

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

Anonymous Referee #2

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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 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 mod-

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els. Apart from this, the presented sensitivity studies provide a wealth of information on model errors and sensitivities of interest to modellers.

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 parameterisation, 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.

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?)

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?

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. 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.

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

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from GOME retrievals with cloud fractions up to 0.7 are used (page 11478, line 5) . The reason why this seems inconsistent is unclear.

Line 9 of page 11479 states that “The Middle East is under the influence of an anti-cyclonic 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”.

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.

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.

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.

Typographical error

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

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

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