Reviewer #3:

Comments:

General Comments:

The authors provide a detailed analysis to constrain CO emissions with multi-satellite measurements in the period of 2000-2017. They demonstrated decreasing trends of anthropogenic and biomass burning emissions, and noticeable influences from the assimilation of HCHO on the estimation of oxidation sources. I found their paper is interesting and helpful for people in this field. I recommend the paper for publication after consideration of the points below.

Response:

We thank the referee for the positive and insightful comments. We have made a point-by-point response here. We have also marked the changes to the initial text in red.

Specific Comments:

1. Abstract: I am not sure whether the biased trends in the bottom-up inventories are still "surprising", as the bias has been found with inverse analysis several years ago.

Response:

We have removed the word "surprisingly" from the abstract.

2. Section 3.2.2: Will INV #1 and INV #2/3 have better agreement in wintertime, when the contribution from NMVOCs is smaller?

Response:

Yes. For example, Inv #1 and Inv #2 estimate the declining rates of $-1.6\%\ yr^{-1}$ and $-2.0\%\ yr^{-1}$, respectively, in annual anthropogenic emissions from China during 2005–2017. However, in wintertime (Jan–Mar) they estimate consistent trends of $-1.1\%\ yr^{-1}$ and $-1.0\%\ yr^{-1}$, respectively.

3. Figure 2b: As the largest difference is in China, it will be helpful to check whether the a posteriori simulations of INV #2/#3 match better with surface measurements in China outflow regions than that of INV #1.

Response:

We evaluated the a posteriori simulations with surface measurements over China and its outflow regions for Inv #1 (Figs. S5c, S5f), Inv #2 (Figs. S7c, S7f), and Inv #3 (Figs. S9c, S9f), respectively. These posterior simulations all correct modelling biases in the prior simulations, but Inv #2 and Inv #3 do not show significantly better performance than Inv #1. This is because the additional assimilation of $HCHO/CH_4$ increases the declining trend of anthropogenic emissions in China, but also increases the CO chemical production, which does not change the CO total source much.

4. Page 9, Line 17-18: "Therefore, it is reasonable to think that Inversion #3 *has a more realistic representation of the global CO budget than Inversion #2 does, and Inversion #2 is better than Inversion #1."*

It may not be as obvious as mentioned here. I agree the observations of HCHO/CH4 will be helpful to distinguish the sources from combustion and oxidation, however, why they will improve the *global CO budget? The assimilation of HCHO/CH4 will affect OH, but the ability of global models to simulate OH chemistry is still weak.*

Response:

The reviewer's point is exactly what we discussed here. The previous sentence (Page 9, Line 16–17) in this paragraph said that "Constraining the CO chemical production can correct the inversion system that may inaccurately attribute some of the decreases in the CO source to the CO chemical production". Just to clarify, we have rephrased the sentence that the reviewer is concerned with as follows.

"Therefore, it is reasonable to think that Inversion #3 has a more realistic representation of the source splitting between anthropogenic emissions and chemical production in the global CO budget than Inversion #2 does, and Inversion #2 is better than Inversion #1."

5. Figure 5b: the trends are generally positive in India and negative in the rest of SEA, which is surprising. I have assumed that they will be similar.

Response:

The trends of anthropogenic emissions are estimated to have been growing in Indonesia but declining over most of mainland Southeast Asia, broadly consistent with MOPITT CO trends (Fig. 1a). However, the drivers behind are not clear yet due to lack of regional bottom-up inventories.

6. Page 12, Lines 29-32: The validation with independent surface measurements is an essential part in this work. These figures should be included in the main text rather than supplement.

I found the numbers for different periods are compared directly, which will affect the reliability of the validation: INV #1 Figure S4c, 2000-2017 INV #2 Figure S6c, 2005-2017 INV # 3 Figure S8c, 2010-2017

In addition, the distributions of data points are very noisy. I cannot see any noticeable difference among those figures by my eyes.

Response:

We have moved Figs. S4, S6, and S8 into the main text to make it easier for readers to access them.

We have rewritten Lines 29-32 Page 12 as follows to validate Inv #1, #2, and #3 at the same period.

"The evaluation with measurement from WDCGG suggests that Inversion #3 gives a fair estimate of surface CO trends during 2010–2017 (NMB = −8%, RMSE = 1.4 % yr^{-1}, Fig. B3c), while Inversion #2 (NMB = −34%, RMSE = 2.0 % yr^{-1}, Fig. B2c) and Inversion #1 (NMB = −47%, RMSE = 1.8 % yr^{-1}, Fig. B1c) still present moderate biases in their study period. During the overlap period of 2010–2017 with Inversion #3, Inversion #2 and Inversion #1 both present slightly larger RMSE of 1.5 % yr^{-1} in the trend estimates."

The difference between these figures is marginal. Please refer to our response to the 3rd comment.

7. Page 14, Line 24: The author name in the citation.

Response:

This is not a citation but a reference to the year 2015. We have clarified the text.