**Atmospheric
Chemistry
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Discussions**

Interactive
Comment

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

Anonymous Referee #2

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Overall quality -

This paper provides an extensive analysis of the TES and OMI ozone measurements (focused on tropospheric ozone columns) and comparison to a chemical transport model (CTM). THe goal of the work is to use the CTM as a transfer standard to assist in identifying weaknesses in the observation data as well as providing cross-validation and an independent assessment of observation error. The paper begins by introducing the remote sensing datasets, and then presents the CTM data and the details of the satellite datasets that were utilized. The third section discussed the comparisons of the tropospheric column ozone (TCO). This discussion presents scatterplots and PDFs of

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Interactive Discussion

Discussion Paper

the TCM, TES, and OMI for NH midlatitudes in July 2005 and January 2006. An analysis of the impact of a priori followed, and then results focused on the tropics. Next, inter-comparison of monthly means on maps are presented and discussed. The forth section is on the topic of day-night differences of TES observations.

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.

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.

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?

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?

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.

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

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!

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.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 16061, 2012.