

Interactive comment on “Global atmospheric carbon monoxide budget 2000–2017 inferred from multi-species atmospheric inversions” by Bo Zheng et al.

Anonymous Referee #3

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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.

Specific Comments:

1. Abstract: I am not sure whether the biased trends in the bottom-up inventories are

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still “surprising”, as the bias has been found with inverse analysis several years ago.

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

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.

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.

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.

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.

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

