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Comment

***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 #3

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Main comments This paper presents a detailed emission inventory for several gas-phase and particulate phase pollutants for the Yangtze River Delta region, a region which accounts for 20% of the Chinese Gross Domestic Product. The paper is generally well organized and it presents a very comprehensive study of each pollutant source category and definitively deserves publication in Atmospheric chemistry and Physics. Nevertheless, some sections of the current manuscript are not very conclusive and/or would deserve more discussion (see specific comments) and it is recommended to improve these sections in the final version of the publication.

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Specific comments P955, L5: any reference for the satellite picture?

Sections 2.3 and 3.5 The emission factors used in the inventory were generally those already compiled by Zheng et al. (2009). In order to add new information to this compilation, it is recommended to develop the discussion on the uncertainty associated with these emission factors; especially for the emission factors which originate from US or European studies and for EF which are associated with high activity numbers (because they will have a significant weight in the total emission). For the EF which have been determined in China, and which have equivalents in US and/or Europe, would it be possible to give the range of differences? (i.e. they differ by a factor of 2? 3?...more?, it is of course source dependent but this could be given for the main sources).

P959: is the emission height taken into account in the source allocation?

P961: SO₂, NO_x, PM₁₀ annual concentrations are presented: where do these results come from? Why have these three specific pollutants been selected?

Section 3.2 (and Fig. 3 and 4): Why CO has not been included in the figures and in the discussion? This section is named “emission contribution by source categories” but also includes a discussion on the comparison with the regional emission inventory from the Pearl River Delta region. Comparison with existing inventories is an important issue and would deserve an independent section. In the introduction the authors state that “low-resolution inventories were thought to cause under-estimation of air pollution simulation in recent modeling studies”. Therefore it would be very valuable to compare the emissions derived from this study for the YRD region with the emissions of the corresponding grid cell of the national inventory. The comparison with the regional PRD emission inventory would also deserve more discussion.

Section 3.3 P962: It is difficult to compare VOC distribution from an emission inventory with measurements performed on-board an aircraft (which are representative of a given time and a given region). If there is no additional information on this comparison, it

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could be deleted as not relevant for the discussion.

Section 3.4 The ozone formation potential discussion is not very conclusive, in particular because biogenic VOCs are not included in the inventory. If kept, then the discussion should be improved (reference to BVOC from GEIA 1990 should be updated: see the MEGAN emission inventory, Guenther et al., 2006).

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

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