

## ***Interactive comment on “Correcting model biases of CO in East Asia: impact on oxidant distributions during KORUS-AQ” by Benjamin Gaubert et al.***

### **Anonymous Referee #2**

Received and published: 28 July 2020

This study investigates the impact of model biases in CO on the ability to simulate oxidants observed during the KORUS-AQ campaign. In particular, it explores the effects of assimilating MOPITT CO as well as using different emissions inventories. The question of how improving simulated CO alters other aspects of atmospheric chemistry is an interesting one, and the study applies state-of-the-art tools to address it. The methodology is rigorous, but I have some suggestions, described in the general and specific comments, for how the results could be presented more clearly.

### General Comments

The study includes quite a few model simulations, both with and without MOPITT assimilation and different prior or posterior anthropogenic and biogenic emissions inventories. Further simulations with different biogenic emissions are also discussed in the

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supplemental material. While I understand the need for multiple sets of simulations to explore different ways of addressing CO bias, it can be difficult for the reader to keep track of what is included in each one and which lines to compare on figures (e.g. Fig. 9) showing many different model simulations. I suggest that the discussion in Section 6 (and some of the figures) would be easier to follow if that section were divided into two parts, with one part focused on the impact of MOPITT data assimilation, and the other part focused on the role of the emissions inventories.

The manuscript devotes a substantial amount of text to describing aspects of the model and assimilation system that do not seem directly tied to the discussion of the results. Some of this information, such as Section 4.2, could be condensed and/or moved to the Supplement or Appendix.

Appendix B contains a considerable amount of information and analysis, but I did not find it referenced in the main text.

The evaluation of NO<sub>x</sub> and other species in Section 2 of the supplement, and Fig. S2 in particular, is quite relevant to the interpretation of the results. It would be helpful to include some of this material in the main text.

### Specific Comments

Line 62: Is “pollution ozone” the same as “ozone pollution”?

Line 460: Why is met data assimilation used in one set of runs and nudging in another set? Does this lead to transport differences between the two sets of runs?

Table 2: The simulation names should be made more informative/intuitive. For example, only one is called “Posterior” even though multiple runs use Posterior emissions.

Line 557: Does this mean it’s the old RMSE minus the new one, or something else?

Lines 591-592: Please elaborate on this statement.

Line 598: I expect that biases in NO<sub>x</sub>, PAN, etc. can strongly impact the comparison

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of the simulated oxidants, so I think this point deserves more discussion in the body of the text. Also, Fig. S2 shows that the MOPITT-DA makes H<sub>2</sub>O<sub>2</sub> worse, and this should also be mentioned.

Line 659: Can you speculate on why the emissions are underestimated?

Line 698: Aren't VOCs also higher in this simulation? If so, how do you attribute the ozone production to CO rather than other VOCs?

Line 746: The definition of the tags needs to be explained somewhere.

Line 777: Does this mean there is a stratospheric intrusion reaching all the way to the surface? Or just more stratospheric/upper tropospheric influence somewhere in the profile?

Fig. 9: It would be helpful to include panels with the CO profile here so readers don't have to keep referring back to Fig. 8.

#### Editorial Comments

Line 204: "efficient" or "effective"?

Line 596: Sentence needs rewording

Lines 652-654: Sentence needs rewording.

Line 766: It would be clearer to avoid the parentheses and say lower OH and higher ozone.

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