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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 #2

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This paper conducts ensemble air quality modeling of NO2, CO, and NH3 over Asia, and evaluates model performance using measurements data in the North China Plain and Pearl River Delta regions. 14 models including 13 regional models and one global model with common emission inventory, meteorological fields, modeling domain, and horizontal resolutions were used for the ensemble modeling. The results show that NO2 and CO simulations are mostly underestimated and NH3 modeling mismatches the observed temporal variations. Possible reasons for the model structural uncertainties and recommendations for the future studies are given by the authors. This paper is good in general and within the scope of Atmospheric Chemistry and Physics. I recommend for publication once the concerns expressed below are addressed. Some

specific comments: 1. Although 14 models are required to use standard meteorological field, the configurations of meteorological models may not be identical. The author also needs to list the configurations of each meteorological model as in Table 1. Meanwhile, since the meteorological parameters have large impact on the modeled concentrations, the modeled meteorological fields also need to be validated against observed data. 2. The model performance in PRD is much worse than that in NCP. The author concludes that it is because of coarse horizontal resolution. I think uncertainties may primarily come from the emission inventory, especially spatial allocations from different emission sectors are not well resolved in the PRD region. I suggest the author use one or two models with finer resolution to test the model performance again in PRD, to see if the horizontal resolution is the main problem as the author demonstrated. 3. I agreed with the author using the available NH3 observations from the other years as an alternative to evaluate the performance of different models. However, to evaluate the modeled temporal variations using observed data from different years may not be appropriate, because the NH3 emissions vary year by year, and control measures may be applied in year of measurement conducted. 4. Figure 5 is an interesting finding in this paper. I am surprised that the NH3 gas-aerosol partitioning simulations from different models have such large discrepancies. Is it because the chemical mechanisms in different models treating NH3 different? Otherwise, please explain why does such large discrepancy of NH3 gas-aerosol partitioning occur in different models. 5. In summary, the author makes a few recommendations for future studies. I think inversions of NOx and CO emissions will help to reduce uncertainties in emission inventory and improve model performance, since many inverse modeling works of NOx and CO emissions have been done using satellite as well as ground observations. However, I have doubts on inversion of NH3 because of the reactivity and uncertainties in the chemical pathways of NH3 gas. 6. In page 1, line 40, change "peral"to"pearl". 7. In page 4, line 4, missing "plain"; line 5, change "peral"to"pearl". 8. In Figure 1, I think the color of CO measurement sites in NCP should be"green"instead of "blue".

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