

Interactive comment on “On biases in atmospheric CO inversions assimilating MOPITT satellite retrievals” by Yi Yin et al.

Anonymous Referee #2

Received and published: 15 May 2017

General comments:

The submitted paper aims to evaluate the quality of CO emissions inversions and inference of other parameters influencing the CO budget using different model configurations, i.e. horizontal and vertical resolution, OH forcing and physics scheme. While the authors try to provide a detailed evaluation, and provide indications of possible biases on MOPITT retrievals and additionally of model and emissions, the methodology and scientific argumentation employed is not sound to me. It is quite unclear what are the exact goals of the paper and it seems that the focus is too broad. The evaluation is extensive which is appreciable but the discussions try to cover too many topics without robustness and convincing arguments. This study needs to mature and needs to be supported by additional experiments. I fear I cannot recommend this paper for publication for the following main reasons:

1. The discussion and especially sections 5.3 and 5.4 are scientifically flawed. The authors drive conclusions without experimenting themselves. I strongly recommend the authors to reconsider their data assimilation experiments and setups before driving such conclusions or consider removing those two sections. I fear that without those two sections the paper will significantly lose substance. Moreover, the sensitivity tests on model parameters are not convincing, a significant increase on model horizontal resolution and using a more detailed chemical scheme would have been more useful to point out intrinsic model deficiencies and uncertainties.

2. The quality of the scientific argumentation can be questioned. A lot of references are cited inappropriately. Number of citations do not support statements made in the present paper (see specific comments below). Demonstrations are often approximate and hand-waving. The conditional form is often used when it comes to conclusions (the forms “would” and “could” are widely used). The authors suggest and anticipate from incomplete set of experiments with few references to drive scientific conclusions.

3. Last but not least, I am concerned about the methodology itself; statistical methodology and the significance of the diagnostics. The reliability of the data assimilation algorithm is not discussed as well.

To support the three mentioned points please consider the following specific comments.

Specific comments:

Line 66: There are also other references that are using MOPITT and data assimilation to study the temporal distribution and variability of CO, e.g. Inness et al., 2015, Myazaki et al., 2015, Barré et al., 2015.

Line 75: Please, change plagued by another word. Models are not plagued, they just misrepresent the truth by man-made simplifications.

Line 83-84: This is not what Hooghiemstra et al., 2012 are proving. Form the conclusion of the paper it is: “However, in the remote SH (30 – 60° S), the comparison

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with MOPITT deteriorates from a 4% negative bias in the a priori to a 10% negative bias in the a posteriori solution, due to an emission decrease suggested by SH surface observations.”

Line 86: Gaubert et al., 2016 is not inverting surface emissions as Yin et al., 2015.

Lines 84-87: This statement is not well supported by either reference provided. For example, Barré et al., 2015 that is assimilating two types of sounders find opposite conclusions. MOPITT assimilation still underestimates CO at the surface over CONUS. This is probably only true in the southern hemisphere.

Lines 87-90: This statement is not clear at all. Please clarify.

Line 88: differences between what and what?

Line 90: While the statement is unclear to me, I do not think this is the Jiang et al., 2015 conclusions.

Line 135: The authors should know what Bayesian means. There is nothing Bayesian in this equation.

Line 140: Clarify the statement, it sounds as you model a profile from measurements.

Line 176: 2.5 by 3.75 degrees is now considered as low resolution, change accordingly.

Line 179: is it another model? I believe you still use LMDz but with a different configuration. Change accordingly.

Line 181: Does changing just the latitudinal resolution from 2.5 to 1.89 degrees relevant? It is then mainly a significant increase on the vertical resolution. Why not keeping the same horizontal resolution? Again 1.89 by 3.75 by 39 levels is not considered nowadays as high resolution.

Lines 199-200: 2009 to 2011? From what month to what month? It could be almost three years to almost one year though.

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Lines 316-317: The authors should detail exactly how they apply the observation operator to retrieve Xmod. Have they smoothed the model profile by the averaging kernel, have they considered interpolating partial columns from the model and then convert to $\log(\text{vmr})$ to match the MOPITT data? The authors should refer to Barré et al., 2015 section 2.2.4 for the correct approach. I am then uncertain if the method used by the author is the correct one, hence I am doubtful about the validity of the results and discussion about the MOPITT profiles validation in rest of the paper.

Lines 323-324: Does this mean that you are taking the nearest grid point. If yes, is that appropriate? Since you are doing DA science you should be able to interpolate at the right location.

Lines 325-328: This is unclear to me, what operation the authors are doing here. Are you shifting or scaling the profile in order to keep the same total column value? What is the “uncertainty from vertical resolution change on the CTM”? Please rewrite, develop, explain better.

Lines 330-333: It is unclear to me what the authors are doing exactly. Are they averaging monthly model values and then they are comparing with monthly averaged observations? If yes, the entire results of this paper would be flawed. Or are they interpolating model to observation at the right time. Moreover, it seems that the correlations in the rest of the paper are made on monthly averaged biases, reducing the sample for correlation to something small and probably not statistically significant. Looking at the correlations plots I see around 12 to 14 point as a sample size. Would it be statistically more sound to calculate those correlations using the entire sample of observations (not reduced by average biases)? I am then doubtful about the robustness of this score during the further analysis of this paper.

Line 358: Please recall what are those big-regions. Cite Yin et al., 2015.

Line 370-376: This entire paragraph is confusing to me, please rewrite.

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Line 427: Higher sensitivity of what to what?

Line 432-433: Please see my comment above about the significance and robustness of those correlations. The statistical methodology as it is presented now is not sound to me.

Line 443-445: This is again confusing with a “hand-waving” argument displaying only the HR correlations, using the word “likely” and not further investigating the possible error on the vertical error CO profile on the posterior. Additionally, I would not trust a correlation of 0.49 with a sample size of 14 using monthly averaged biases.

Line 469: The word contamination is not appropriately used. For example, there is contamination in data when an instrument is not working correctly and generate a systematic error. Please replace this word.

Lines 474-479, lines 506-513, figures 4 and 5: MOZAIC profiles are most likely close to the sources whereas HIPPO measures remote scenes. The bias observed in the posterior for HIPPO profiles are due to an overly long CO lifetime in the simplified chemistry model. The presented data assimilation system infers the surface CO sources but do not directly corrects for CO lifetime error due to an (over) simplified chemical scheme.

Lines 531-532: Please rephrase. “The MOPITT profiles are well reproduced by the model. . .”. The model does not reproduce MOPITT profiles.

Lines 541-544: In Deeter et al., 2014, the MOPITT V6 validation with HIPPO do not see such errors in the upper troposphere. Also, the author should also take into account the spatial sampling of MOZAIC, HIPPO versus MOPITT. The longitudinal distribution of CO, in the tropics can be highly variable.

Line 565: Which tropical ocean? Rephrase.

Line 574: “over the ocean”

Line 607: What are those big regions? Recall or cite Yin et al., 2015.

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Lines 638-643: What is the purpose of this paragraph? It is not clear what the authors are trying to demonstrate. Please clarify, develop, rephrase.

Lines 649-652: The syntax of this sentence is not correct.

Lines 683-695 and section 5.3 in general: The conclusion of “positive biases in the MOPITT retrievals” is flawed here. The authors utilize only one inversion technique from only using total column product. They infer only the surface emissions that is not a direct measured quantity from MOPITT CO retrievals. Depending of a model quality (i.e. resolution, chemistry, horizontal and vertical transport, and so on. . .) inverting the emissions only can lead to good result for the wrong reasons and conversely often having the “correct” emissions and having significant errors in the atmosphere. Data assimilation rely on observation but ALSO on models, you could have the best observation quality, if the model is inaccurate the analysis and the subsequent forecasts would be degraded. Before jumping quick in such important conclusions several things should be tested carefully such as:

Assimilating the CO fields directly with total CO columns and CO profiles Rerunning the current experiments with a more complete and detailed chemistry

Line 692-695: Deeter et al., 2014 made the direct comparisons between MOPITT V6T (which is the same as MOPITT V6J over the ocean) and HIPPO measurements: providing a quantification of the MOPITT biases: 1.5% 7.7% at the 200hPa level. How can the authors can explain such discrepancies with those results and figure 4 and 7. The authors compare figure 2 and 4 with figure 7. Again, the representativity of the statistics made here should be considered. HIPPO and MOZAIC cover specific regions whereas MOPITT provide a global picture. Is it reasonable to compare those figures in order to drive conclusions about biases without quantification?

Lines 702-707: This indicate an issue in your CO lifetime (see comments above). I would suggest having an estimate and quantification of your CO lifetime and budgets (e.g. like in Gaubert et al., 2016). This will help you investigating and quantifying what

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is responsible for the biases in the posterior: MOPITT retrievals, LMDz or the 4DVar.

Lines 709-714: This statement now refers to MOPITT V5T, the rest of the paper is dealing with MOPITT V6J. This is confusing and probably not relevant.

Lines 713-714: The reference to the George et al., 2015 paper is misleading. It makes think the reader that is it a paper about MOPITT biases regarding IASI as a reference. This is not the goal and conclusions of George et al., 2015. Please remove, or rephrase. For a data assimilation comparison between MOPITT and IASI CO profiles, please refer to Barré et al., 2015.

Lines 721-739 and section 5.4 in general: This paragraph is not clear and to my mind drives conclusion without the necessary convincing experiments. The authors “anticipate” that assimilating CO profiles would produce larger biases. Why the authors did not assimilate the profile and then not just “anticipate” but prove this conclusion. I recommend either removing section 5.4 or provide the necessary experiments to support such conclusions.

The authors only support their conclusion by citing papers not accurately that are not using the same model and data assimilation system and experiments. For example, Gaubert al., 2016 do not infer the CO surface emissions.

Lines 749-754: The authors point out a well-known problem in chemical data assimilation. This can be overcome by using eigenvalue or more generally singular value decomposition to diagonalize R and avoid calculating off-diagonal terms of B (e.g. Migliorini et al., 2008) in a variational framework. Alternatively, approximation and ad-hoc assumption can be made on off diagonal values of B or assuming that R is diagonal and tuning diagonal values of R.

Lines 755-760: That is a shame that at the very end of the paper (and few other lines i.e. around line 665) is it stated that the sources and sinks of CO on the model could be responsible of the biases in the posterior analyses. I recommend that this should

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be reinforced in the entire discussion by having further diagnostics and experiments.

Lines 760-765: I am again not sure about the validity of those statements. There is a difference between assimilating surface network data and assimilating surface retrieved data. The representativity of these two types of data sets are fundamentally different, e.g. coverage, revisit/time-sampling, accuracy, spatial resolution. This is again very speculative, consider removing.

Line 767: "On either the satellite", syntax error, please rephrase.

Lines 776-777: Barré et al., 2015 conducted a study assimilating MOPITT and IASI and compared biases and errors with an extended set of independent observations for validation. Please refer to this paper.

References:

Barré, J., Gaubert, B., Arellano, A. F. J., Worden, H. M., Edwards, D. P., Deeter, M. N., ... Hurtmans, D. (2015). "Assessing the impacts of assimilating IASI and MOPITT CO retrievals using CESM-CAM-chem and DART." *Journal of Geophysical Research: Space Physics*, 120(19), 10501-10529. DOI: 10.1002/2015JD023467

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