Review of "**Seasonal variation of trans-Pacific transport of carbon monoxide (CO) in the upper troposphere: MLS observations and GEOS-Chem and GEM-AQ simulations**" by Jin et al.

General comment:

The aim of the paper is to characterize the transport of "pollution" from Asia to America across the Pacific. The authors use Aura/MLS observations to document the CO seasonal distribution in the UT and to validate CTMs simulations. The use of tagged CO with one of the two models enable to quantify the contribution of different sources to the UT CO budget in different region from Asia to the US coast. Finally, sensitivity simulations performed with deep convection switched off are made to determine the impact of deep convection on CO transport to the UT and across the Pacific. The subject of the paper is perfectly suited to ACP and the methodology used is generally correct to address the objectives. Nevertheless, I have some important concerns about important points concerning the methodology and the discussion of the results that are addressed below. Furthermore, I am not satisfied with the quality of the English. Many –if not most of the- sentences are not clear, the syntax is approximate and there are too many typos and errors. Because of its poor English, this paper should not have been published in ACPD as it is. I think that a paper written by authors from US and Canadian institutions should reach much higher language standards.
I therefore recommend publication of the manuscript in ACP after the specific comments detailed below are addressed and after the quality of English is largely improved.

Specific comments:

- the word "pollution" is used throughout the paper when CO distribution and transport are concerned. "CO is a tropospheric trace gas" emitted by pollution sources and an O3 precursor but CO itself is harmless for health and crops and is not affecting directly air quality. It is stated in the introduction that CO is a tracer of pollution which is correct. Therefore, more care should be taken concerning the intensive use of the word "pollution" when dealing with CO.

- P3225L14-P3226L6, section 3.1: the description and first interpretation of the latitude-time cross-sections of MLS CO is a bit confusing. The issue of biomass burning over Southeast Asia is mentioned once for "spring" and once for fall at the end of the paragraph. Furthermore, "fast upward transport" or "deep convection" are mentioned to lift the products of fires to the UT, without further information, as if it was always the case all over Asia. I think that things could be easier to understand if some details were given regarding (i) the seasonal variations of SE Asia BB (ii) monsoons and convective seasons in the different part of Asia. The latest statement about NH CO is not providing information concerning the subject of the paper and it would be better to briefly describe the CO variations in the lower troposphere over Asia in relation with the CO sources.

- P3225L15-17: The authors correlate "January-March" high tropical CO in the UT to "spring" BB emissions in Southeast Asia. As mentioned below in the text, boreal spring extends from March to May rather than from January to March!

- P3225L23: the Subtropical Westerly Jet (SWJ) should be introduced here for the first time to explain the "eastward transport".

- P3226L9-12: I do not understand the sentence! In winter, there are low CO concentrations over both west and east Pacific and high concentrations in spring. How do the authors conclude “strengthened” trans-Pacific transport from that? To be rigorous, the strength of horizontal transport should be deduced from CO fluxes (combination of vmrs and winds) across a vertical surface rather than from raw concentrations. For instance if the winds were twice larger in winter and concentrations twice lower, the fluxes and therefore “transport” would be equal. Therefore, either the authors mention “CO vmrs” when they deal with “vmrs” or they mention that they link vmrs and transport considering that winds are not substantially varying in time or location.

- P3227L4-5: the statement “back trajectories… summer” is not clear. Do the authors mean that summer is the only season for which Asian pollution travel to North America in the UT?

- P3228L4-8: the large GEOS-Chem bias over SE Asia is attributed to “large CO” produced by GEOS-Chem “in FM2005”. This is not really an explanation. Is there a bug in the model? Is there an overestimation of fires in the GFEDV2 BB emission inventory for SE Asia in winter 2005? The latest seems unlikely because GEM-AQ uses the same inventory but is not overestimating UT CO over SE Asia.

- P3228L10-11: the differences between modelled CO UT distributions are very quickly attributed to differences in the convection schemes. It is mentioned that the “vertical transport” in GEM-AQ “needs to be improved”. This is a very imprecise statement. The vertical transport in any model should be improved! But what in particular goes wrong in the model? Are there some bibliographic references that show a problem for GEM-AQ concerning deep tropical convection? In order to prove that the convective schemes are responsible for the differences between the simulated CO distributions, the author should analyse diagnostics from the schemes such as convective precipitations, mass fluxes. The differences could also come from the chemical scheme, the parameterization of turbulent mixing…

- P3228L17-18: the sentence is symptomatic of two recurrent problems in the paper. It is not correctly written and there is confusion between “transport pattern” which doesn’t mean anything precise and which is certainly not measured by MLS and “CO distribution resulting from the transport of pollution”.

- P3228L28-P3229L1: the mention of “strong episodic vertical transport of BB emission in early 2005” is very highlighting concerning a potential cause of the high bias of GEOS-Chem over south and SE Asia. Is it the same problem as the “large CO” produced by GEOS-Chem mentioned for winter (see above)? Why is this statement made for spring when early 2005 is mentioned? What is early? If the same problem concerns winter and spring, this should be clear. Why does this problem happens only in 2005 and not in 2006? Is it related to the interranual variability or to the model itself ? I think this point is interesting, especially for the community of GEOS-Chem users and that it has to be addressed in more details.

- P3229L3-6: here again, we have a description of the model differences and of the discrepancies model vs. observations but no discussion about their potential origin. Why GEM-AQ has a better behaviour than GEOS-Chem to represent the spring to summer evolution?

- P3229L6-L17: this part is very unclear to me. I do not understand the link between this paragraph dealing with westward transport of east Asian pollution and Figure 2 cantered over the Pacific.

- P3229L14: Is a plume 2000 km wide (30°-50°) narrow?

- P3228L15-17: what is the usefulness of this statement without comments?

- P3229L29-P3230L3: same comment as for P3228L10-11. We would like to have a real diagnostic concerning GEM-AQ and GEOS-Chem to explain their diffrences. We need more than "suggest that".

- P3229L4-P3229L13: here again the statements are rather general and we need some more precise information concerning the 2 models used in this study. For instance, as in the studies referred to (and as I mentioned above), the authors should compare the vertical mass fluxes from the 2 models.

- P3231L4: the authors probably mean fall-winter-spring. It should also be mentioned that the model is characterized by high biases relative to the observations in fall and winter over SE Asia (section 3.1 and Fig. 2). Therefore, the SE Asian contributions to the CO budget are probably upper bounds.

- P3231L26-28: the sentence "even though…Sect 3.1" is unclear.

- P3233-P3234: I have a concern about the interpretation of the "convection off" simulation" (see Lawrence and Salzmann, ACP, 2008). In the tropics part of the convective mass flux is accounted for by the large scale winds used to drive the advection schemes of the models. It implies that, even when parameterised convection is switched off, a large part of convective transport is still occurring in the simulations. The authors mention this problem P3234L27-29 when they argue that the "strong upward large scale circulation within the ITCZ" explain the high UT CO vmrs in JJA over Asia and NW Pacific. They should mention this methodological problem at the beginning of Section 5, take this fact into account when dealing with the interpretation of their sensitivity simulation (as they do P3234L27-29) and modify the text accordingly in the abstract and the conclusions.

- P3234L6-8: the "upward transport of large CO emissions" during SON over the Indian Ocean is probably due to the large scale upward winds and an illustration of the previous comment.

- P3235-3236: the summary should be updated in order to take all the changes into account.

- P3235L23-24: the study should come to more specific conclusions concerning the convective parameterization of GEM-AQ.

- P3235L26-27: "Detailed… simulations". As I mentioned previously, I think some more precise elements should be brought by this study to highlight the causes of the high UT CO in the GEOS-Chem simulations.

Technical corrections (far from exhaustive):

P3220L23 : tropospheric, are
L25: are
L26: variations
P3221L11: there is a strong
L18: exceedance ?
L23: although Asian CO concentrations are larger
L26: define UT earlier and use UT consistently in the text
L28: “compared to in” does not sound correct
P3222L15: and of the
L16: and in
P3223L25: include
P3224L10: not yet been
L16: Multiscale
P3225L9: seasonal variations of transpacific CO distributions as measured by MLS. MLS is not measuring “transport”.
P3225L20-25: a first mention of the subtropical westerly jet should be made here to explain the “eastward transport”
P3226L24: “MLS most likely senses the …and senses the upper…”
P3231L5: larger
P3231L9: biomass burning
P3231L16 Asian fossil fuel use
P3231L25: Asian fossil fuel burning
P3235L9: GEOS-Chem