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Interactive Comment

Interactive comment on "Transport pathways of CO in the African upper troposphere during the monsoon season: a study based upon the assimilation of spaceborne observations" *by* B. Barret et al.

B. Barret et al.

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Answer to the specific comments by anonymous referee #1

P 2869 (line -14 to -20); I doubt if this is necessary

We have removed the detailed description of the screening of the MLS data as it can be found in the (Livesey et al., 2007) reference.

P 2870; Folkins et al. (2006) s climatology consists of all seasons from Feb. 04 - Nov. 05 and the ACE covers the tropics in spring and fall during this time. The authors may need to appreciate the difference between the Folkins et al. (2006) s climatology and the data used here and also the possible effect on the results. Or using the climatology of the ACE-FTS CO from the summer months (including 2006) would be recomended.

We have computed ACE-FTS CO annual and JJA climatological profiles for the tropics using all observations for the 2004 to 2007 period. The differences between our annual profile and the Folkins et al. (2006)'s climatology are within 5%. The JJA climatology is lower by more than 11% compared to Folkins et al. (2006)'s. Using the JJA climatology would therefore lead to an higher underestimation of CO compared to the in-situ MOZAIC observations which represent the most accurate validation dataset at the bottom of the vertical domain around 200 hPa (see section 3). We have therefore chosen to use Folkins et al. (2006)'s results. Even though it is an annual climatology, it allows to achieve a better representation of the vertical gradient of CO correcting the MLS altitude dependent biases.

P 2870 (line 12); It has not been explained how the scaling factor was chosen by a comparison with the MOZAIC either in section 2.1 or section 3.

The method used to adjust the scaling factor at 215 hPa by comparisons with the MOZAIC data has been better explained in section 2.1.

P 2872 (line 4); MLS has a daily global coverage I think. Considering the regional extent of this study, I do not agree that we need a week to months of average of the data. And for ACE-FTS, a month worth of data does not necessarily guarantee the coverage at the tropics.

It is true that MLS provides a "daily global coverage". Nevertheless, the along track resolution is 500 km and there are about 1400 km between two successive tracks at the equator. It is therefore necessary to average at least several days to get a real global coverage at 2°x2° resolution equivalent to the one of the model. We have changed "week to months" to "several days to weeks". We removed the mention to ACE-FTS as the coverage is too poor for assimilation.

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P 2875 (Fig. 1); The comparison between the model run and the MLS observations seems reasonable at 147 and 215 hPa (both morphology and the absolute concentrations) even though the authors mentioned the possible overestimation of the BB in the model over Africa. The difference between the model and MLS seems more problematic at 100 hPa. Rather than the CO emissions in the model, I would think the disagreement might be related to the model dynamics (weak monsoon circulation) or convection.

We agree with the reviewer, but an underestimation of the emissions in agreement with other studies cannot be ruled out. The dynamics and/or the convective parametrization are probably the cause for the most important disagreement at 100 hPa. The text has been updated to make this point clearer.

P 2875 (Fig. 2); I wonder if Fig. 2 is necessary (and so does the second paragraph on P 2875). It seems unavoidable to have a larger error over small domain. It might be simple enough to keep the last 1 or 2 sentences.

Fig. 2 and the second paragraph on P2875 are important regarding the performance of the assimilation system. The point is not that the domain is smaller, but rather that the fast vertical transport characteristic of the Asian domain is the feature which is the most difficult for the model to capture (cf. previous comment). Nevertheless, Fig. 2 shows that even over this domain, the assimilation system reaches convergence after a few days.

P 2876; The meaning of the 1st sentence is unclear.

We have skiped the second part of the sentence which is not necessary.

P 2883 (section 5); The idea of section 5 may need to be reconsidered in a simpler way. Let s suppose that the CO concentration over Africa is affected by the strength of the Asian monsoon anticyclone and the tropical easterly jet. I think the authors tried to quantify this relationship. First of all, the latitude range in Fig. 8 is at the center

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of the Asian monsoon anticyclone. And it is expected that CO has a high correlation with the low PV inside the anticyclone. But this does not tell much about the effect of the Asian monsoon anticyclone to the African upper tropospheric CO concentration. The correlation between CO and the zonal wind at 150 hPa is not clear from Figure 9. I would think that it is more important to show the correlation between synoptic variability of CO over Africa and strength of the Asian monsoon anticyclone and the tropical easterly jet. It can be done by a scatter plot or a time series of CO over Africa vs. ASM or TEJ variability.

The Hovmoller diagrams (Fig. 8 and 9) represent a more compact and illustrative way of showing correlation between the AMA/TEJ with CO than scatterplots. They give information about time series for a broad longitudinal domain encompassing Africa. The correlation coefficients indicating the level of correlation for each level are given in the text. Even though the correlation between CO and zonal wind is not clear for all the domain and the whole period, Fig. 9 clearly shows that high CO concentrations are bounded by strong easterly winds and that high CO is only observed west of 0° following incursions of the TEJ.

This paper can be shortened by eliminating some details especially in section 2 and section 5 and the cited references.

This paper has two objectives. First the description of the methodology based on the first assimilation of UTLS CO spaceborne data. We have already made Section 2 as short as possible for a complete enough description of the assimilation setup and performances. Nevertheless, following referee #1 comment, we have shortened this section and eliminated some references. Secondly, we study the transport pathways of CO in the african UT. Section 5 dedicated to the evaluation of the possible causes of the CO synoptic variability is only 2 pages long and can hardly be shortened.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 2863, 2008.