

***Interactive comment on* “Tropospheric column ozone: matching individual profiles from Aura OMI and TES with a chemistry-transport model” by Q. Tang and M. J. Prather**

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We thank anonymous referee 2 for careful reading of the paper and constructive suggestions. The following is our reply (questions in *italic*).

Overall, the paper is well organized and well-written. There is a lot of dense discussion in section 3, I would recommend that subsection headings be added so it is easier for the reader to digest.

Added subsection titles “Comparisons of monthly 2-D TCO PDFs for NH middle latitudes and tropics” and “Comparisons of monthly mean TCO latitude-by-longitude patterns” in section 3.

Specific comments:

1. *The paper of Zhang et al (2010) is cited in this work, as a reference to the DOFs of the TES and OMI data and validation against ozonesondes. In fact, the Zhang et al paper also evaluates the TES and OMI data with a CTM. This paper should discuss the Zhang findings, and integrate a discussion of the Zhang comparisons with a CTM with this comparison with a different CTM.*

This is our oversight. We were aware of Zhang et al., 2010, and recognized that they apply a CTM to compare TES and OMI ozone data. Their results examine specific levels (i.e., 500 hPa and 860 hPa) and thus it is not straightforward to directly compare with our TCO results. Their methodology and equations will be described in the revised paper. We will reference the Zhang et al paper upfront, noting their use of a CTM, and noting how we are extending this to TCO.

2. *In fact, on page 16069 you discuss ozone from biomass burning and hypothesis that TES and OMI have limited sensitivity, so don't report elevated ozone. Yet, Zhang (section 3) says that both TES and OMI see enhanced ozone due to biomass, and the Geos-Chem model is low. You argue that TES and OMI are low relative to your model. What evidence do you have that your model is correct in these regions?*

We hypothesize that either model error or TES and OMI's low sensitivity in the lower troposphere causes the model-measurement differences in the biomass burning region on page 16069 line 15-19: "These model-measurement biases are consistent across both OMI and TES and suggest model deficiencies in these regions, but may also be a consequence of common features of OMI and TES...". The CTM TCO is closer to the TCO data derived from OMI total column and MLS stratospheric column using a residual method (Ziemke et al, 2009 Fig. 1). In South America, Ziemke et al., 2009 show a TCO value of 45 DU for July 2005 and 40 DU for January 2006. Parallel results for Western Africa are 55 DU and 40 DU. Other than that, we cannot explain the Zhang conclusion regarding GEOS-Chem.

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More reliable measurements (e.g., SHADOZ ozonesonde, aircraft) are required to have a better idea about the “true” values of biomass burning TCO, but this single result is not core to this OMI-TES comparison, and full resolution is beyond the scope of this study.

At the end of this paragraph, add “In the biomass burning regions (e.g., South America and Western Africa), the CTM reports closer TCO values to those of Ziemke et al, 2009 than OMI and TES in these months. On the other hand, Zhang et al., 2010 finds that OMI and TES can detect enhanced ozone due to biomass burning at 500 hPa. Therefore, further analysis and measurements (e.g., SHADOZ ozonesonde) are required to determine which TCO is closer to the reality.”

3. The analysis focused on the years 2005-2006 - why were these selected? Was this driven by the observational dataset, or some aspect of the model results?

The CTM is driven by a particular pieced-forecast ECMWF meteorology. We only have such fields on $1^{\circ} \times 1^{\circ}$ resolution for 2005–2006. The remaining fields are at coarser T42 resolution and thus not suitable for this study. We note that our results greatly expand the single-year study of Zhang to begin to look at year-to-year variations.

4. There is some discussion of the influence of the priors on the TES and OMI data. The authors should consider if there is value in including plots of the prior in the paper. Or, reference an available source of this information.

We referenced the a priori sources (McPeters et al., 2007; Kroon et al., 2011 for OMI and Brasseur et al., 1998; Park et al., 2004 for TES) in section 2.

5. There are two points that are made in the discussion that are not well captured in the conclusions. I suggest the authors consider including - a statement about where TES and OMI have the most sensitivity to TCO and where they are less sensitive - the importance of applying the same prior when making direct comparisons of TES and OMI.

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The reviewer is correct in the interest/importance of these points. We believe that this is now highlighted in the conclusion section due to a range of revisions, but could find no specific fix to address this.

6. The conclusions state that it is astounding that there are no significant differences in the TES day and night data. Although there is less material published on this topic, there was a great deal of care to maximize the thermal stability of the instrument. In addition, scattered sunlight is minimized, but of little concern for the wavelengths used in the ozone measurements. I'd suggesting changing the word from astounding to reassuring. That was the planned result!

Agreed and revised as suggested.

technical corrections

1. page 16068 - 4 lines from the bottom. Do you really mean to say " Inadvertently, the OMI data are excluded where the surface pressure is less than 700 hPa,". That makes it sounds like you made an error in data processing and left it that way.

Revised to “The reported OMI AK contains unreasonable values where the surface pressure is less than 700 hPa, and we drop those data.”

2. In acknowledgements - I think you mean OMI and TES researchers, rather than researcher

Yes, revised.

Reference:

Ziemke, J. R., S. Chandra, B. N. Duncan, M. R. Schoeberl, O. Torres, M. R. Damon, and P. K. Bhartia (2009), Recent biomass burning in the tropics and related changes in tropospheric ozone, *Geophys. Res. Lett.*, 36, L15819, doi:10.1029/2009GL039303.

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