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Atmospheric Chemistry and Physics Discussions

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 are thankful to anonymous referee 3 for helpful comments and constructive suggestions. We have carefully considered all the questions and suggestions and incorporated them as follows (questions in italic).

This study uses the CTM as a transfer standard to conduct a three-way comparison of CTM, OMI, and TES tropospheric column ozone and evaluate the effect of a priori on retrievals. It demonstrates that the use of relatively noise free CTM can improve the validation of measurement precision much better than direct comparison and also reveals some weakness in CTM and both OMI/TES observations. This paper is generally

well written and organized. It makes important contributions to the validation of tropospheric ozone column and is suitable for publication on ACP. However, more references could be cited to help interpret the results e.g., those with respect to wave-1 pattern, enhanced ozone over the South Atlantic (not just limited to lower troposphere and not just due to biomass burning), the sensitivity of OMI to capture the wave-1 pattern. As pointed out by reviewer 2, Zhang et al., 2010 is cited but not cited for its main work, which provides a theoretical framework to use the CTM as the transfer standard to compare observation with similarly coarse and yet different vertical resolution by using OMI and TES ozone (focus on ozone at 500 hPa). The figures could be improved by adding several panels. Some of the discussions are not clear. The abstract/conclusion emphasizes that OMI and TES biases are within a few percent, but it should be clearly stated that this is for monthly zonal mean biases as there are large monthly mean biases at many places. Overall, I recommend this paper to be published on ACP after addressing the following specific comments.

Overall, we agree with the reviewer here, and more references regarding wave-1 pattern and the OMI's sensitivity to this pattern will be added as suggested. The reference to Zhang et al., 2010 will be revised for its theoretical study on using CTM as an intercomparison platform to compare different satellite measurements. The comments on figures as well as abstract and conclusion are addressed in the following.

Specific comments 1. In Abstract, add "zonal" between "monthly" and "mean OMI-TES" on L18 and add "zonal" before "OMI TCO" in line 19.

Yes, revised.

2. P16063, L10, Zhang et al., 2010 should be cited as it also provided a theoretical framework of using a CTM as a transfer platform to compare OMI and TES retrievals.

Agreed, done.

3. P16064, L20, Levelt et al., 2006 is a better reference for OMI.

Levelt et al., 2006 only contains some brief, general information about the OMI ozone profile product. We still think OMI Team, 2009 is better here, but will include both as one in the regular literature.

4. P16066, L9-10, I agree with reviewer 1 that this sentence is difficult to understand. I suggest changing it to "Given the limited DOFS (vertical sensitivity) in the troposphere, their integrated TCO is less dependent on the a priori information than ozone at individual tropospheric layers due to the removal of smoothing errors between tropospheric layers in the integrated TCO, and thus we do not adjust the retrievals due to AK differences in the direct OMI-TES comparison."

Revised to make this sentence clearer (see reply to reviewer 1).

5. In Figure 1 caption, please define the mean biases (i.e., y-axis variable minus x-axis variable) although it is defined in the text.

In Fig. 1 caption add "The mean bias and standard deviation are defined for the vertical-axis variable less the horizontal-axis variable".

6. In Figures 1 and 2, it would be very useful to add another panel by comparing CTM TCO vs. TES TCO, which can help the readers understand the CTM vs. OMI/TES comparison with the same a priori (e.g., understand the sentences on P16068, L1-7).*

We think the key point here is to illustrate the importance of using the same a priori when comparing different satellite observations. The mean biases and the standard deviations of the difference of CTM vs. TES* will remain the same as those of CTM vs. TES, since CTM less TES equals to Ates(Xm-Xt) – Btes – Etes, which is independent of the a priori. Although the patterns of the 2-D PDF will be different, we do not think adding more panels of PDFs in Figs. 1 and 2 is the best choice to show the impact of a priori.

We propose to add TES* TCO latitude-by-longitude panels in Fig. 3. The differences in TES vs. TES* geographic patterns will clearly show the influence of different a priori.

7. P16067, L5, I agree with the reviewer 1 that the conjecture of "larger TCO variations over NH mid-latitudes in summer . . ." can be checked from model simulations. In addition, the retrieval sensitivity for OMI decreases due to the increase of solar zenith angle and the retrieval sensitivity for TES might also decrease due to the decrease of temperature, which can also cause the reduction of standard deviations.

The modeled TCO shows larger variations over NH mid-latitudes in summer than in winter (see reply to reviewer 1). The suggested causes of smaller TCO variation in winter due to reduced OMI and TES retrieval sensitivity will be incorporated, but we have also revised the text to note that the variability due to the summer sub-tropical double tropopauses is large and may dominate that due to wintertime variations in the tropopause height.

8. P16067, L2, is the statement of large TES TCO measurement noise based on the figure/table, which is not clear, or based on some references? Please make it clear or give some references. From Fig. 1 (a) and (c), the model-TES standard deviation is actually smaller than the model-OMI standard deviation.

The statement is based on the standard deviations of model-measurement shown in supplementary tables. Changed to "... TES TCO generally has greater measurement noise (see Tables S1 and S2 columns 6 and 9)" on P16067 L12.

9. P16067, L24-25, to accurately understand why the model-observation correlation is much larger than OMI-TES correlation, you need to start from the definitions of the quantities: Model TCO (Xm) with OMI averaging kernels: Xm,omi' = Xa + Aomi (Xm-Xa) OMI TCO: Xomi' = Xa + Aomi(Xt-Xa) + Bomi + Eomi Where Xt is true TCO, Bomi is OMI TCO bias and Eomi is OMI TCO noise Xm,omi'-Xomi' = Aomi(Xm-Xt) – Bomi – Eomi Xm-Xt is the model error Model TCO with TES averaging kernels: Xm,tes' = Xa + Ates (Xm-Xa) TES TCO: Xtes' = Xa + Ates(Xt-Xa) + Btes + Etes Xm,tes'-Xtes' = Ates(Xm-Xt) – Btes - Etes OMI TCO: Xomi' = Xa + Aomi(Xt-Xa) + Bomi + Eomi TES TCO: Xtes' = Xa + Ates (Xt-Xa) + Btes + Etes Xomi'-Xtes' = (Aomi-Ates)(Xt-Xa)

+ Bomi + Eomi – Btes - Etes The noise free in CTM makes the standard deviation of model-observation differences (either Eomi or Etes) much smaller than that of OMI-TES differences (both Eomi and Etes). If neglecting B and E and assume Xm = Xt, then model-observation correlation is 1 while OMI-TES correlation is not 1 due to retrieval sensitivity differences (Aomi- Ates)(Xt-Xa). Therefore, the larger model-observation correlation is not only due to the generally uncorrelated nature of OMI and TES measurement noise but also due to the OMI/TES retrieval sensitivity differences and the relatively small model error. It mentions that "noise . . . is larger than the model error." But I don't think it is clear from these equations about whether OMI or TES noise is larger than model error. Please clarify it.

The above equations such as (Xm,omi'-Xomi'), (Xm,tes'-Xtes'), and (Xomi'-Xtes') are helpful for understanding the theoretical basis of the analysis here. We will include some of the key equations as well as brief explanations in the method section and point to Zhang et al., 2010 for full details. However, these equations defining the differences for two variables are different from the correlation coefficient, which is defined as r = E[(x-E(x))(y-E(y))]/(σ(x)σ(y)), where E is the mean and σ is the standard deviation. From this equation, we can clearly see that there is no necessary link between the difference and the correlation. Since r is normalized by σ(x)σ(y), it is not affected by the systematic mean bias. If x-E(x) and y-E(y) always have the same sign, r will equal to 1, otherwise, if always have opposite sign, r = -1.

We define the observation "noise" as random errors in the measurement retrievals that are not physical and define the model "noise" as the high-frequency errors in the model due to missed timing of fronts, location of the jet or trop-folds. Note that this model noise is not the model mean bias. The conclusion that the model's high-frequency error (i.e., due to missing jet location, fold timing) is smaller than the observation noise is based on the result that both standard deviations (σ) of the model-measurement differences are smaller than that of the difference between the two measurements. The σ(OMI-TES*) includes the contribution from different AKs, which is relatively small, since the TCO

is less dependent on AK than ozone at individual layers as noted by the reviewer in the question 4. The sentence was revised to "We conclude that the noise in these two measurements is uncorrelated and the larger TES-OMI STD than model-measurement STD suggests that the noise is larger than the model error."

10. P16068, L7, "CTM-TES" should be "CTM-TES".*

It should be CTM-TES as there are no CTM-TES* results in Fig. 1.

11. P16068, L23, the sentence "only part of which can be due to the a priori" is not clear to me. How a priori explains why model-observation correlation is larger as the same a priori is used in both retrievals and model TCO processed with retrieval averaging kernels? The reasons should be similar to those at middle latitudes.

By using the same a priori, the TES-OMI correlation is improved, but still smaller than the model-measurement correlation. Therefore, the a priori is only part of the reason why the model-measurement correlation is better. Other reasons are as indicated above that TES-OMI correlation has additional terms, such as differences in AKs and uncorrelated measurement noise.

12. P16068, L26-27, does not TES also report surface pressure less than 700 hPa for these high terrain regions?

This has been revised for clarity. It is the OMI data rather than TES data that have problem for the places where the surface pressure is less than 700 hPa. Since OMI-TES comparison requires both datasets are available, we cannot conduct analysis for these regions.

13. P16069, L8, as reviewer 1 also suggested, it would be better to elaborate this rather than based on personal communication. It is well known that the TCO derived using tropospheric ozone residual methods from TOMS/OMI total ozone minus stratospheric column ozone can capture the wave-1 in the tropics, low ozone in the Pacific and high ozone (not limited to lower tropospheric ozone only but also in the middle and upper *troposphere as seen from Figure 4 of Thompson et al., 2003, not just due to biomass burning but a combination of biomass burning, lighting and dynamics as shown in Martin et al., 2002) over the Atlantic (e.g., references as early as Fishman and Larsen, 1987 to recent references like Ziemke et al., 2006 and Schoeberl et al., 2007). The wave-1 pattern, originating from total ozone, suggests that UV retrievals even with a few wavelengths like the TOMS algorithm have the sensitivity to see the wave-1 pattern. Ozone profile algorithm, which uses many more wavelengths, ideally should have more sensitivity to better capture this pattern. The papers by Liu et al. (2005, 2006, 2010) using a similar ozone profile retrieval algorithm (also with zonal mean a priori) clearly show ozone profile retrievals from GOME and OMI can clearly captures the wave-1 pattern: from as low as 10 DU over the Pacific to as high as 60 DU over the Atlantic. However, the retrievals need to fit the measurements in the 310-340 nm to better than 0.2-0.3% to extract useful tropospheric ozone information. Therefore, the inability to capture wavel-1 pattern in the operational OMI product is an algorithm specific issue rather than the OMI/UV's limitation.*

Yes, missing the tropical TCO wave-1 pattern is a problem specific to this OMI product rather than a limitation in OMI technique and measurement. The sentence was rephrased (see reply to reviewer 1) and the suggested references will be added where applicable.

14. P16069, last paragraph, according to Zhang et al. (2010), a better approach to evaluate the true OMI and TES biases than OMI-TES or OMI-TES is the CTM method by using CTM as transform standard (equation 13 of Zhang et al., 2010), i.e., the differences between CTM-OMI and CTM-TES*. I suggest adding a panel for CTM-TES* and a panel for (CTM-OMI) minus (CTM –TES*).*

The panel for (OMI-CTM) minus (TES*-CTM) will be added together with TES* as an additional row at the bottom of Fig. 3. As stated in the answer to comment 6, CTM-TES* will be the same as CTM-TES (Fig. 3 j, l), so no need to duplicate them. The differences between OMI-TES* and (OMI-CTM) minus (TES*-CTM) will show the

advantage of using a CTM to intercompare different satellite measurements. The OMI-CTM results will be subsampled for TES-CTM coincident when calculating (OMI-CTM) minus (TES*-CTM).

15. P16072, L10, I think that near uniform zonal bands of OMI TCO in both the tropics (without the well-known wave-1 pattern) and mid-latitude with jumps makes it not suitable to derive longitudinal and latitudinal dependent TCO climatology.

As discussed above, these OMI TCO problems are product-specific. The other OMI ozone product (Liu et al., 2010) captures the wave-1 pattern and does not have mid-latitude jumps and hence can be used to define a TCO climatology. Both products use the same a priori following McPeters et al., (2007), so the differences are likely due to how details are handled, especially in wavelength and radiometric calibrations and forward model simulations.

On L11 add "Since this OMI product (OMO3PR V003) encounters problems, such as lack of tropical wave-1 pattern and uniform middle latitude TCO bands, another OMI product free of these issues (Liu et al., 2010) is a better choice for defining the TCO climatology."

16. P16072, L17, I suggest changing "implying large single profile noise in each observation" to "due to the inclusion of both OMI and TES profile noise" according to comment 9.

Agreed.

17. P16072, L18-19, see comment 9, it is not clear to me about why OMI/TES TCO noise is larger than model uncertainties.

See answer to question 9.

18. P16072, L21, Do you mean "this level of noise" as sigma(OMI-TES*) in those tables? Please make it clear in the text. In supplement tables, how is sigma*(OMI-TES*) calculated? Do you calculate the sigma(OMI-TES*) for each 5x5 box and then* *take the average of all boxes within the given latitude bands or do you take the root mean square of sigma(OMI-TES*) in all boxes? Please make it clear in the Table S1 footnote. According to the tables the noise is mostly 2-4 DU except for S4 and S8 (3-5 DU). I think that this combined noise of 2-4 DU is very small for single OMI and TES measurements. What is the single measurement requirement to detect tropospheric folds or stratospheric intrusion, whose signal I think can be much larger than 2-4 DU?*

Yes, by "noise" we mean sigma*(OMI-TES*) and we indicated it in the parenthesis "(see . . . for OMI-TES* STD)". The sigma*(OMI-TES*) is calculated as the root mean square of (OMI-TES*)', where (OMI-TES*)' is defined as (OMI-TES*) minus mean (OMI-TES*) for each 5x5 box.

The tropopause folding and stratospheric intrusion events occur on variety spatial and temporal scales, for example, Hsu et al., (2005) shows column O_3 anomalies due to westerly duct (Figs. 2 and 3) that would give 10–20 DU, but these are very large and anomalous compared to those near the jet, which are a few DUs. Note that the 2–4 DU noise is averaged over a large latitude range containing $\sim$1000–4000 times of observations per month. The single measurement noise should be larger than this averaged noise. But we are not clear how to precisely quantify the single measurement noise. We could not find such established approach in the literature. Worden et al., (2009) introduces an approach to estimate the upper bond of the retrieval bias. Our previous case study (Tang and Prather, 2011) shows that the individual Aura retrieval is too noisy and usually incapable of detecting tropopause folds or stratospheric intrusions. However, these events can be identified by the high anomaly in the geographic pattern of OMI TCO, which is stated in our manuscript.

19. P16073, L24-25, since the small OMI/TES biases refer to the zonal mean biases, it is good to make it clear in these conclusive statements. I suggest adding "zonal mean" before "OMI". It says "OMI and TES agree within a few percent and imply that OMI TCO bias is at most a few Dobson units". Note that 1 DU is about 3% based on global average TCO of $\sim$30 DU. According to the supplement tables, the OMI-TES bias is*

typically within 3 DU. I suggest changing it to "zonal mean OMI and TES typically agree within 5-10% and imply zonal mean OMI TCO bias is at most a few DU" and making similar changes in the abstract.

Thanks. Revised as suggested.

20. P16074, L2, I suggest changing "two instruments" to "two instruments/algorithms" to reflect that it is due to both instrument and algorithm differences.

Thanks. Adapted.

Reference:

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Worden, J., et al. (2009), Observed vertical distribution of tropospheric ozone during the Asian summertime monsoon, J. Geophys. Res., 114, D13304, doi:10.1029/2008JD010560.

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