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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation and uncertainty investigation of the NO_2, CO and NH_3 modeling over China under the framework of MICS-Asia III" *by* Lei Kong et al.

Anonymous Referee #3

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This work evaluated 14 model simulations of NO2, CO and NH3 over China under the framework of MICS-Asia III with the aim to assess the capability and uncertainty of current CTMs in East Asia. Model results were provided by a larger number of independent groups and covered a full year (2010). The results show that most models well captured the monthly and spatial patterns of NO2 in NCP though NO2 levels are slightly underestimated, but relatively poor model performance was observed in the PRD region. All models significantly underpredict CO concentrations both in the NCP and PRD regions and failed to reproduce the observed monthly variation of NH3 in NCP.

This work quantifies the impacts of model uncertainties on simulations of the three primary gases, which shows the large uncertainty (spread) in simulating more reactive and/or short-lived primary pollutants (e.g. NH3). This work is important and valuable to the scientific and regulatory community as it provides information on the capability and limitations of some widely used models. The manuscript is well organized and well written, and model results (tables and figures) are clearly presented. I recommend its publication after the authors have addressed my comments listed below.

1. For comparison with the NO2 measured from the regular monitoring networks, please note that these networks employ a thermal conversion method which converts NO2 to NO, followed by detection of NO. This method is known to overestimate NO2 as it also converts other NOy species such as HONO and PAN etc (e.g., Xu et al., 2013). It is important to correct this measurement problem before making the comparison, using, for example, the approach by Zhang et a. (2017). After corrections of the measurement data, a closer agreement would be seen between the modelled results and the observations in the present work. If the author cannot make such corrections in view of a large number of groups involved, at least some discussions should be provided on this point. References

Xu, Z., T. Wang, L. K. Xue, P. K. K. Louie, C. W. Y. Luk, J. Gao, S. L. Wang, F. H. Chai, and W. X. Wang. "Evaluating the Uncertainties of Thermal Catalytic Conversion in Measuring Atmospheric Nitrogen Dioxide at Four Differently Polluted Sites in China." Atmospheric Environment 76 (Sep 2013): 221-26. http://dx.doi.org/10.1016/j.atmosenv.2012.09.043.

Zhang, L., Q. Y. Li, T. Wang, R. Ahmadov, Q. Zhang, M. Li, and M. Y. Lv. "Combined Impacts of Nitrous Acid and Nitryl Chloride on Lower-Tropospheric Ozone: New Module Development in WRF-Chem and Application to China." Atmospheric Chemistry and Physics 17, no. 16 (Aug 2017): 9733-50. http://dx.doi.org/10.5194/acp-17-9733-2017.

2. Section 2.2. The comparison of NO2 and CO concentrations are only for NCP and

PRD. Any reasons why not to include other regions?

3. For simulations of NO2 (and NH3), accurate representation of nitrogen chemistry is critical. Recent studies have shown that the HONO sources may be under-represented in some models which would give rise to larger simulated NO2 values (as it underestimates the oxidation of NO2 by OH) (e.g., Zhang et al., 2017; Fu et al., 2019); N2O5 uptake on aerosol may be treated differently in models which could also affect the NO2 simulations. Therefore, in discussing the discrepancy in modelled NO2, information on how models treat these nitrogen processes would be helpful.

Fu, Xiao; Wang, Tao; Zhang, Li; Li, Qinyi; Wang, Zhe; Xia, Men; Yun, Hui; Wang, Weihao; Yu, Chuan; Yue, Dingli; Zhou, Yan; Zheng, Junyun; Han, Rui "The significant contribution of HONO to secondary pollutants during a severe winter pollution event in southern China" ATMOSPHERIC CHEMISTRY AND PHYSICS, Volume: 19, Issue: 1, Pages: 1-14, JAN 2019

4. The photo-chemical mechanisms used in this study are CBMZ, CB05, and SAPRC 99, and some of them have an updated version such as CB06 and SPARC 07. These updated mechanisms could give different results on model performance. The author is advised to discuss this point to alert the reader that their conclusion may not be applicable to the newer version of the respective mechanism.

5. The present comparisons focused on yearly and monthly model performance. It would be interesting to show how different models compare during severe pollution episodes. An important application of CTMs in China is to forecast severe episodes based on which emergency source control measures are activated.

6. The model comparisons were conducted for NO2, CO, and NH3. How about SO2, which is another important primary pollutant? I think the reader would be interested in seeing the model performance for SO2 as well.

7. Conclusion (1) recommends to improve the CO emission inventory which is for year

2010. Does the recent CO emission have similar problem?

8. This study reveals a large spread of model simulations for reactive gases. As the exact causes for the difference have not been identified for the individual model, I think it is important to emphasize the need to validate the individual model before using its results to make important policy recommendation.

minor comments:

Line 40 page1, line 4 page 4, the "Peral" should be "Pearl".

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