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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of CO in the tropical troposphere using Aura satellite data and the GEOS-Chem model: insights into transport characteristics of the GEOS meteorological products" *by* J. Liu et al.

Anonymous Referee #1

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This paper compares the GEOS-4 and GEOS-5 model versions to CO satellite observations from TES and MLS. Although the subject is interesting, the paper is way too lengthy and tries to deal with too many subjects. The result is that the paper reads difficult, that the figures become unreadable, and that an average reader looses interest in the paper after, say 10 pages. A total of 25 figures with many sub-panels is beyond a normal length paper. The paper would benefit from a severe reduction and the authors should make a choice what to discuss. Now I count the subjects:

- GEOS-4 vs. GEOS-5
- The year 2005 vs. 2006
- Attribution of the different sources to the vertical profile
- The effect of the averaging kernel
- Analysis of the different regions
- The complex interplay between convection and emissions
- ....

Of course scientific writing is also a matter of taste. Nevertheless, I have the following recommendations for the paper.

- Start by removing the global picture in sections 4.1 and 4.2. Figures 3 and 4 are too small and most of the details are repeated later in the paper
- Skip the source attribution. Figures 8, 17, and 21 are again unreadable, and you should limit the discussion. When important you can mention (some) details in the text
- Avoid the discussion about the averaging kernels. Sampling without taking into account the averaging kernel is simply wrong and your aim should be to produce readable figures with less lines

Apart from these recommendations, I have some questions about the results that should be clarified. First of all, the authors should mention the CO sink that potentially plays an important role. The OH levels may be off in a full chemistry simulation and the best way to verify the levels is to quote the methane and methyl chloroform lifetimes.

Second, the simulations are done in a rather coarse resolution. The process studied is quite delicate, since the interplay between convection and emissions around the ITCZ is known to be difficult to describe numerically. An assessment of resolution effects is therefore required. Third, the conclusion that GFED-2 biomass burning emissions are too low may be related to the fact that the vertical mixing in both GEOS-4 and GEOS-5 may be too slow or shallow to bring the CO upward. Interference with the resolution and the OH field may also play a role. In other words, the conclusion may depend strongly on the model used in the study. When I have a close look at figure 12 (and 16) I observe that in GEOS-5 the surface vertical mass-flux goes to zero. Moreover, the mass fluxes for GEOS-4 and GEOS-5 differ substantially and the true convective mass flux profile may again be substantially different. Maybe the authors should test more vigourous vertical mixing (or less vigourous mixing in Africa) like they test enhanced biomass burning emissions. I think to authors claim too strongly that the flaw of their simulation can be attributed to wrong biomass burning emissions. Model errors and also errors in (absolute) satellite retrievals (remember that MLS has to be scaled down considerably!) interfere with emission errors. In that respect I could not agree more with the authors on the last page (19659), where they clearly indicate that caution is needed in inverse modelling approaches in which traditionally model-measurement differences are attributed to emissions only. In that respect, I think there is not much wrong with this paper scientifically. But the authors should narrow down their message to a few key points.

1 Other Issues

page 19634, line 23 I remember a MINOS paper by Lelieveld et al. that addresses this issue.

page 19635, line 4 I miss a clear definition of UT and LT. What ranges are exactly

meant here? Please define.

page 19636, line 1 I would rather use "thorough evaluation" instead of "useful tests".

page 19637, line 16 CO emissions ..... add "from burning".

page 19637, line 18 I think such a detailed description of the emission model is not necessary here..The paper becomes too lengthy

page 19638, line 16 NO_x concentration .... add "fields".

page 19640, line 1 Again: too detailed.

page 19641, line 9 I think that these sections can be removed. The pictures are too many and too small and many of the issues are reiterated later.

page 19644, South America: I would leave out the averaging kernel results from the plots.

page 19645, line 12 ..moisture and latent heat flux....I do not see what is the difference between moisture and latent heat flux. I think the authors (Fu?) suggest a build-up of moisture, due to a latent heat flux?

page 19646, line 18: Please also define MT (see above).

page 19649, line 23: ..The low surface emissions in the model....This assumption might be related to the model at hand. I might also be that convection simply does not bring enough CO upward.

page 19652, line 21: ..deficiencies in the GFED2 emissions. I am not convinced here. I really wonder what will happen at the surface (Figure 2) with these higher emissions. Also, a strong interplay with OH can play a role (see main comment).

page 19654, line 21: TEJ?

page 19659, line 28: ...to estimate source uncertainties. I think the authors mean ....to estimate sources (and their uncertainties).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19631, 2010.