

# ***Interactive comment on “Improved provincial emission inventory and speciation profiles of anthropogenic non-methane volatile organic compounds: a case study for Jiangsu, China” by Yu Zhao et al.***

## **Anonymous Referee #2**

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Improved provincial emission inventory and speciation profiles of anthropogenic non-methane volatile organic compounds :a case study of Jiangsu, China By Zhao et al.

### General comments:

The paper describes a bottom-up development and evaluation of a highly-resolved regional emission inventory for NMHCs in the area of Jiangsu, a strong industrialized region in Eastern China. The reference period is almost 10 years. The quantification of chemical processes is based on the determination of realistic source profiles for industrial activities by near-source measurements. The authors provide an extensive

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work.

The paper is divided into 4 parts: first part describes the inventory methodology, second part compares the newly released emission inventory to other downscaled emission inventories regarding absolute emissions and speciated emissions; third part uses the CMAQ model to test the ability of the model to reproduce hourly maximum ozone concentrations with the new emission inventory. Evaluation of emission inventories is important for improving the simulation and forecast of air quality and climate and is unfortunately often neglected. While the science is of relevant atmospheric interest, I have major concerns about the paper:

1/ the paper is not easy to read and the reader gets easily lost. For instance, the authors often go backandforth with figures and associated discussion (ie. Figure 7). In several parts or sections, the paper relies on information reported in the Supplement Material which often makes the paper hard to follow, especially regarding the development of the emission inventory.

2/The sampling and analysis strategy in the field is not described and motivated. Line 103: the choice was put on the speciation of chemical industries. The sampling strategy and the representativeness of emission measurements should be detailed.

3/ Some sections do not provide reliable information

Lines 514-525: The authors compare the spatial distribution of emissions from industrial activities from three different methods including one without any information on individual plants, which uses proxies like population density. They show that the spatial allocation from this method is wrong and not representative of local characteristics. One could wonder whether such result could have been predictable.

4/ The interpretation of the figures are incomplete or approximate. Regarding the various emission inventory evaluation, it looks like the improvement with the update is not so clear in term of absolute concentration, reactivity and spatialization.

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Lines 476-486: the discussion on uncertainty comparison should be revised or at least clarified. Differences in uncertainties between inventories in Table 3 could be also due to the way uncertainties are estimated or the spatial resolution. Indeed the authors give the impression that the uncertainty of the new released inventory is better. It might be for the wrong reason.

Line 551-561: Comparing the updated speciation of VOCs to the SPECIATE emission profiles is relevant. It is surprising to see that the updated VOC speciation profiles are not so different from the foreign SPECIATE database excepted aromatics and ethylacetate. It would be also relevant and interesting to compare database by only considering the measured profiles.

Figure 4: the comparison with other emission inventories for the same spatial domain reveals a quite good consistency regarding absolute values and trends. As stated by the authors all the results are within the 95% confidence limits. From this figure, it seems that differences are not statistically different. However the authors keep insisting on such differences. Figure 7: The difference between speciation is not so significant after updating and finally raises the question of the usefulness of the updating except for aldehydes. This deserves some discussion.

The simulation with the CMAQ model. The model outputs not only depend on the representation of emissions but also on the representation of chemistry and dynamics. The authors should take these two drivers as well to explain potential differences with the observed ozone-hourly maximum. Note that we also see some differences between seasons. Emissions might not be the only limitation. The get free from dilution effects or chemistry effects the authors could rather use ratios. To conclude. This paper is of importance and I would like to highlight the extensive work that has been accomplished. However, given my comments above, I would not recommend publication. I would encourage the author to submit again their manuscript after improving the clarity of the paper (organization) and the accuracy of conclusions. The authors should also reduce the length of the paper as some parts are not supportive.

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Specific comments: Figure 2: characters are not visible Line 337: explain why SPECIATE is used. There are other database (European) that might be also relevant. Please explain why using SPECIATE. Line 439: explain the use and atmospheric relevancy of the OFP/emission ratio

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