

Interactive comment on “Sensitivity of inferred regional CO source estimates to the vertical structure in CO as observed by MOPITT” by Z. Jiang et al.

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We thank the reviewers for their thoughtful and detailed comments. Below we respond to the individual comments. In addition to the revisions discussed below, we made significant modifications to the manuscript. Four figures were removed to make the paper shorter and more concise. We have also moved the discussion about the optimization scheme and the validation of the MOPITT data to appendices to make the manuscript easier to read.

I think the paper can be published but should be shortened and checked for spelling

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mistakes. I thought the contrasting results on emission estimates of two different OH fields were quite interesting. It is not an overly long paper but sometimes it is not very concise.

(1) Line 10: "the CO emissions in the GEOS-Chem model and accounts for the vertical smoothing of the MOPITT retrieval". You mean equation (1) is applied to model profiles and smoothed by the averaging kernels? If so, please make a reference to equation (1). You should also include a single line sentence or short reference why the adjoint does not give you error bars in the posterior emissions estimates.

Thanks for your suggestion! The expression for the observation operator is analogous to equation (1). We have added the expression for the observation operator. We are also modified the discussion on the a posteriori errors.

(2) Figure 2: Maybe you should put forward a sentence or two about how the OSSE should ideally look. As far as I understand it, in a perfect world the grid boxes would have a color of yellow which corresponds to the number 1. But it seems to me that there are still a considerable number of grid boxes which are either much lower than 1 or much higher, respectively. It further looks like the OSSE does a better job at the equatorial regions. I think this needs particular consideration as your main results in the following sections adjust emissions to higher values in the extratropics. Did you include the distribution of the MOPITT observations in your pseudo observations and therefore the OSSE reflects the less stringent constraint at higher latitudes? If so please clarify in the text.

In the adjoint of GEOS-Chem, the cost function is minimized in regions where the gradients are the largest. According to Equation (A1-A3), grids with strong CO emissions, such as East Asia, India, equatorial Africa and South America, will have large initial gradients and thus will be optimized preferentially in the OSSE, which leads to more yellow color in the equatorial regions. The adjoint model will optimize the weak emission regions, after reducing the model bias due to the strong emission regions. We will

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see the effect (more yellow color) if we have more iterations in the OSSE.

We didn't include the distribution of MOPITT observations in the OSSE. The pseudo observations are the complete CO fields from model.

(3) Results and discussion: You need to go through the text again and make it shorter by at least 30%. The text does not have a very good focus.

Thanks for your suggestion! Four figures were removed to make the paper shorter.

(4) 3.1 Figure 3: Just a comment. Please tell the reader how are you smoothing the GEOS-Chem model output on the vertical grid. Are you just using 1 model grid box that corresponds to the actual altitude of GMD observation?

We didn't consider the actual altitude of GMD observations. Because the model resolution is coarse ($4^\circ \times 5^\circ$), there is clearly going to be large representativeness errors associated with the actual altitude of the GMD site vs. the model altitude. In this figure, we compare GEOS-Chem surface level CO concentrations with the GMD data. This figure has been moved to appendices.

(5) 3.2. Figure 4: Is this Figure 4 really needed here? Also, one could probably guess but there is no explanation if the "difference between two types of scaling factors" is the left panel minus middle panel or vice versa.

This figure has been removed.

(6) 3.3. Figure 5 and Figure 6: I do not know if this would be easy to do. If so, you should consider merging Figure 5 with Figure 6. Figure 5 is essentially the summary of Figure 6 on an annual scale. Again Figure 6 does not say if the "difference" is based on surface inversion minus profile inversion or vice versa.

Thanks for your suggestion! These two figures have been merged.

(7) 3.4 Figure 8: Is this Figure essential? I think everything is being said already in Table 1. However, you could expand Table 1 so that it includes all the seasonal values

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instead of Figure 8. On another note: I would also multiply the values in the color table of Figure 8 by 100. You also need to state the unit in the Figure legend. Table 1 shows the "Relative difference between surface and profile inversion" with the % unit?

Thanks for your suggestion! This figure has been removed.

Table 1 already has quite a bit of information. We feel that expanding it to include all the seasonal values as well would make it difficult to read.

(8) 4. Section 4.3 Ideal Tracer Experiments: The prior two sections 4.1 and 4.2, respectively are much too long but this section is rather short. You need to explain a little bit more what you exactly did here. You are saying you emit 3.33 Tg CO/day in each of the defined tagged regions, and 1/30 of that is being destroyed after each simulation day?

As the reviewer indicated, we emitted 3.33Tg CO/day and imposed a loss rate that is given as $[CO]/\tau$, where $[CO]$ is the concentrations in molecules/cm³ and τ is the timescale for loss, which is 30 days. The description has been modified.

(9) 4.1 I think (if easy to do) Figure 10a and Figure 10b could be combined into 2 useful single Figures for $P > 700$ hPa and $700 > P > 250$ hPa, respectively. As far as I understand every subfigure in Figure 10 tells the story for each individual tagged tracer. But it is also obvious from the Figures that the partial columns of the tagged tracers are confined in the individual tagged regions (although there may be some overlap through to transport).

Thanks for your suggestion. We decided to remove Figure 10b. It was not critical to the discussion in that section.

(10) 4.2. Figure 10a and 10b It is not really clear from that Figure or from the text in section 4.3 what the actual units of the color bars are. 4.3. Table 2 Again how do units in Table 2 relate to Figure 10a and 10b, respectively? And what is "Upper fraction (%)"?

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The unit in the figure is 10^{18} molec/cm². In Table 2, we calculated the monthly mean mass of continental CO tracers. The “upper fraction” is calculated by $\text{Mass_upper} / \text{Mass_total}$. The captions have been changed.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22939, 2014.

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