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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Improvement from the satellite-derived $NO_x$ emissions on air quality modeling and its effect on ozone and secondary inorganic aerosol formation in Yangtze River Delta, China" *by* Yang Yang et al.

**Yang Yang et al.**

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We thank very much for the valuable comments and suggestions from the reviewer, which help us improve our manuscript. The comments were carefully considered and revisions have been made in response to suggestions. Following are our point-by-point responses to the comments and corresponding revisions.

0. The authors developed a "top-down" methodology based on the inversed chemistry transport modeling and satellite data to estimate the NOx emissions for four seasons



in YRD region in 2016. The results show that the improved NO2, O3, and SNA simulation results can be achieved with top-down estimates comparing to current bottom-up estimates. Further sensitivity study of O3 formation indicates the effectiveness of controlling VOCs emissions on O3 pollution abatement for PRD region and reducing NH3 emissions was crucial to alleviate SNA pollution of YRD in winter. The manuscript was generally well written, the research presented is innovative and the results can guide the policymaking. I recommend this paper to be published in ACP after some comments have been addressed.

Response and revisions:

We appreciate the reviewer's positive remarks.

1. Please revise the introduction part thoroughly to improve the narrative logic, the current version is a little hard to follow and some statements need to be summarized.

Response and revisions:

We thank the reviewer's comment. We thoroughly checked and revised the introduction. The section was better paragraphed to make the narrative logic clear. Some distracting sentences were deleted, and some summarizing phrases were added in corresponding positions. Now the main contents of introduction include 1) the importance of NOX emission inventory and its bottom-up development method; 2) the top-down method; 3) the more application of the top-down method at the global/national scale compared to the regional scale (limitation); 4) the evaluation of the top-down emission estimates and limitation; and 5) summary of main tasks of this work.

2. Line 259-265: The description of Table S3 does not agree with Table S3 shown in the Supplement file. And please clarify the meaning of "-" in Table S3, preferably with a footnote.

Response and revisions:

We thank the reviewer's reminder and we are sorry for the error. The description for Ta-

ble S3 was corrected in the revised manuscript (Lines 271-277) and now the statement agrees with Table S3. The meaning of "-" in the original table was that the emissions was not changed, and "No change" in the revised table is applied instead of "-" to avoid the confusion.

3. Line 386-389: Why did the authors only perform an extra simulation of exploring the influence of BVOCs emissions with top-down estimate instead of with both top-down and bottom-up estimates to prove that a better O3 simulation can be achieved based on top-down NOx estimates? Please clarify it.

Response and revisions:

We thank the reviewer's very valuable comment. The evaluation of emission inventory could be complicated with different species included. In this work, as shown in Figure 4c (Figure 3c in the original submission) in the revised manuscript, very clear improvement in NO2 simulation was found with the top-down NOX estimates for July, implying the improved emission estimation with the satellite constraint. The O3 simulation for July, however, was poorer when top-down estimate was applied (Figure 6c). We expected many other factors contributed to the uncertainty in O3 simulation, besides the NOX emission input. One possible factor could be the overestimation of BVOCs emissions. That's why we performed an extra case by reducing half of BVOCs emissions in the YRD region. Although the model performance was improved compared to the case without BVOCs reduction (Figure S3 in the revised supplement), still it was poorer than the case with bottom-up NOX emission estimates applied (note the NMBs with bottom-up NOX emissions applied was very small at 1.1% in Figure 6c). This comparison thus suggested that the complicated mechanism for summer O3 formation was insufficiently considered in current model, and it is partly out of scope of current paper. We clarified this in lines 393-396 and lines 404-407 in the revised manuscript.

4. Line 409-413: Please add references after these two statements.

Response and revisions:



We thank the reviewer's comment. We add references (Wang et al., 2019 and Li, 2019) after the two statements.

Reference: Wang, N., Lyu, X., Deng, X., Huang, X., Jiang, F., Ding, A.: Aggravating O3 pollution due to NOx emission control in eastern China, Sci. Total Environ., 677, 732-744, 2019.

Li, L.: Application of new generation natural source emission model in Yangtze River Delta and its influence on SOA and O3 (in Chinese), The 4th application technology seminar on air pollution source emission inventory in China, Nanjing, China, September 18-19, 2019.

5. Line 423-426: Please explain more to support the inference and can authors replot figure S2? The current one is blurring.

Response and revisions:

We thank the reviewer's comment. We explain more to support the inference in lines 440-445 in the revised manuscript: As east-central YRD was located in a VOC-limited region, the O3 concentrations of the region would be elevated along with the reduced NOX emissions (Wang et al., 2019). The comparison between Figure 7 and Figure S2 (original submission) thus reflects the negative effect of NOX control on O3 pollution alleviation in the region. We also replot Figure S2 and improve the figure quality in the revised supplement. We move to the figure to the main manuscript as Figure 3 (please see our response to Question 7 of Reviewer #2).

Reference: Wang, N., Lyu, X., Deng, X., Huang, X., Jiang, F., Ding, A.: Aggravating O3 pollution due to NOx emission control in eastern China, Sci. Total Environ., 677, 732-744, 2019.

6. Line 427: I think changing SIA to SNA would be better to keep the consistency of the full text.

Response and revisions:

We thank the reviewer's comment and have changed SIA to SNA in the full text.

7. Line 451-453: Sha et al. (2019) reported that SO2 heterogeneous oxidation can largely improve the sulfate simulation results in Nanjing. Authors may incorporate the related mechanisms to perform the simulation, if possible, or at least mention this potential reason when discussing the factors influencing the accuracy of SNA simulation. References: Sha T, Ma X, Jia H, Tian R, Chang Y, Cao F, Zhang Y. Aerosol chemical component: Simulations with WRF-Chem and comparison with observations in Nanjing. Atmospheric Environment. 2019 Dec 1; 218: 116982.

Response and revisions:

We thank the reviewer's important comment. We agree with the author that the chemical mechanisms in the model could be important for model performance. We have added the reference (Sha et al., 2019) and the statement that SO2 heterogeneous oxidation can largely improve the sulfate simulation results in Nanjing in lines 472-475 in the revised manuscript.

Reference: Sha, T., Ma, X., Jia, H., Tian, R., Chang, Y., Cao, F., Zhang, Y.: Aerosol chemical component: Simulations with WRF-Chem and comparison with observations in Nanjing, Atmos. Environ., 218 (116982), 1-14, 2019.

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