comprehensive study on the emission inventories of various air pollutants in YRD, China. The reviewer appreciate the huge efforts to integrate all the information together, and I think this is really necessary for YRD region to have a localized emission inventory to understand the regional air quality problems, it can also serve as a basis for further evaluation and improvement. Therefore I would suggest acceptance after some necessary revisions.

Specific comments: 1. The title read a bit strange, could be changed to The study on emission inventoies of major anthropogenic air pollutants in the Yangtze River Delta region, China ? 2. The methods for emission inventories, such as mass balance, top-down approach, and bottom-up apporach are used in the manuscript. I wonder why? The euqation(2) does not look like a realiable mass balance methos (the residue in coal ash were not counted), and secondly, is it possible to have inter-comparison of these approaches? 3. The activity data have also two sources: the enviromental census and Statistical Yearbook, for the cities that have both sources, is it possible to compare these two datasets? At least give an idea to the readers how different they will be? And also important, the MS stated that the high-resolution of emission inventories is essential, how to get the actitity data for the countryside in the YRD region? The authors needs to explain this part to convince readers that the actitity data of this MS is more complete than previous ones. 4. The major obstable of the MS come from the selection of emission factors. I notice that the authors mainly cited the EFs from literatures, and this is a highly risky way. I believe if the authors compare the EFs for the same source from different researchers, the difference would be very large.I would like the authors put more efforts in this section, and illustrate to the readers the current understanding and progress of the emission factors, why the emission factors were selcted for the 2007 YRD inventories. 5. The uncertianty analysis is weak. For an inventory work, the reviewer believe that the uncertainty analysis is equally important as the emission data. One could not understand the statement for the solid or problematic of the inventories just from the current description. I would like to suggest a quantitative evaluation for typical inventories (e.g. SO2, Nox, and VOCs), and for typical sources (e.g. vehicle, biomaa burning). 6. The English expression in the manuscript needs to be imporved. The introduction section is too generally, I would like to add technical progess here for the emission inventory development, e.g. the EFs, and how to get reliable activity data, how to better allocate the emissions? For industrial sources, is the term " exhaust treatmentefficiency" or "exhaust control efficiency" correct? Why use two different terms?

ACPD

11, C162–C164, 2011

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 951, 2011.

ACPD

11, C162–C164, 2011