

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2021-1035

Anonymous Referee #2

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Referee comment on "Discrepancy in assimilated atmospheric CO over East Asia in 2015–2020 by assimilating satellite and surface CO measurements" by Zhaojun Tang et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1035-RC2>, 2022

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### General comments:

This manuscript presents results from data assimilation experiments using surface CO observations and satellite CO retrievals. The assimilation system is based on GEOS-Chem model and Kalman Filter approach. The CO analysis fields from two data sources are compared and discussed. The authors found the discrepancy between assimilating surface observations and satellite retrievals and concluded that this discrepancy reflects the different vertical sensitivities of satellite and surface observations to CO concentrations in the lower and free troposphere. The comparative study of assimilating surface CO observations and satellite CO retrievals is of interest to inverse modeling and air quality forecasting communities. However, the manuscript in the current shape seems to me incomplete and lacks the scientific depth. The evaluation of both the efficiency of assimilation method adopted and the accuracy of analysis CO fields is not given in the manuscript, making the results and conclusions less convincing. The differences are analyzed and discussed in details, but what they imply for the better use of both surface observations and satellite retrievals and what strategy could be used to improve the assimilation is not provided. More detailed analysis and extra experiments are required to reach robust conclusions which can truly contribute to a better understanding of assimilating CO observations.

### Specific comments:

Line 1: This title is too general and should be specific to what you found or what you conclude.

Line 21-29: Why use mol<sub>c</sub>/cm<sup>2</sup> as the unit for column CO other than ppb the same as that for surface concentration? Suggest to use the same unit for column and surface CO. Only the inconsistency of column CO is emphasized. Does that mean your study focus on the

investigation of column CO?

Line 74-79: The logic here is not complete. The authors provided descriptions of literatures on assimilating surface CO observations and satellite CO retrievals and pointed out the unsolved problems in the fields. But it is still not clear why the comparative study could help us get closer to solving the exiting problems in assimilating CO observations.

Line 103-109: How is the quality of MEE CO measurements? The information on the accuracy of those measurements should be provided and discussed.

Line 132: The ensemble Kalman Filter and 4D-Var are commonly implemented assimilation techniques to account for the complexity of atmospheric transport and chemistry. I doubt the capability of simple scheme such as sub-optimal Kalman Filter to assimilate CO observations efficiently. The authors should provide evidence on the legitimacy of adopting sub-optimal Kalman Filter approach. BTW, the authors should provide more detailed description on assimilation setup such as initial conditions, DA time window, and how the forecast steps are advanced, etc.

Line 157-157: Why profile and column data were used to produce two types of boundary conditions? As stated in the later paragraph, the differences between two approaches are small, why not just stick to results from just one approach to do the analysis?

Line 170-173: Since the boundary conditions were produced by assimilating MOPITT CO observations using a global assimilation system, theoretically, assimilating the same MOPITT data over a regional domain are supposed to produce more details on CO field spatially and temporally and there should not be relatively large difference for regional averaged column CO. So where is this marked enhancement coming from?

Line 194-195: If the boundary conditions used in assimilating MEE data the same as those used in assimilating MOPITT data? If not, the authors may have to account for how much differences produced by using different boundary conditions. The MEE observations were averaged over a  $0.5^{\circ} \times 0.625^{\circ}$  model grid. So the representative information specified for those stations might be smoothed out to a certain extent. Thus the specified representative error might not correspond with the model grid.

Line 197-199: Data assimilation is expected to improve the agreement between observations and model simulations. The independent observations should be taken to evaluate the assimilation results.

Line 229-236: Does the modeled CO concentration refer to CO concentrations analysis? In

that case, the ratio of 1.5 is still relatively large, suggesting the observations are not efficiently assimilated. There are fewer observation stations in less polluted area. Consequently, the adjustment of prior CO concentration should be small, giving rise to large ratios up to 6 over less polluted area. Overall, those ratios just indicate that the prior CO concentrations are severely biased and an efficient correction scheme should be devised before doing the assimilation.

Line 245-246: It is not appropriate to use the ratios from fig.1 d to remove systematic background biases since the assimilation results are used to computer those ratios. BTW, why the observations are scaled other than prior CO field? Suggest to identify the possible systematic biases through the evaluation of simulations results from forward model run against observations.

Line 252-253: Is the PBL height the dominant factor regulating the variability of meteorological conditions? Please explain.

Line 255-261: Does the assimilation using normalized surface CO performs better than that using raw surface observations? The authors should give more discussions so that readers could judge the validity of the normalization approach.

Line 267: The description and discussion of the evolution or trend of CO over E. Asia is relevant only if the authors can show their assimilation results are solid enough. At the current stage, I suggest the authors also to check trends of surface observations and MOPITT CO retrievals and compare with assimilated trends.

### **Technical corrections:**

Line 71: Replace "sufficient" to "sufficiently".

Line 77-81: Please check the consistency of this sentence.

Line 97-98: "radiance" can not measure "radiation". Please correct.

Line 105: "criteria pollutants"? Please check.

Line 107: "reference state" or "reference temperature"? Please check.

Line 162: In figure 2, suggest to refine the color scheme of lines to make the different lines more distinguishable. The suggestion also applies to lines in other figures.

Line 165: In Table 1, suggest to use ppb for the unit of column CO. How is the "MEE CO(normalized)+MOPITT column" experiment conducted? It is not explained and discussed in the manuscript. BTW, the results from last two rows are the same, please check.

Line 193: "impacts" of what? Please check.

Line 220-221: Please check the grammar and rearrange this sentence.

Line 243: Replace "account for" to "to account for" or use a more appropriate phrase.