Response to reviews on "Transport analysis and source attribution of seasonal and interannual variability of CO in the tropical upper troposphere and lower stratosphere"

by Junhua Liu et al.

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We thank the reviewers for their comments. The reviewers raise a number of issues that we respond to in detail below. We note that there are several misunderstandings on the part of the reviewer 1, so we have clarified the text in the relevant sections.

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

Anonymous Referee #1
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A): It is not clear what conclusions are new in this analysis. Much of what is presented seems to have already been shown by Liu et al. (2010), Duncan et al. (2007), Liu et al. (2007) and Schoeberl et al. (2006). In the introduction, the authors state that the present work "builds on our earlier study", but they don't even discuss the findings of that study. I recommend that the authors discuss the conclusions of Liu et al. (2010) and clearly state how the work in the current paper is different and new. The two papers have very much the same "look and feel". In fact, the first sentences (i.e., the purposes of the manuscripts) of the two abstracts are very similar.

We disagree with this comment for the reasons given below. As recommended by the reviewer, we have clarified in more detail the focus and conclusions of Liu et al. (2010) in the Introduction section, so the reader can see more easily how the present paper builds on that work.

In Liu et al. (2010), we assessed transport pathways of CO in the tropics with a focus on understanding the interplay of convection, long-range transport, as well as CO sources, on the redistribution of CO in the troposphere during and immediately after the biomass burning seasons on the different continents. Our evaluation of transport in CTMs driven by GEOS archived fields was based on the discrepancies we identified between model and satellite data, on a continent-by-continent basis, in the lower and upper troposphere. In this paper, we examined the controlling factors on the seasonal and interannual variability of CO "tape recorder" in the upper troposphere and lower stratosphere. We used the CO tape recorder pattern in MLS data to deduce an independent constraint on vertical velocities in the UT and to provide a quantitative evaluation for GEOS transport in the UTLS. The topics are different between these two papers, in that the first focused on transport of CO from the lower troposphere to the upper troposphere, and this one on transport in the UTLS.

We said that this paper builds on our earlier study because one of the controlling factors forming the "head" of the CO tape recorder is the seasonal variation of surface biomass burning, and the lofting of these emissions by convection, which was examined in Liu et al. (2010).

ACPD routine performs a similarity check during the submission process. We note that the report shows that the total similarity index is 9% and the similarity to Liu et al. (2010) is only 1% (6% with Liu et al., 2010, ACPD).

B) The importance of the main findings is not discussed. Is it important from a chemical or climate perspective for a model to simulate CO well in the UTLS? What are the implications of too slow transport through the UTLS? Are there consequences for other important trace gases?

Yes, there are consequences for important trace gases. Gases enter the stratosphere through upward transport in the tropics, as part of the Brewer Dobson circulation, and the topic of transport in the UTLS has been the focus of a major research effort in atmospheric dynamics and chemistry, with new research summarized in a series of major review papers (Holton et al., 1995; Gettelman and Forster, 2002; Fueglistaler et al., 2009), and sessions at almost every AGU meeting for a number of years. Most observational constraints on vertical transport are for the region above ~100 hPa (~17 km) as we discuss in the paper, and we argue in this paper that CO provides a useful constraint on vertical transport from ~200 to ~100 hPa. We modified the Discussion section of the paper to emphasize the importance of our main findings.

The fact that a model does not simulate the tape recorder CO well in the UTLS definitely has implications for the ability of that model to simulate other gases, for example ozone. Simulations with the GMI model driven by GEOS-4 and GEOS-5 show that ozone is too high in the lower tropical stratosphere when driven by GEOS-5 meteorological fields (and less so with GEOS-4), consistent with too slow upward transport (J. A. Logan, unpublished results). This is entirely consistent with our findings from the analysis of CO in the UTLS.

C) There is no discussion on the limitations of the data for the purposes of this manuscript. For instance, does the data's vertical resolution influence what can be concluded on vertical transport through the UT/LS? Please include a discussion on the precision of MLS and the statistical significance of the averaged data. Here are some examples of my recommends to address this concern, which generally apply to all figures: Figure 1. Add uncertainties to the MLS data. Add a discussion on the number of points that went into the average and the statistical significance. Are these monthly averages?

<u>MLS Precision</u>.

We have added information on the precision of individual and averaged MLS data to the discussion of the various figures in the revised paper. We provide a summary below.

We show the MLS data as monthly averages, and we have clarified this in our figure captions. Livesey et al. (2012) state that the precision of individual MLS v3.3 CO observations at 215 hPa is about 20 ppbv (1σ). The mean precision for individual MLS v3.3 CO profiles in the UTLS from 215 hPa to 68 hPa ranges from about 20 to 15 ppbv (see Table 3.6.1 in Livesey et al., 2011). For the figures showing the zonal monthly means for 10°N-10°S (Figures 1, 7, 11), each monthly point is an average of ~9500 observations, so this improves the precision to ~ 0.15 - 0.2 ppb, much smaller than the variability observed in MLS CO monthly means. The standard errors for the MLS monthly zonal means are very small (0.05 to 0.6 ppb) compared to their absolute values (tens of ppb), so we did not include these in the time series figures as they would not show.

Each monthly zonal mean point in Figure 4 (every 2° in latitude) is an average of ~950 observations, giving a precision of ~0.5-0.7 ppb. Each daily mean point in Figure 12 is an average of ~317 observations, for a precision of ~0.9-1.1 ppb. Each monthly mean point (2°x2.5° grid) in Figures 2, 5, 6, 8 is an average of ~7 observations, so the precision is ~ 5.7-7.6 ppb, still smaller than the spatial gradients observed in CO.

Vertical resolution of MLS CO data.

The vertical resolution of the MLS CO product is in the range 3.5 – 5 km in the upper troposphere (100-215 hPa), as defined by the full width at half maximum of the averaging kernels (AKs) (Livesey et al., 2011). The AKs are sharp functions, and their relative sharpness allows realistic depiction of atmospheric structure on length scales somewhat finer than the averaging kernel widths, as discussed for e.g., ozone, by Santee et al. (2011) (N. Livesey, personal communication). The average distance between adjacent MLS retrieval levels is ~2.4 km. The AKs are given on the retrieval levels only (Livesey et al., 2011). We first interpolate the model output to the MLS retrieval levels, and then we apply the MLS averaging kernels to the model output. Thus we treat the model output as if it were viewed by MLS. We can only deduce a vertical velocity within the constraints of the MLS data, which is admittedly with low vertical resolution. We now comment further on this in the revised paper, and acknowledge this limitation in the Discussion.

To deduce the vertical velocity from MLS CO, we used the daily MLS CO at their retrieval levels (7 levels from 215 hPa to 30 hPa). We do not interpolate between levels. It takes about 47 to 280 days for CO to be transported from one level to the next level (~2.4 km). In our method, we correlate every 6-month of data with every 6-month of data in the upper level, with the lower level lagging the upper level by 1 day to 14 months. We chose a 6-month moving window, which corresponds to about one/half temporal wavelength of the tape recorder signal below/above 100 hPa, respectively. According to Niwano et al. (2003), using data with half the wave length of the tape recorder is enough to deduce the vertical velocity. We use daily MLS CO data (each point is the average of ~320 observations), so that 6 months of data includes 180 points, enough to calculate the correlation.

D) The data and model are clearly correlated for the most part, but can you present the correlation and bias?

We calculated the biases and correlations for Figure 1 and included a subset of the results given below in the revised paper. Here is a complete table of the results.

	r (MLS-G5_GFED2)	r (MLS-G5_GFED3)	r (MLS-G4)
215 hPa	0.64	0.65	0.71
147 hPa	0.56	0.65	0.72
100 hPa	0.63	0.68	0.72
68 hPa	0.84	0.83	0.84
46 hPa	0.12	-0.01	-0.22

	bias (MLS-G5_GFED2)	bias (MLS-G5_GFED3)	bias (MLS-G4)
215 hPa	10.2	12.7	9.7
147 hPa	7.6	9.4	4.7
100 hPa	21.7	23.0	15.9
68 hPa	7.3	7.9	1.3
46 hPa	-5.4	-5.5	-7.9

E) A few paragraphs on just how the model was sampled should be added to the paper somewhere, including a little more mention of the averaging kernels.

We have added this material to the revised paper.

F) Figure 4. Do you interpolate the data between the pressure levels: 215, 147, 100 and 68 hPa? Where does the structure between pressure levels originate? Does the data from adjacent pressure levels actually "overlap" when one considers that the data's vertical layers are _ 4 km? Does this limit your ability to make conclusions about the model's performance in the UTLS?

No, we did not interpolate between the pressure levels. The structure between the levels seen in Figure 4 (zonal mean plots vs. pressure) arises from the latitudinal binning of the data (every 2°) and from the IDL contouring routine used to display the results on a color scale, a standard way to display results.

Please see the response to (C) above about the vertical resolution of the MLS data. We emphasize that we always show the model output as it would have been seen by MLS by applying the averaging kernels. We do not attempt to infer results with higher vertical resolution than given by MLS.

According to the MLS v3.3 user guide, at 215 hPa and higher pressure levels, the vertical averaging kernels are all peaked at the level being retrieved. There are some overlaps with adjacent levels as the MLS average kernels are not delta function (Figure 3.6.1 in MLS v3.3 user guide).

G) Overall, the manuscript is far too long as there is too much discussion of details that may only be of interest to very few people. For example, is vertical transport in your model with

GEOS-4 and GEOS-5 meteorological fields of interest to the general science community or to a small group of users of the fields?

The GEOS-4 and GEOS-5 fields are used to drive chemical transport models, including the GEOS-Chem model, with a world-wide network of users at 70 institutions (http://acmg.seas.harvard.edu/geos/geos_people.html). The fields are also used in the NASA/Goddard GMI model (e.g., Duncan et al., 2007; Strahan et al., 2007; Considine et al., 2008; Allen et al., 2010) and the Goddard GOCART aerosol model (e.g., Colarco et al., 2010). The GEOS-5 MERRA fields (very similar to the GEOS-5 fields used here) are used in many other models, including groups at NCAR (e.g., Lamarque et al., 2012). Thus we would argue that evaluation of transport in the GEOS fields is of interest to a large group of users, and beyond that, of interest to anyone who wishes to understand differences in results from global CTMs that were included in recent intercomparisons.

Anonymous Referee #2

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General Comments

Liu et al. present analyses on impacts of surface emissions and transport processes on the observed carbon monoxide (CO) variabilities in the upper troposphere and lower stratosphere (UTLS) obtained from Microwave Limb Sounder (MLS) onboard the Aura spacecraft. GEOS-Chem chemistry transport model driven by the GEOS-4 and GEOS- 5 assimilated meteorological fields with GFED2 and GFED3 emission inventories are used to quantify various processes in the model. A specific focus is made on the CO tape recorder in the tropical lower stratosphere with detailed analyses on vertical transport in the model. Overall, the models are capable of reproducing observed CO distributions in the tropical UTLS region with some limitations. Below the authors may find specific comments with some suggestions.

Specific Comments

1. *Overall, CO simulations from the model have reasonable agreement with MLS but with noticeable disagreement. If GEOS-Chem is more suitable for tropospheric chemistry simulations, the UTLS region might not necessarily be well defined in the model even though it covers the stratosphere. In that case, I would recommend including more details and challenges of using GEOS-Chem in the UTLS region with any cautions we should take in interpreting model results.*

   Yes, the reviewer is correct that the standard GEOS-Chem model is more suitable for tropospheric chemistry simulations. However, as noted in Section 2.2, we made two changes to the standard model to improve this situation, to improve the vertical resolution in the stratosphere over that which is generally used in GEOS-Chem and to improve the stratospheric chemistry relevant for CO. In our tagged CO simulation we used the production rates and loss frequencies of CO in the stratosphere archived from the fully coupled stratosphere-troposphere combo GMI model (e.g., Allen et al., 2010). We ran the GEOS-Chem model at the full vertical resolution of the GEOS fields in the stratosphere, instead of the reduced vertical grid in the stratosphere used in standard tropospheric simulations. These changes were made to ensure that the version of GEOS-Chem we were using was appropriate for this study of the UTLS.

2. *The observed CO variability in the UTLS is obtained exclusively from MLS. Any known biases or issues of the MLS CO data need to be included for quantitative comparison with the model.*

   In the upper troposphere, comparisons with various in situ CO observations (NASA DC-8, WB-57 and the MOZAIC dataset) indicate that there was a factor of 2 high bias at 215 hPa in MLS CO v.2. This bias been largely eliminated in MLS v3.3 (Livesey et al., 2011), and we note this in our paper. There are ample data for validation at 215 hPa, but not at higher altitudes. We assume that the MLS CO data are "true values" up to 70 hPa. We found a possible seasonal bias at 46 hPa as discussed in page 17410 from line 10 to 25, so we cut off our analysis below this level.

3. *Multiple simulations of the model based on different emission inventories (GFED2 and GFED3) and assimilated meteorological fields (GEOS-4 and GEOS-5) are used in this study and the difference between the different model setups is insignificant in some cases. Although this provides more information, it is somewhat unclear what the key findings are. The comparison can be done on a select simulation (for example, GOES-5 with GFED3) and the other details can be included in a separate section when it is necessary.*

   The comparison between GEOS-4 and GEOS-5 is necessary since we are evaluating vertical transport in the two sets of meteorological fields. For the comparison between GFED3 and GFED2 runs, the difference is small. In all spatial plots, we only show results from GFED2 as these were used in both GEOS-4 and GEOS-5 runs, and the GFED2 emissions were used in our previous study (Liu et al., 2010). In plots involving temporal evolution, we added the results of the GEOS-5 model with GFED3 to show the simulation results in 2009 and 2010, for which GFED2 emissions are not available. We have clarified our key findings in the last section of the revised paper.

4. *The authors tried to explain the discrepancy between modeled vs. MLS CO with limitations in surface emissions and vertical transport (for example Figs. 2 and 6) in the model. However, no attempt has been made for possible model improvement. For example, if the CO emission over South Africa were underestimated in the model in July, would increasing the model emission solve this problem?*

   In Liu et al. (2010) paper, we explored the effect of increased emissions over southern Africa and South America on model results using GEOS-4. Kopacz et al. (2010) found that emissions of CO were too low by ~55% in South America and by ~85% in southern Africa, based on multi-satellite inversions (AIRS, MOPITT and SCIAMACHY). We conducted a sensitivity study where we multiplied the CO emissions by monthly scaling factors from Kopacz et al. (2010) in each model grid, using the results of the inversion study only over South America and southern Africa. The scaling factors vary seasonally, with maxima in September and October over southern Africa and in October over South America. We found that increasing emissions over southern Africa leads to improved agreement with observed CO in the LT and middle troposphere (MT), but the amplitude of the seasonal variation is too high in the UT in GEOS-4, as shown in Liu et al. (2010).

For this paper we looked at how the changing emissions affect the CO tape recorder pattern. Figure S1A compares the tropical zonal mean CO at 215 hPa between MLS observations and the two simulations with standard and adjusted emissions and Figure 1B compares the CO tape recorder pattern from GEOS-4 with that from the sensitivity run using increased emissions (described above). Since the scaling factors are from an inverse study using GEOS-4, we show the comparison between the standard run and sensitivity run driven by GEOS-4. In the sensitivity run, zonal mean CO at 215 hPa in September/October in 2005 and 2006 is ~15 ppb higher than in the standard run. The model is then biased high by ~5-10 ppb from September to December of 2005, and but is too high only in October of 2006 in terms of absolute values (Figure S1A). The increased emissions however do not significantly change the spatial and temporal propagation of the CO tape recorder pattern, as shown in Figure S1B.

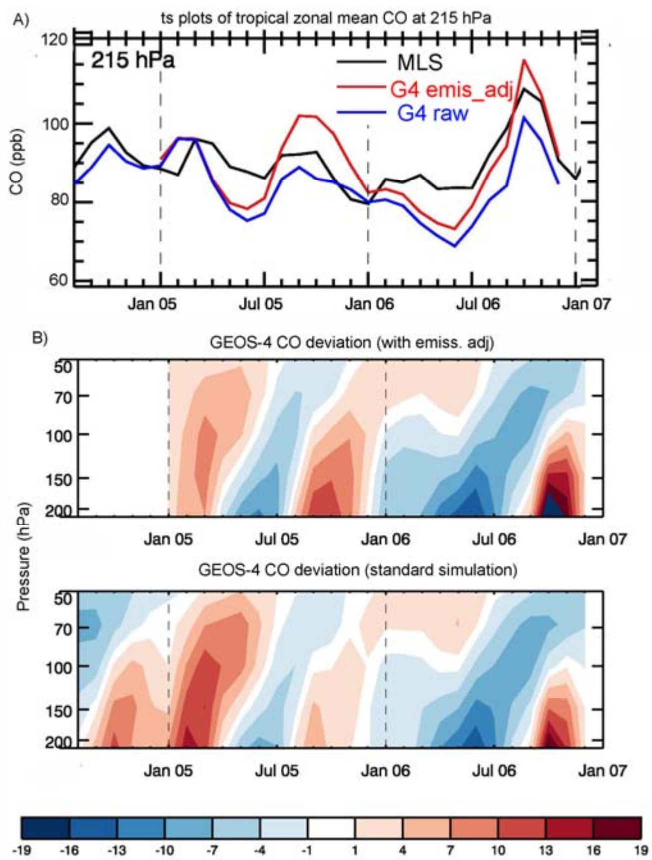

Figure S1: comparison of A) the tropical zonal mean CO at 215 hPa between MLS observations and the two simulations with standard and adjusted emissions, B) the CO tape recorder pattern from GEOS-4 between the sensitivity run using increased emissions (top) and the standard run (bottom)

5. *When the model reproduces observed variability well, tagged CO run might be helpful to understand detailed contributions from various processes. I am not so convinced with usefulness of tagged CO results when there is disagreement between the model simulations and observation.*

   Clearly we have to be careful in using results of tagged simulation when the model and observations show large disagreements. However, we would argue that the tagged results offer insights into possible deficiencies in model transport, especially given the strong seasonality in the biomass burning sources from the two hemispheres, and the interannual variability in these sources. The spatial maps of CO also show that the model CO maxima are correctly located, even if their magnitudes are too low. Given the many aspects of the morphology and seasonality of the observed CO patterns revealed by MLS that are reproduced by the model, we argue that the tagged results are very useful in understanding the contributions from various CO sources.

   Our comparisons indicate that the phase of the seasonal cycles of both model simulations agree reasonably well with the observations at 215-100 hPa (Figure 7, 11), although there is a low bias in the models (Figure 1); the phase of the model results is not altered in the sensitivity run with increased CO emissions from South America and southern Africa. At 68 hPa, the low bias is smaller in both GEOS-4 and GEOS-5 models and that driven by GEOS-4 agrees well with observations during boreal summer.

   The tagged CO run allows us to show which sources contribute to the seasonal and interannual patterns in the model, and from these results we can imply which sources contribute to the patterns in the data. Among these sources, the pattern of CO from biomass burning from the tropical continents is closely related to the upward transport in the model at the end of the dry season. CO from fossil fuels with mainly mid-latitude sources is more determined by horizontal transport at higher levels. CO from methane (which is specified) and isoprene is controlled by the emissions and by photochemical processes in the model. We argue that the difference in model performance between 68 hPa and lower levels results from the different source contributions in the model at these levels. Thus, the tagged CO simulation, combined with what we know about the deficiencies in vertical transport, allow us to hypothesize the reasons for the decrease of the low bias in the model at the higher level. Our results suggest that the contribution from methane and fossil fuel, which are less influenced by the tropical vertical transport in the model, increase a lot from 100 hPa to 68 hPa.

6. *Line 79 - In section 2.1 (satellite data), description of MLS IWC data and the procedure of constructing gridded MLS CO data need to be included.*

   We refer the paper by Jiang et al., 2010 for the description of the IWC product. We filter the CO data as recommended in the MLS Users Guide (Livesey et al., 2011). The gridded CO data are simple arithmetic averages of all acceptable retrievals in each grid box.

7. *Line 134 - CO observed by MLS (black solid line) and simulated by the model driven by GEOS-4 (blue line) and GEOS-5 using GFED2 emissions (red dashed line),*

   We now refer to the data as the black solid line. This information was given in the figure caption.

8. *Fig. 4 – I am not convinced what new information Fig. 4 has to add to this article.*

   Figure 4 show how diffusive the models are. It indicates that the CO distribution is broader and less isolated in the tropics in both models than in the observations, which may imply too slow ascent and stronger in-mixing from higher latitudes in both models. The comparisons support the discussion of more tropical recirculation in GEOS-5 than GEOS-4 (broader peaks in GEOS-5).

9. *Also MLS zonal mean CO seems to be noisy as monthly averages.*

   That is what the data shows.

10. *Fig. 5 – I wonder the weaker Asian monsoon maximum is unique to CO or other species (for example, water vapor) in the model. By comparing different species in the model, one should be able to tell if it's related to emission or vertical transport in the model.*

    Water vapor in the GEOS model is relaxed to a zonal mean climatology in the stratosphere. Examining species other than CO is beyond the scope of this paper. We know that the deficiency in the modeled CO maximum over the Asian monsoon system is not caused by the CO emissions, since we evaluated CO in the lower troposphere as discussed in page 17404, line 25 (line 183-189), and did not find an underestimate.

11. *Line 230 – I do not understand what GEOS-4 anomalies constructed this way mean in Fig. 7.*

    First, these are not anomalies. They are the deviations from the long term mean. This is the standard way to show the tape recorder pattern. For GEOS-5, we calculate the monthly mean from 2005 to 2008 and the corresponding deviations over these years. For GEOS-4, since we only have 2 years of data, we have to adjust accordingly. This is a standard way to align two dataset covering different time periods.

12. *I think isoprene chemistry with CO should be included in the text to be able to understand Fig. 9.*

    The results in Figure 9 are from the tagged run. The treatment of CO from isoprene in the tagged run is described in Duncan et al. (2003) which we refer to.

13. *Fig. 11 is somewhat duplicated with Fig. 1. It would be better to differentiate those figures to address separate questions.*

We use these figures to address different aspects of the CO behavior in the tropical UTLS. Figure 1 is the time series of the CO monthly means, and shows the biases in CO as well as the seasonal behavior. Figure 11 shows CO deviations from the long-term annual mean, and highlights the upward propagation of the phase of seasonal cycle.

14. *Fig. 13 – Black solid lines, supposedly MLS CO, can not be found in this figure.*

The solid lines are the vertical velocity deduced from MLS CO, with different colors representing different seasons. The dashed lines in the left panel are the vertical velocities in GEOS-4, and those in the right panel are the vertical velocities in GEOS-5, with the same color code for the seasons. We have clarified the caption.

Line 347 – I am not sure if it is fair to say that ACE failed to capture the semi-annual cycle at 215 hPa. ACE has at least four samples per year so it might be possible to capture it.

It is hard to get a clear semi-annual cycle from ACE when there are many months each year with no measurements in the inner tropics. We modified the text as "failed to capture the details of the semi-annual cycle at 215 hPa".

15. *Line 379 – I wonder if there is a way to simplify this paragraph or even remove it.*

We cannot remove this paragraph, since the discussion indicates the upper limit of the CO tape recorder pattern.

16. *Line 395 – Instead of 'rising', 'vertically propagating' might be more relevant.*

The text has been modified as suggested.

17. *Extensive analyses on vertical velocities related to CO tape recorder are included in this study, which includes big uncertainties by nature. The authors need to address potential errors and uncertainties in their analyses.*

We now discuss this in more detail, and clarify that the vertical velocities we derive from MLS are averages over a few km, given the relatively low vertical resolution of MLS CO.

18. *The conclusion contains so much information and some contents are duplicated in the text. It would be recommended to move discussions about vertical velocity in to different section and reduce the content of it significantly for more clarity.*

We modified the structure of this section to make it clearer, and we have omitted some of the material in the submitted version.

Anonymous Referee #3

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The paper by Liu et al. compares multiannual time series of MLS satellite observations of CO with model simulations of GEOS-Chem driven by different meteorological input fields (GEOS-4 and GEOS-5). Biomass burning data are GFED V2 for GEOS-4 and GEOS-5. In addition GEOS-5 data are used with GFED V3 and tagged CO tracers to conclude on possible source regions. Potential causes for differences between the models and observations are discussed. The central point of the discussion is the discussion of the CO tape recorder and vertical transport in the TTL-region and above. The authors deduce upward velocities from the CO phase in the TTL and the tropical lower stratosphere. In general the paper is well written, but a bit lengthy. The Figures are clear and the paper contains a lot of information, but I would suggest to more focus on the main aspect. It is not entirely clear to me what the final purpose of this paper is: To investigate the distribution of sources and sinks in models and satellite observations? Or to evaluate model uncertainties due to different emissions and transport schemes? Or to investigate vertical transport across the TTL region? I regard the latter as most important since the time for this pathway determines in the end, how much of a tracer with finite chemical lifetime enters the stratosphere.

Therefore I highly recommend to restructure it before publication and focus more on the central point: the discussion of the upward velocities in the tropics.

There are two objectives in our paper: (1) To determine what controls the CO tape recorder in terms of both CO sources and vertical transport, and (2) To determine any constraints on vertical transport from the MLS CO data in the tropical UTLS. The first part of the paper focuses on the first objective and examines the interaction of the two controlling factors. We also discuss the influence of photochemical process on the CO tape recorder pattern, as the CO lifetime limits the height to which the pattern is controlled mainly by transport. In the second part of the paper, we used the long-term CO measurements to deduce a new independent constraint on vertical transport in the UT. We modified the paper to highlight and clarify these two objectives.

P.17401, l.17: Please provide some key information relevant for transport or at least the names of the convection schemes. Were the CO emissions released at ground or at a given emission height?

We now refer to the convection schemes used in GEOS-4 and GEOS-5.
The CO emissions were released in the model boundary layer.

p.17412, l.1: How do the transport times relate to the trajectory studies of e.g. Fueglistaler et al., JGR, 2004, 2005, or Ploeger et al., JGR, or in-situ measurements (e.g Marcy et al., Atmos Env. 2007)

Thanks for pointing out these references. We already discussed the paper by Ploeger et al. 2010. In Fueglistaler et al. (2004), the mean heating rate is about 1 K/day near the tropopause, which is about 3 mm/s. In Ploeger et al. (2010), the model studies suggested an annual mean tropical vertical velocity of around 1 K/day between 360 K and 440 K (~3 mm/s at 360K and 380K, ~14-

16 km or ~140 hPa to 100 hPa). Our estimates show that vertical velocity derived from MLS CO is around 1 mm/s at 125 hPa, ~3mm/s at 215 hPa.

Marcy et al. (2007) make a rough estimate of the residence time in the TTL from their CO profile observations, assuming a constant value for OH from a model, and that loss by OH is the only factor affecting the CO vertical gradient. They show that this value (60 days) is in the middle of the range of values in the literature for the residence time. A value of 60 days is within the range of transport times in GEOS-4 and GEOS-5 from 215 hPa to 100 hPa. We do not refer to their work as there are later measurements and analyses of tracers (e.g., CO_2) that yield vertical velocities (and are not dependent on assumed OH), such as the work of Park et al. (2010) that we cite. Marcy et al. cite the earlier work on CO_2 by Park et al. (2007).

We include comparisons of our results for vertical velocities with those derived from the water vapor tape recorder, from heating rate calculations, and from analysis of the CO_2 data in our Figure 13, and we discussed these comparisons in the text.

As we mentioned in the paper, we compared the GEOS-4 and GEOS-5 vertical velocities with the vertical velocities deduced from radiative heating rates calculated from observed water vapor and ozone using a radiative transfer model (Yang et al., 2008) and they show similar seasonal variations.

p.17415, l.18 and Fig. 10d) How is it possible to have zonal mean negative mean upward velocities in the tropics during boreal summer? Even if there is some local downwelling, doesn't this point to a general transport problem in the model?

We discussed this issue in our Discussion, and referred to the paper by Ploeger et al. (2010) which shows the same problem in ECMWF fields (interim ERA). In their paper, they examined the influence of the vertical velocity scheme on modeling transport in the tropical tropopause layer and suggested that almost no subsidence occurred in the UTLS in a diabatic scenario with the cross-isentropic velocities deduced from different diabatic heating rates; however using a kinematic scenario, which is commonly used in off-line CTM models, could frequently generate subsidence in the tropical UTLS. And if regions with subsidence are large enough (such as in GEOS-5) it could generate a mean subsidence velocity in the tropics during boreal summer. We compared spatial maps of the tropical vertical velocity fields in GEOS-4 and GEOS-5 with those in the ERA-Interim kinematic vertical wind fields (Figure 6 in Ploeger et al., 2010) and they are very similar, showing similar spatial patterns of downward and upward transport. We already discussed this in the submitted version of our paper. It is not clear whether this subsidence in the tropics is real or an artifact of the kinematic scenario. However it shows the modeling community that we need pay attention to the possible bias generated by different vertical velocity schemes in off-line chemical transport models using archived wind fields.

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