Atmos. Chem. Phys. Discuss., 6, S6679–S6682, 2007
www.atmos-chem-phys-discuss.net/6/S6679/2007/

Atmospheric Chemistry and Physics Discussions

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

B. Sauvage et al.

Received and published: 6 February 2007

We would like to thank referee#1 for her/his useful comments on our ACPD paper " Remote sensed and in situ constraints on processes affecting tropical tropospheric ozone ". The comments and suggestions clearly strengthen our manuscript. Below is the final response to the different comments.

Anonymous Referee #1 Received and published: 11 January 2007 General comments. This paper is a well-written, high-quality modeling/data analysis of processes affecting tropical tropospheric ozone. One of the strong points of this work is the simultaneous assessment of multiple satellite and in-situ measurements of ozone and its precursors in a modeling framework well suited to assess the sensitivity of ozone profiles to various processes and uncertainties in emissions. The characterization of the uncertainties in various parameters and ranges in calculated values are quite helpful for

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

allowing the reader to assess the precision of the various calculations. The relatively consistent improvement in magnitude and variation of ozone compared to a variety of measurements in most, but not all cases, provides significant evidence that the model processes are correct within the stated uncertainties. The bounding limits on the lightning NOx influence in the context of much larger scientific uncertainty of that process is welcome, particularly in the context of the authors' meteorological analyses and the scaling to the LIS/OTD measurements. Likewise the sensitivity to heterogeneous reactions involving HNO3 and HO2, while not as convincing at the other sensitivity studies, remains a contribution to the literature. With the model/measurement agreement approaching the measurement uncertainties in most cases (MAM and SON CO at Dubai and SON ozone at Ascension are curious exceptions) the fidelity of these calculations is remarkable.

This paper is suitable for publication in ACP following revision to address the following comments:

1) Abstract: "biomass burning inventory is larger by a factor of 2". Larger than what?

Reply: The biomass burning inventory is larger than the bottom up inventory by a factor of 2. This is corrected in the revised version.

2) Pg.2, 1st para: replace 'confounded' with 'limited'.

Reply: Correction will be done in the revised version.

3) Pg. 3, 2nd para: How does GOME provide NO2 in cloudy pixels? The effect of using pixels with 50% or 70% (which is it?) cloud fraction could be significant to the reported tropospheric NO2 amounts.

Reply: As pointed by referee#1, this explanation was not clear. For the all GOME data, molecules are retrieved for observations in which less than 50% of backscattered intensity comes from clouds. This is better explained in the revised manuscript.

4) Pg. 4, Sect. 3.2: The terminology "Standard (improved)" is confusing. Original and

ACPD
6, S6679–S6682, 2007

improved would make sense; however, anticipating third and subsequent versions, the authors might prefer to give the model configurations version numbers.

Reply: Helpful suggestion. In the revised version we will change “standard” to “modified”. In this study we use version 7-02-04 in the original version and then develop a modified version. We can not anticipate the subsequent versions of the model by giving version numbers, as updates of the model including our modification are under process and other versions of the model have already been released.

5) Pg. 7, 3rd para: The disagreement in SON ozone at Ascension (figs 5 and 10) begs for an explanation.

Reply: Good point. Indeed the disagreement is puzzling. We suspect that it reflects a seasonal bias in the altitude at which lightning NOx is released into the upper troposphere. We further discuss this issue in the manuscript in reference to the simulations with GEOS-3 (section 4.4), and with the increased IC/CG ratio (section 4.1.2), both of which contribute to a seasonal improvement.

6) Pg. 8, sect 4.1.2b: the suggestion of a seasonal variation in the IC/CG ratio should be clearly identified in the conclusions as a point of interest for further study.

Reply: As suggested, we add this point in the revised version. We also update our manuscript with lightning NOx source best estimate of 62 Tg N/yr, following for recent Hudman et al., JGR, 2007 paper, suggesting 1Tg N/yr increase over mid-latitudes deduced from ICARTT campaign. This has no effect on our Tropical analysis.

7) Pg. 8, sect. 4.2.1: The HCHO (and NO2 elsewhere) model/GOME correlations are remarkably high. The authors should clearly describe the relationship between the GOME retrievals and any a priori constraints that might influence the model/measurement correlations.

Reply: In the revision of the paper we will include model/GOME correlations between satellite slant columns and the model, then before the AMF is applied to calculated

ACPD

6, S6679–S6682, 2007

retrieved columns. In that way we address reviewer concern by avoiding any GEOS-Chem influence in the correlations calculations. This gives lower calculations between modified simulation/ GOME and original simulation /GOME, but the modified version still presents higher correlations, for NO2 and HCHO.

8) Pg. 9, end of 4.2.2: The authors should consider elucidating the point that the Scan-angle Method is the only one of several satellite tropospheric techniques that captures the correct seasonal variation of tropical tropospheric ozone over Africa. The suggestion that the vertical profile of ozone and instrument sensitivity to that vertical distribution may likely be the key aspects of capturing the true variation. Perhaps there really is no paradox.

Reply: We thank reviewer for his/her good suggestion. We now elaborate on the attributes of the Scan-angle method in capturing the true regional variation. Indeed we believe that there is no paradox over the African continent.

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