

Interactive comment on “Remote sensed and in situ constraints on processes affecting tropical tropospheric ozone” by B. Sauvage et al.

B. Sauvage et al.

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We would like to thank referee#2 for useful comments on our ACPD manuscript “Remote sensed and in situ constraints on processes affecting tropical tropospheric ozone”. This clearly strengthens our manuscript. Below is the final response to the different comments.

Anonymous Referee #2 Received and published: 10 January 2007

General comments This paper comprehensively investigates the effect of using emissions improved with satellite observed information in the chemistry-transport model GEOS-CHEM. More limited evaluations of using satellite derived emissions in chemistry models have been made before, but may have left readers with the question as to what the effect on non-evaluated model simulated tracers, or on the overall model

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Discussion Paper

performance, would be. By evaluating the emission modifications with independent observations of multiple species (ozone, NO_x, CO, HCHO) and from several instruments the added value of emission information from satellite observations is much more rigorously demonstrated than in any previous study. Satellite information obviously allows for significant improvement of NO_x and HCHO emissions used in tropospheric ozone chemistry models. Apart from this, the presented sensitivity studies provide a wealth of information on model errors and sensitivities of interest to modelers.

The interpretation of the effect of using GEOS-3 instead of GEOS-4 input data on tracers in section 4.4 seems very difficult and might be erroneous. First of all, it is difficult to interpret an experiment where one varies several parameters (convective parameterization, cloud optical depth, cloud top height etc.) in terms of a single one of these parameters. Secondly, in line 15-24 and figure 10 it is found that GEOS-4 better simulates upper tropospheric ozone and this is ascribed to differences in convective detrainment. This should also have consequences for CO. However, figure 9 shows that overall CO seems to be better simulated by GEOS-3. Thirdly, there are also large differences in simulated RH in GEOS-3 and GEOS-4 (see figure 11 and also 9). Different water contents can cause major differences in model simulated ozone, but this effect does not seem to have been quantified. This section requires rewriting.

Reply: As pointed by referee#2, it is difficult to quantify the effect of convection from clouds (uplift, radiative effect, cloud top height), on ozone, relative humidity and carbon monoxide distribution comparisons. As suggested, we have rewritten this entire section in the revised version of the manuscript. The role of RH is now discussed. The goal of this section is not to quantify one process influence regard to other process, but more to discuss possible explanations on the different distributions. For that reason we discuss on the possible effect of different processes, based on the characteristics of the different meteorological assimilated fields and of scientific references. We also removed the CO comparison in Figure#11 because of lack of data for conclusive comparisons.

Specific comments

In line 21 of page 11475 biomass burning emissions are expressed in the unusual unit of Tg N/season. It should at least once be explained what the season is in this unit (3 months?)

Reply: We agree that this is not usual way to express that unit. We change the unit in the revision of the paper to Tg N/yr.

Line 5-7 of page 11476 is not clear. How can a 10-year average allow for interannual variations? I assume that the rescaling factor is computed from a 10-year average, and that rescaling retains the interannual variations, while modifying the seasonal variation?

Reply: We clarify this in the revised version. The rescaling factor is indeed computed from a 10-year average and applied to a 10-year average from the model. The model interannual variability is unaffected. Indeed to make rescaling factor calculations we first archive model monthly flashes over the 10 year period.

In the first paragraphs of page 11478 (about TOC) it should be explained what it is - to what extent does it represent the vertical integral of ozone between the surface and the tropopause.

Reply: The vertical column is from surface to the tropopause. We clarify this point in the revised manuscript.

Also the procedure of applying averaging kernels with model simulated ozone in figure 4 should be described. Many readers will not be acquainted with this.

Reply: Helpful suggestion. We describe the procedure in the section 2 of the manuscript, and also refer to Liu et al. JGR 2006 paper for more details.

On page 11471, line 4 it is mentioned for NO₂ retrievals only GOME observations from pixels with less than 50 % cloud fraction are used, whereas for the evaluation of TOC from GOME retrievals with cloud fractions up to 0.7 are used (page 11478, line 5) .The reason why this seems inconsistent is unclear.

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Reply: Reviewer is correct. This was a mistake in the text. NO₂, HCHO and O₃ retrievals use the same condition on clouds, with excluding GOME scenes with more than 50% of cloud fractions. We will correct than in the revised version.

Line 9 of page 11479 states that “The Middle East is under the influence of an anticyclonic circulation”. This is a climatological feature, not a persistent or daily recurring feature, so please add an adjective such as “frequently” between “is” and “under”.

Reply: We will add “frequently” adjective as suggested by reviewer.

Line 20-21 of page 11482 should be expanded to include a comparison to several other estimates of the total annual production of NO_x by lightning published in the last 5-10 years.

Reply: We will expand the sentence to include a review of the total lightning NO_x production estimated by other studies in the last years.

In section 4.2.1 (page 11482) it is shown that correlations of model output with satellite observations from 2000 are much improved if emissions are modified according to these same satellite observations. It would have been interesting if the possibility had been investigated of improving model simulations for another year. This may not be sensible for all emissions, especially not for emissions strongly affected by meteorological variability.

Reply: We agree that applying these emissions for years with meteorological variability may lead to different results and less sensitivity of constrained emissions. That's also one reason why we decided to focus on year 2000, to avoid anomalous years like ENSO. We thank reviewer for her/his suggestion, it would be a good idea to examine whether or not top down inventory improves another year. However that would imply substantial work as presented in this paper, with all the simulations and comparison to satellite and in situ over all tropical sites. We now suggest this as a topic for a future work.

Interactive
Comment

For the sensitivity studies, in particular for CO (figure 9) it would have been more illustrative to see also the difference between standard and original profiles, not only GEOS-3 and GEOS-4.

Reply: We did not include a comparison between modified and original simulations of CO, as the present study does not include CO emission modifications. Moreover, modifications of surface NO_x and VOC emissions and of lightning NO_x emissions have very weak influence on the CO distribution. For that reason it would not be helpful include such a comparison. We will clarify that point in the revision of the manuscript.

Typographical error Replace “developments” by “developments” in line 18 of page 11473.

Reply: Error corrected. Thanks

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11465, 2006.

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