

***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**

Q. Tang and M. J. Prather

qtang@cornell.edu

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We thank anonymous referee 1 for the constructive suggestions and helpful comments, which improve the paper. The following is our reply (questions in italic).

Comments

Page 16062, line 23, Abstract. Use of past tense (“highlighted”). Given that the Abstract is written in the present tense, I think this should also be the case here.

Yes, changed to “highlight”.

Page 16062, line 25, Abstract. Change to “This study also highlights . . .”, given that in

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line 23 a first “highlight” is mentioned.

Yes, revised.

Page 16063-16064, Introduction. The introduction is very clear and nicely explains what problems are encountered in the use of OMI and TES TCO in scientific studies, but I miss the exact motivation for this study. It will probably be something like “Because of these issues in using these TCO measurements, we present a detailed analysis of . . . etcetera”. Please provide a brief motivation as to why this study, preferably on page 16064, after line 6.

Add “Because of the above problems in comparing and using these TCO datasets” at the beginning of line 7 on page 16064.

Page 16066, line 10, Section 2. It is stated that “Given the limited DOFS in the troposphere, their integrated TCO is expected to be less dependent on the AK, . . .”. I don’t understand, less dependent than what? And why should that be the case? Both UVVIS and IR measurements of the TCO suffer from reduced vertical sensitivities (depending on the wavelength due to line of sight angles, albedo and the temperature structure of the atmosphere). That doesn’t change by combined levels of a profile to a column product. Please clarify.

Changed to “. . .less dependent on the AK than on the a priori. . .”. DOFS is a measure of the vertical sensitivity of the observation. Larger DOFS values indicate that more observational signal goes into the final retrieval product, and vice versa. Because of the low vertical sensitivities (small DOFS) of both OMI and TES in the troposphere, their TCO retrieval is largely determined by the a priori rather than AK. So, here we only adjust the differences in the a priori.

Page 16067, lines 4-6, section 3. It is noted that certain biases are larger in July than in January, and as possible explanation it is mentioned that this could be due to larger TCO variations over NH mid-latitudes during summer compared to winter. For

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now I challenge that notion, as I would think that TCO variations are larger in winter than in summer (summertime NH = smaller tropopause height variations, less strat-trop exchange). However, whether or not this could be the case could be clarified by looking at model results: is variability in summer indeed larger than in winter.

Figure 1 below shows the CTM TCO standard deviation (STD) (unit: DU) on $1^\circ \times 1^\circ$ grids for June and December 2005. The STD is calculated from the hourly model output. The TCO variations are apparently largest in summer (at the sub-tropical jet) than in winter over NH mid-latitudes. The tropopause height is defined as the highest level where the e90 abundance turns stratospheric, and this leads to the inclusion of stratospheric ‘folds’ when there is a double tropopause. In the model this fold is well resolved, and we could remove this from the CTM TCO by using the e90 tracer (Prather et al, 2011), but because OMI and TES have very coarse vertical resolution near the tropopause it is not clear how to do this. Effectively we assume that the stratospheric fold is included in the OMI and TES values below the upper tropopause. Using a single tropopause definition can cause artificial TCO variations at transient regions where the tropical and extra-tropical tropopause overlap. To remove these artificial variations, in our previous study (Tang and Prather, 2010) we calculate the TCO variations separately for tropics and extra-tropics and get similar seasonality of TCO variations.

Page 16067, lines 11-12, section 3. CTM-OMI correlations appear much higher than CTM-TES correlations on a profile-to-profile basis. One thing that could be tested is whether or not the sampling could be causing this by doing the CTM-OMI comparison on the CTM-TES sampling. If that results in a similar lower correlation for CTM-OMI, you know that it is the sampling that is an issue here.

We re-did the CTM-OMI comparisons on CTM-TES coincident as suggested by the reviewer. The CTM-OMI correlations (as indicated by R^2) become larger (from 0.87 to 0.90 at NH mid-latitudes and 0.55 to 0.72 at tropics) in July 2005. In January 2006, the R^2 values get larger (0.66 to 0.72) at NH mid-latitudes, while slight decrease (0.41

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to 0.39) at tropics. Therefore, the lower correlation for CTM-TES than CTM-OMI is not caused by the differences in the OMI and TES sampling. We will add the CTM-OMI subsampling results on the label of Figs. 1 and 2 and change the text accordingly.

Page 16067, lines 18-25, section 3. One question that came up here is to what extent the comparison OMI-TES improves if monthly means are used. This study obviously focuses

This is an interesting question, but beyond the scope of this paper, which focuses on coincident comparisons. We have the plan to do similar analysis on monthly means to see how different they are from coincident pairs.

Page 16068, line 13, section 3. It is found here that OMI misses the low ozone part of the Pacific. It appears very well possible that this is related to the OMI a-priori, which is a zonal mean and given the wave-one structure of tropical ozone thus not representative of the Pacific low-ozone, and reduced sensitivity to the lower troposphere (OMI thus being filled by too high a priori values). Is there a way to get to that conclusion based on this analysis? If so, I would suggest adding it specifically as it highlights the importance of including the vertical sensitivity in satellite TCO measurements – which is still overlooked issue.

Yes, I think that the results (differences between OMI-TES and OMI-TES*) can lead to the conclusion that the missing low ozone over the Pacific is primarily due to the OMI a priori with high ozone in this region and the conclusion will be revised accordingly. Adjusted with the OMI a priori, the OMI-TES difference reduced to 5 DU from 25 DU, which means a TES TCO enhancement of about 20 DU using the OMI a priori in this region.

Page 16069, lines 2-3, section 3. The latitudinal jumps in the averages are attributed to the OMI a priori. But why? Because of jumps in the a-priori? It is not mentioned, so please explain.

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It is because of the jump in the a priori and stated that "...attributable to latitudinal jumps in the OMI a priori profiles."

Page 16069, line 8, section 3. It is stated that the inability of OMI to report low TCO values is likely the result of OMI's fitting algorithm (based on personal communication with X. Liu). Please elaborate in very general terms on why this is the case. What specifically hampers the retrieval? Because this statement is based on personal communication there is no way to check this claim.

The inability to report low TCO values appears to be a problem for this specific OMI ozone product (OMO3PR V003). The other product (Liu et al., 2010) does not have this problem. The basic retrieval technique of these two products is the same, but there are many differences in details, especially in wavelength and radiometric calibrations and forward model simulations.

The sentence is rephrased to "Although OMI has some sensitivity down to the surface, the inability to report such low TCO over the equatorial Pacific and high ozone over the South Atlantic likely results from the retrieval fitting algorithm of this OMI ozone profile product (OMO3PR V003), as another retrieval algorithm with the same monthly zonal mean climatological a priori (McPeters et al., 2007) can capture such wave-1 pattern in the TCO (Liu et al., 2010, Fig. 8c)."

Page 16069, lines 15-19, section 3. It is argued that overestimation of ozone over biomass burning areas may be related to low sensitivities in the lower troposphere. But that should have been taken into account by the averaging kernel, and thus quantifiable. Apparently averaging kernels do not explain these differences, so I wonder if this conclusion is substantiated. There are other possibilities – aerosol effects come to mind – but without additional information that is mere speculation. If there is no clear evidence that the vertical sensitivity is a play here, this should be rephrased and the vertical sensitivity should be avoided. Please clarify.

What we argue here is the overestimation of the model relative to measurements is

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likely due to the fact that these retrievals miss the high ozone related to biomass burning in the lower troposphere. Over these biomass burning areas, the true O₃ abundance is generally higher than the a priori information which is based on the zonal mean climatology. Given the low sensitivity in the lower troposphere, the satellite retrieval will have little knowledge to adjust the low-biased a priori to higher values, and thus cause underestimation in their final TCO. It is, however, also possible that these positive signals over biomass burning regions reflect the high bias of the model. But considering the seasonality of the geographic pattern, we still think the differences are more likely because of missing high O₃ of biomass burning in the measurements.

Reference:

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McPeters, R. D., Labow, G. J., and Logan, J. A.: Ozone climatological profiles for satellite retrieval algorithms, *J. Geophys. Res.*, 112, D05308, doi:10.1029/2005JD006823, 2007.

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

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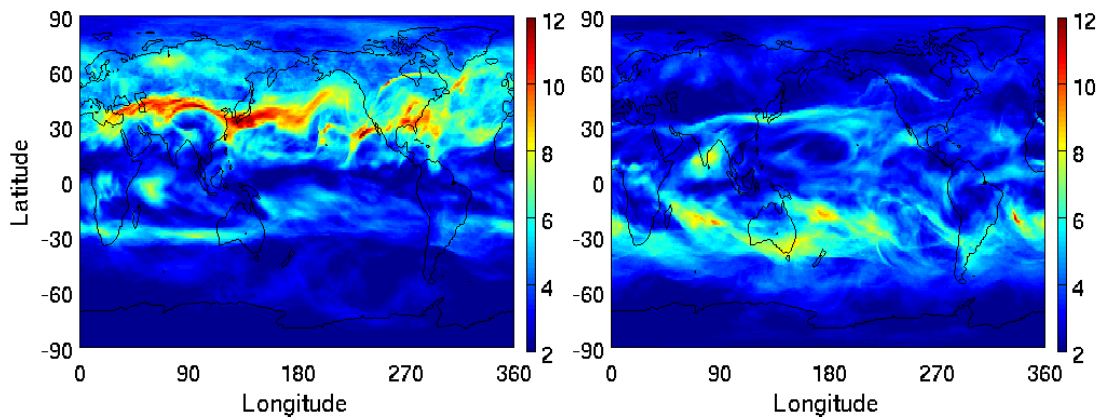


Fig. 1. Latitude-by-longitude CTM TCO standard deviation (unit: DU) on 1 degree by 1 degree grids for June (left) and December (right) 2005.

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