Interactive comment on "Analysis of CO in the tropical troposphere using Aura satellite data and the GEOS-Chem model: insights into transport characteristics of the GEOS meteorological products" by J. Liu et al.

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We thank the three reviewers for their comments. Two recommend publication with minor (Rev. 3) or no (Rev. 2) revisions. Reviewer 1 raises a number of issues that we respond to in detail below. We note that there are several misunderstandings on the part of this reviewer, so we have clarified the text in the relevant sections. We agree that our paper is long, with many figures, but this is because it is larger in scope than many papers; to quote Rev. 2 and Rev. 3, "it is intelligent, comprehensive, and in-depth". Each figure that we included is there for a purpose.

In the following, we address the concerns raised by all the reviewers. Reviewers' comments are italicized.

Review 1:

1): Start by removing the global picture in sections 4.1 and 4.2. Figures 3 and 4 are too small and most of the details are repeated later in the paper.

We cannot remove these two figures as suggested by the reviewer for two reasons:

I. These two figures are crucial to highlight the spatial characteristics of the discrepancies between model and observations; they are not global, but show the tropics (30 N – 30 S), the focus of this paper. They set up the rest of the analysis that follows. The spatial patterns are not shown later in the paper.

II. Details of the figures are readable when one figure is printed on one page.

2): Skip the source attribution. Figures 8, 17, and 21 are again unreadable, and you should limit the discussion. When important you can mention (some) details in the text.
We disagree with this comment.

I. In this paper we use the satellite data to test the ability of the models to simulate the processes influencing the distribution of CO. We identified that vertical transport in the model contributed to the discrepancy between model simulations and the observations in the timing of the CO maxima in the upper troposphere in South America. However, the vertical transport problem could not account for all the discrepancies between model and observations. The analysis of source attribution was essential to diagnose other problems in the model simulations, for example the isoprene source over South America which is too high.

II. The tagged tracer runs are a standard method used to interpret modeling studies. CO is sufficiently long-lived that emissions from one region influence others, for

example the effects of CO from fires in southern Africa influencing northern Africa. We show the source contributions in the right hand panels of Figures 8, 17, and 21 as they underpin much of our analysis and allow us to determine the reasons for the discrepancy between model and observations.

3): Avoid the discussion about the averaging kernels. Sampling without taking into account the averaging kernel is simply wrong and your aim should be to produce readable figures with less lines.

We agree with part of the reviewer's comment. We apply the averaging kernels (AK) to properly compare the model and satellite data. We include the model results without the AKs for two reasons. First, the sensitivity of the TES instrument to CO changed after the bench warm-up in late November 2005, so the time series of the model results with the AKs can easily be misunderstood, in particular the comparison of 2005 and 2006 over South America and southern Africa. Second, the tagged CO results used for source apportionment can easily be compared to the model results without the AKs. When they are full-size, our figures are clear.

4): Apart from these recommendations, I have some questions about the results that should be clarified. First of all, the authors should mention the CO sink that potentially plays an important role. The OH levels may be off in a full chemistry simulation and the best way to verify the levels is to quote the methane and methyl chloroform lifetimes.

Model OH indeed influences the CO distribution, but given the long lifetime of CO, even in the tropics (at least a few weeks), it is the timing of the biomass burning source and of vertical mixing that are dominating the time series seen in the upper troposphere above the various source regions. The methyl chloroform lifetime with respect to removal by tropospheric OH is 5.9 years in our model driven by GEOS-5, and 5.5 years in our model driven by GEOS-4, which are consistent with lifetimes derived from analysis of data for methyl chloroform (e.g., Prinn et al., 2001, 2005).

5): Second, the simulations are done in a rather coarse resolution. The process studied is quite delicate, since the interplay between convection and emissions around the ITCZ is known to be difficult to describe numerically. An assessment of resolution effects is therefore required.

Although the simulations in the paper used resolution of 4x5 degrees (latitude by longitude), the native resolution of the meteorological fields that we regridded is 1x1.25 (GEOS-4) and 0.5x0.67 (GEOS-5), as was stated Section 2. We have conducted model runs at 2x2.5 in GEOS-Chem, and the results were almost identical when aggregated to the regions analyzed here.

6): Third, the conclusion that GFED-2 biomass burning emissions are too low may be related to the fact that the vertical mixing in both GEOS-4 and GEOS-5 may be too slow or shallow to bring the CO upward. Interference with the resolution and the OH field may also play a role. In other words, the conclusion may depend strongly on the model used in the study.

Indeed, conclusions about the magnitude of CO sources depend on model transport, and this has been shown in the inversion study of Arellano et al. (2006) which we cite. In terms of evaluating the model right over the source regions, the lifetime of CO is too long for potential problems with OH to have much effect. However, the conclusion that GFED-2 biomass burning emissions are too low in Africa is found for both GEOS-4 and GEOS-5 meteorological fields, and in a simulation with the LMDZ-INCA model, the paper we cite by Chevallier et al. (2009). A comparison with different emission inventories also suggested that GFED2 CO emissions are too low over Africa (for example: comparison with AMMA inventory by Liousse et al. (2010) and comparisons three other bottom-up inventories by Stroppiana et al. (2010). The CO source derived for Africa using the MOZART model (Petron et al., 2004) is also lower than the GFED2 emissions, as noted by Stroppiana et al. (2010).

7): When I have a close look at figure 12 (and 16) I observe that in GEOS-5 the surface vertical mass-flux goes to zero. Moreover, the mass fluxes for GEOS-4 and GEOS-5 differ substantially and the true convective mass flux profile may again be substantially different. Maybe the authors should test more vigourous vertical mixing (or less vigourous mixing in Africa) like they test enhanced biomass burning emissions.

The GEOS-4 and GEOS-5 models have different vertical resolution, particularly in the planetary boundary layer (PBL), and use different convection schemes, so yes, the convective mass flux profiles are different, particularly near the surface. In this version of the GEOS-Chem model, the emissions are mixed throughout the PBL, which will mitigate the differences in vertical mixing by convection.

There is no way to properly adjust the vertical mixing in a chemical tracer model (CTM) that uses archived meteorological fields. The fields are from a GCM assimilation run that has self-consistent convection and advection. Changing convection in the GCM would inevitably change other aspects of vertical transport. Lawrence and Salzmann (2008) give a nice discussion of difficulties of interpretation that arise in adjusting convection in a CTM with archived fields.

8): I think to authors claim too strongly that the flaw of their simulation can be attributed to wrong biomass burning emissions. Model errors and also errors in (absolute) satellite retrievals (remember that MLS has to be scaled down considerably!) interfere with emission errors. In that respect I could not agree more with the authors on the last page (19659), where they clearly indicate that caution is needed in inverse modelling approaches in which traditionally model-measurement differences are attributed to emissions only. In that respect, I think there is not much wrong with this paper scientifically. But the authors should narrow down their message to a few key points.

We glad that the reviewer thinks “there is not much wrong scientifically” with our paper. We think that the detail in our paper is necessary to identify the causes of discrepancies between model and observations, particularly in the context of the timing of the maxima in the upper troposphere (UT). We are not attributing the problems with timing of these maxima to errors in the magnitude of biomass burning emissions, but to problems with transport and with isoprene emissions (for South America). We use the MLS data to

show the timing of the UT maxima, so in this respect the scaling is not a problem. We do not use the MLS data to comment on the GFED2 emissions.

1 Other Issues

page 19634, line 23 I remember a MINOS paper by Lelieveld et al. that addresses this issue.

Thanks for pointing out this paper. We now cite it in the paper.

page 19635, line 4 I miss a clear definition of UT and LT. What ranges are exactly meant here? Please define.

In Section 4.1, Lower Troposphere, we show TES data and the model results at 681 hPa, while in Section 4.2, Upper Troposphere, we show results at 215 hPa. We are using a pressure around 700 hPa to represent the LT, while we use the MLS data at 215 hPa (and 146 hPa for the time series plots) as the UT. We also refer to the middle troposphere (MT) on p. 19644, line 17 as 400-500 hPa, and we refer to the levels around 400 hPa in the time series plots as the MT. There is no formal definition of lower, middle, and upper troposphere (and definitions in the literature are somewhat arbitrary), and we do not feel that there is any ambiguity in our paper. Clearly we use TES data for the LT and MT, and MLS data for the UT, and the levels used are given in all our plots.

page 19636, line 1 I would rather use "thorough evaluation" instead of "useful tests".

Changed.

page 19637, line 16 CO emissions ..... add "from burning".

We are describing Figure 1, which shows biomass burning emissions, so the suggested change in not needed and would be redundant.

page 19637, line 18 I think such a detailed description of the emission model is not necessary here..The paper becomes too lengthy

Unfortunately, this information is not in other papers using GEOS-Chem, as we are using a relatively new implementation of MEGAN v2.1, so we are required to include it.

page 19638, line 16 NOx concentration .... add "fields".

Changed.

page 19640, line 1 Again: too detailed.

The validation paper of TES based on the comparison with aircraft data is for TES V002 data. We use TES V003 data in our paper, so it is necessary to state clearly the information on data quality.

page 19641, line 9 I think that these sections can be removed. The pictures are too many and too small and many of the issues are reiterated later.

Please refer to the first response above which explains why the figures and discussion are necessary.

page 19644, South America: I would leave out the averaging kernel results from the plots.

Please refer to the second response above.

page 19645, line 12 ..moisture and latent heat flux....I do not see what is the difference between moisture and latent heat flux. I think the authors (Fu?) suggest a build-up of moisture, due to a latent heat flux?
We agree with the reviewer's comments and changed the text appropriately.

page 19646, line 18: Please also define MT (see above).
We already defined the pressure level of middle troposphere on page 19644, line 16.

page 19649, line 23: ..The low surface emissions in the model....This assumption might be related to the model at hand. I might also be that convection simply does not bring enough CO upward.
Please refer to the sixth response above.

page 19652, line 21: ..deficiencies in the GFED2 emissions. I am not convinced here. I really wonder what will happen at the surface (Figure 2) with these higher emissions. Also, a strong interplay with OH can play a role (see main comment).
Please see the sixth and fourth responses above.

page 19654, line 21: TEJ?
Added in the paper: Tropical Easterly Jet

page 19659, line 28: ...to estimate source uncertainties. I think the authors mean ....to estimate sources (and their uncertainties).
What we mean is: to estimate the uncertainties of different sources.

Review 2:

We thank this reviewer for the favorable comments.

Review 3:

1). P19637, lines 15-16: South America was much drier in 2005 than in 2006. This is mainly due to warmer sea surface temperatures (SST) in the north tropical Atlantic (see Marengo et al. 2008, J. Climate, Zeng et al. 2008, Envir. Res. Let), not due to La Nina in 2005. Similarly, on P19646, lines 13: An El Nino tends to suppress convection and induce an anomalous subsidence over Amazon. The stronger vertical mixing in 2006 compared to 2005 is probably due to weaker convection in 2005 caused by warmer SST in the north tropical Atlantic.

Thanks for pointing out these papers, which we now refer to. We have changed the text as follows (line numbers refer to the ACPD paper):

I. P19637, Lines 15: "In South America there was a drought in southwest Amazonia in 2005 caused in part by anomalously warm sea surface temperatures in the north tropical Atlantic (Marengo et al., 2008; Zeng et al., 2008), and CO emissions from fires that year were almost twice those in 2006."

II. P19646, lines 13: “In 2005 the observed CO maximum occurs one month later at 215 hPa than at 681 hPa, while in 2006, the observed CO maximum occurs in September from 681 hPa to 215 hPa, implying stronger vertical mixing in 2006 than in 2005. Convection may have been weaker in 2005 because of the warmer sea surface temperatures in the north tropical Atlantic (Marengo et al. 2008; Zeng et al., 2008).”

2). Figures 3, 4, 5 and 6: there are too many panels in these figures so it is difficult to see them clearly. I suggest dropping a half of the panels since they are not discussed in the paper, and also label the month of each panel.

We have not changed these four figures for the following reasons:

I. These figures are crucial to highlight the spatial characteristics of the discrepancies between the models and observations.

II. We discuss many of the individual panels, and all the months are necessary to show the temporal evolution of the spatial features of the CO distribution that we analyze. In addition to the discussion of these figures in Section 4, we refer back to them in Section 5.

III. Details of the figures are readable when one figure is printed on one page. The figures already have the months labeled in the upper left of the left hand columns.

3). Fig. 8: Since the four panels in the right column are not discussed until after discussion of Fig. 12, I suggest naming these four panels as Fig. 13.

Indeed, the discussion of right-hand columns of Figure 8 appears later than that of the left-hand columns. However, we think this is the best arrangement of figures and discussions. With the present figures, the reader can easily compare the temporal variation of total CO and its sources contributions, which helps their understanding of different source contributions to the CO variation.

4). Because there are so many figures and so much detailed discussion for each region in Section 5, I suggest adding a brief summary at the end of discussion for each region to help reader see your main points before they reach the conclusion section.

Our paper is long enough already, and Section 6 (discussion and conclusions) includes a brief summary of each section.

5). Section 5.4: Could you explain why CO over Indonesia is similar between a non-El Nino (2005) and an El Nino (2006) year over Indonesia in Fig. 25?

This is not the case. Figure 25 shows much higher CO over Indonesia in October-November of 2006 compared to 2005 as a result of the major fires there. This is also obvious in Figure 3-6.

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