

## ***Interactive comment on “Geographic and seasonal distributions of CO transport pathways and their roles in determining CO centers in the upper troposphere” by L. Huang et al.***

**Anonymous Referee #2**

Received and published: 25 January 2012

This is an interesting study trying to unravel the impact of deep convective transport of biomass burning CO from Africa and South America on the seasonality of CO in the tropical upper troposphere. However, I have several major concerns about the basics of the adopted approach (see below). To adequately address these concerns, it might require significant modification and/or improvement in methodology. Therefore, I feel this manuscript is not ready for publication in Atmospheric Chemistry and Physics at this stage.

Major comments:

1. The authors proposed to categorize the upward transport of CO from the surface

C14715

to the tropical UT into three different categories: local convection, LT advection, and UT advection. If the ultimate goal of this study is to understand how seasonality in surface emissions and dynamic transport contribute to the seasonality of CO in the tropical UT, isn't the three categories too much a simplification in a region where south-north movement of ITCZ, east-west transport associated with ENSO and long-range transport from Indonesia and South Asia, etc, all mingled together? At minimum, the authors need to provide a comprehensive review of what are the dominant processes that could contribute to the observed seasonality and how to interpret their derived results in this larger context.

2. By averaging emission data and satellite CO data at 8-day intervals and  $4^{\circ} \times 8^{\circ}$  (approximately 450 km x 900 km) and using the co-occurrence of surface emission and elevated UT CO as identification of local convection, the authors are in fact assuming air remain relatively stagnant in the region. This in fact might not well be true. Assuming mean winds about 5 m/s in the LT and 20 m/s in the UT (which are reasonable numbers), an air mass can travel  $\sim 3500$  km and  $\sim 14000$  km in 8 days and be placed in a distant downwind region. Therefore, while surface emissions indicate surface fire activity and satellite data shows elevated CO above in the UT, it is some times possible that the elevated UT CO is not associated with local convective lofting, but advection from remote resources.

3. Although the authors did not say explicitly, the discussion in the second paragraph on page 32426 and section 5 seems to imply that emission and transport are the only processes that contribute to seasonality of CO. This is misleadingly incomplete. A significant part of CO seasonal variations is also due to i) seasonal changes in its lifetime due to changes in solar radiation (therefore OH) (Duncan et al., 2007), ii) seasonal changes in CO production from biogenic sources and CH<sub>4</sub> oxidation (Duncan et al., 2007; Liu et al., 2010). The impact of the above, relative to seasonal variations due to convection transport and surface emissions, need to be addressed.

4. The authors state that “To our knowledge, the influences of seasonality in the distri-

C14716

bution of transport pathways on the seasonality of CO concentration in the tropical UT have not yet been clearly identified or addressed.” The two above referenced papers, Duncan et al. (2007) and Liu et al. (2010), though did not address directly the impact of the distribution of transport pathways, both presented a comprehensive modeling analysis together with satellite measurements to look at the impact of upward transport of biomass burning emissions as well as many other sources on the seasonality of CO in the UT and LS region. These two studies are highly relevant to this study and need to be acknowledged. In addition, I strongly encourage the authors to compare the results from this work with those from Duncan et al. (2007) and Liu et al. (2010) and discuss how they agree or disagree.

For example, the authors conclude that “This result suggests that the seasonal prevalence of the local convection pathway plays a key role in the seasonal variation of UT CO over Central Africa” and “the seasonal variation of UT CO over South America was consistent with that of the occurrence frequency of the local convection pathway” (Page 32436). In comparison, Liu et al. (2010) suggested that the spring and fall peaks in CO at 215 hPa over tropical Africa are due to the combined impact of local biomass burning emissions, long-range transport from Indonesia and South America, as well as production from biogenic emissions. Similarly, the austral spring peak in South America is due to the combined impact of local biomass burning emissions, long-range transport from S. Africa and production from biogenic emissions (Liu et al., 2007).

Minor comment:

Section 3.1 – the Boreal winter case: By looking at the wind arrows, it seems the northward transport at  $\sim 11$  km is  $\sim 5$  times faster than the near-surface southward transport. This implies if the transport pathway is LT advection  $\rightarrow$  convection  $\rightarrow$  UT northward advection as the authors argued, the CO hot spot in the UT should be displaced to the north of the surface emission center, not where convection happens. What is the possible explanation in the UT displacement?

C14717

In addition, I suggest the authors include a figure showing back trajectory results for this case. This can be a more robust piece of evidence in demonstrating the transport pathway.

Duncan, B. N., Logan, J. A., Bey, I., Megretskaia, I. A., Yantosca, R. M., Novelli, P. C., Jones, N. B., and Rinsland, C. P.: Global budget of CO, 1988–1997: Source estimates and validation with a global model, *J. Geophys. Res.-Atmos.*, 112, D22301, doi:10.1029/2007jd008459, 2007.

Liu, J., Logan, J. A., Jones, D. B. A., Livesey, N. J., Megretskaia, I., Carouge, C., and Nedelec, P.: Analysis of CO in the tropical troposphere using Aura satellite data and the GEOSChem model: insights into transport characteristics of the GEOS meteorological products, *Atmos. Chem. Phys.*, 10, 12207–12232, doi:10.5194/acp-10-12207-2010, 2010.

---

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 32423, 2011.

C14718