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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #1

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General comments:

Using the global satellite measurements from MLS and the data assimilation through the chemistry transport model (MOCAGE), the authors investigated transport pathways of CO over West Africa in the upper troposphere for July 2006. Due to sparse data coverage in the tropical upper troposphere, data assimilation can be a useful tool to study transport of CO especially over Africa and Southeast Asia. And the results seem reasonable that the elevated CO over western African monsoon is related to the local emission, large-scale circulation and the Asian monsoon circulation. I have a few comments for the authors might take into consideration.

Specific comments:

P 2869 (line -14 to -10); I doubt if this is necessary.

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 recommended.

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.

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.

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.

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.

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

ACPD

8, S484–S486, 2008

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 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.

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

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

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8, S484–S486, 2008

Interactive Comment