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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemistry-transport modeling of the satellite observed distribution of tropical tropospheric ozone" *by* W. Peters et al.

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General Comments:

The study presents a first comparison of a 14-year dataset of tropical tropospheric ozone columns (TTOC) derived from satellite measurements with a corresponding multi-year simulation of a chemistry-transport model (CTM). The uniqueness of the comparison and the long time span of the data make this study an important contribution to the evaluation of CTMs. Several sensitivity simulations have also been performed and used in conjunction with 3 strongly aggregated measures of error to point out directions of improvements and future research foci. The paper is clearly organized and well written and reaches several important conclusions.

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Given that only three condensed measures of error are reported I think that the interesting sensitivity runs deserve a longer discussion (or mentioning at all). It is strongly recommended to add at least the values of the three ϵ-parameters (instead of just plotting "+" or "-") to Table 2 and to extend the discussion in Section 8 giving some more information on each of the sensitivity simulations listed in Table 2. This could be very helpfull for other researchers which might focus on specific sensitivities in future studies. It is also more scientifically sound to address all simulations and not only the ones that were qualified as improvements.

Specific Comments:

The statement in the introduction that by the use of the defined measures of error the influence of meteorology and photochemistry on the model results is separated does not hold in a strict sense and is misleading. The influence of meteorology has been shown by the use of model results from different years (which still reflect the combined effect of meteorology and photochemistry) and not by the definition of the error. Ideally, also the ϵ_2 error should improve upon use of the correct meteorolgical year. If it doesn't, this shows that other model deviciencies still have a large impact on the model results.

To my knowlegde all chemical mechanisms used in 3D large scale models currently make some use of the lumping of species (which may be named "family concept"). It would therefore be much more precise to classify the CBM-4 scheme as a scheme using structural lumping as opposed to molecular lumping (see e.g. Gery et al., J. Geophys. Res., 94, 12925-12956 or Dodge, Atmos. Environ., 34, 2103-2130. 2000) instead of using the misleading term "family concept". The latter expression is more commonly used for a solution concept of the differential equations of a chemical system in which the solution of strongly interacting species (the family) are solved for separated from the interaction with other species (see e.g. Austin, J. Geophys. Res., 96, 12941-

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12974, 1991). This expression should therefore be avoided.

Section 2.2, line 7. The statement "... such that low NO_x conditions are also well captured." is too euphemistic in view of more than 60% deviation compared to the RACM chemical scheme shown by Houweling et al. (J. Geophys. Res., 103, 10673-10696, 1998) for OH. I suggest to replace this with "... such that low NO_x conditions are better represented".

The method of determining the tropopause height in the model should be briefly reported, even though the error associated with this is probably small compared to the uncertainties in the separation of the stratosphere and troposphere in the satellite data (see also comment below).

It is not clear why only the meridional averages of model and satellite data are used in the calculation of the errors. In Figure 1 for example it can be seen, that the meridional gradients near the Atlantic ozone maximum are even reversed in the model and satellite data. Those information will be lost in the procedure described in the paper. The definition of ϵ_1 and ϵ_2 could be easily extended to use all pairs of data points. With this it can be hoped that the differences in the sensitivity simulations would be more distinct.

The argument in Section 8 (page 10, right column) that transport of biomass burning products from the PBL into the free troposphere is too weak due to the low temporal resultion of the PBL growth is not convincing. Given the uncertainty in such parameterizations the scheme could nevertheless over- or underestimate the actual exchange. A more interesting point is the lacking local thermal perturbation associated with biomass burning which should lead to a higher effective injection height of the biomass burning products.

The above points, however, are only minor compared to the importance and robustness of the conclusions reached in this highly recommended paper.

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