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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effect of different emission inventories on modeled ozone and carbon monoxide in Southeast Asia" *by* T. Amnuaylojaroen et al.

Anonymous Referee #2

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This paper applies five emission inventories (RETRO, INTEX-B, MACCity, SEAC4RS, and MACCity/SEAC4RS) to a regional air quality model (WRF-Chem), to study the effect of different emission inventories on CO and ozone in Southeast Asia. Despite the considerable difference among different emission inventories, the authors find much smaller difference in the modeled CO and ozone. More importantly, the authors find overprediction of ozone and underprediction of CO by WRF-Chem with all five emission inventories. Thus none of these inventories can help to substantially improve the simulation of CO and ozone. While the results are useful to the existing literature, a lot of details are missing or incomplete. Overall, I feel that this paper lacks in-depth

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discussion on the model results. I recommend this paper to be published with major revisions.

1. A major conclusion of the paper is that there is severe underestimate of CO and overestimate of ozone by WRF-Chem, and this cannot be fixed by using different emission inventories. For the underestimate of CO, the authors provide two possible reasons: (1) vertical distribution of biomass burning plumes (2) missing CO sources such as biofuel and trash burning. But this is not satisfying as vertical distribution of bb plumes does not help the underestimate of CO in December when biomass burning emission is relatively small. The more interesting question is, how much increase on emission is needed to reproduce those CO surface observations? Kopacz et al. [2010] recommends a large increase of current CO anthropogenic emissions over Southeast Asia, by almost a factor of 2. The authors also state in the text that CO emission has an uncertainty of $\pm$185%. It seems more interesting that the authors can scale up their emission inventories by a factor of 2-3, to see if the bias in CO can be eliminated. Another possible reason is the model chemistry. A recent paper by Mao et al. [2013] suggests that the heterogeneous process can also help to improve modeled CO particularly over Southeast Asia. It seems to me that further discussion is warranted on these hypotheses.

2. The authors seem to have ignored the overestimate of ozone in the discussion section. Some insights are needed for this bias.

3. I agree with reviewer #1 that, a table with detailed comparison for each inventory, including seasonality, year, total amount etc., would help the reader to understand the difference among these inventories.

4. Comparison with satellite. The discussions on satellite comparison are very brief and problematic. My understanding is that MOPITT signal is very weak for the surface air due to low thermal contrast between surface air and the surface itself [Deeter et al., 2007]. What is the uncertainty level for MOPITT retrieval for surface CO? Also it

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seems that MOPITT CO is less than all modeled CO, and even further away from those ground site observations. This makes the reader suspicious of the quality of MOPITT retrieval on surface CO.

5. Model spin up. Given the long lifetime of CO and ozone (particularly in winter), how was the model spun up for each simulation with different emission inventories? Did the WRF-Chem have a few months to spin up, or just use the same boundary condition from MOZART at the beginning of March and December of 2008? This needs to be clarified. If there is bigger discrepancy among those inventories in other months (February for example), how will that affect the modeled CO or ozone?

Reference

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Mao, J., S.-M. Fan, D. J. Jacob, and K. R. Travis (2013), Radical loss in the atmosphere from Cu-Fe redox coupling in aerosols, Atmos. Chem. Phys., 13(2), 509-519, doi:10.5194/acp-13-509-2013.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9345, 2014.