

Interactive comment on “Correlating tropospheric column ozone with tropopause folds: the Aura-OMI satellite data” by Q. Tang and M. J. Prather

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We thank anonymous referee #2 for the helpful and valuable comments and suggestions. We can revise the manuscript to address most of all of these comments as described below.

General comments:

Since this is a combination model/analysis study, avoid qualitative statements that could be checked or stated directly, for example: ‘not found in the OMI observations, likely because . . .’

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Yes, we will tighten up the language overall. This sentence will be changed to ‘not found in the OMI observations, because . . .’

‘probably due to redistribution of pollution plumes in the 1 x 1 grid’

Changed to ‘due to the smoothing of the Hong Kong pollution plume on our 100-km grid’.

‘OMI essentially provides daily coverage’ -> if your concern is the swath width, there is daily coverage of the sunlit Earth for the first few years of OMI operations (prior to the row anomaly).

Rephrased as ‘OMI provides daily coverage except for the points dropped due to OMI quality control and the gaps between swaths in tropics’.

‘the difference might be due to the climatological a priori’ (discussion of figure 4).

We are not sure if there are other sources, besides the climatological a priori, contributing to this difference. The sentence will be rewritten as ‘an obvious reason for the difference is the use of a fixed climatological profile as the a priori in the retrieval’.

‘probably due to the more zonal structure in the SH, as large scale planetary waves may drive the location of final STE mixing away 25 from the jet in the NH.’

We have no simple fix for this comment — since this is only an hypothesized cause.

Major comments:

Clouds will have an impact on your analysis and are not discussed at all. Any pixel with thick cloud will have something done to it to account for the (unmeasured) ozone under the cloud. There are a number of possible approaches, and I am curious what you did about this.

Clouds were already considered in the retrieval of OMI ozone profiles. A missing value

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is reported for the total column data as well as the profile when the retrieval cannot pass the quality control. In some cases with thin, high cloud, OMI retrieves both, and we use them.

The OMI profiles have only one degree of freedom in the troposphere. The statements that follow this

Thus, TF detected by OMI would be represented as an overall enhancement in column, but not as a fold defined by sonde or CTM. OMI profiles (magenta dot dash line) generally underestimate ozone values in lower and middle troposphere but overestimate them in upper troposphere and lower stratosphere (Fig. 1).

are worrisome. From the cited document: It is recommended to be extremely cautious with any conclusions on tropospheric ozone based on these data. Personally I hate statements like this one – I don't know who is judging whether or not the user has been 'extremely cautious'. I suggest that the authors attempt to show that the interpretation used in this paper correct, either via better referencing or some good case study examples from the sondes. The latter seems like it would be an obvious step, since you have already gone used the sonde data to evaluate the CTM. I don't think that Figure 1 supports the idea of an 'overall enhancement' where a sonde or the CTM predict a fold, and it is hard to imagine where to place the tropopause that the estimates of tropospheric ozone would match among the CTM, the sonde, and OMI.

The sentence mentioned above will be modified as 'Thus, TF detected by OMI could at best be detected as an enhancement somewhere in column, most likely in the troposphere, but not as a fold resolved by sonde or CTM'. We assume the OMI retrieval algorithm is working properly.

We also noticed that the OMI ozone profiles in the troposphere have not been evaluated. But the good agreement shown here between OMI and CTM tropospheric col-

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umn ozone (TCO) should at least suggest that OMI retrieval derives reasonable TCO. Moreover, we compared the model results with Liu Xiong's OMI ozone product (*Liu et al.*, 2010a, not shown in the paper), and we also see good agreement with the geographic TCO pattern in this other independent product (we will note this in the revised paper). In addition, *Liu et al.* (2010b) validated the OMI profile against MLS data in the stratosphere and implied that the OMI TCO can be correct.

Ideally, if there are enough overlaps between sonde and OMI measurements, the OMI data in the troposphere can be evaluated as suggested by the reviewer. The timing difference between sonde and the OMI swath can be several hours and this is too large a mismatch for the fast-moving folds which tend to occur in strong westerlies. With the model, it is easy to simulate the observations of sonde and OMI.

The problem with the discussion of Figures 5c-d and 6c-d is that the features seem to be in the eye of the beholder. I don't fully see how to conclude that 5c and 6c show little month-to-month change. I can't stop looking at the organized red patch in 5d, that clearly slopes off the 1:1 line, and wonder where those points come from (geographically). This is discussed (p. 14883, l 5-6) but only after the nice statements about the symmetric errors and the 1:1 line. I am a little impressed by the number of comparisons, but really – with tropospheric O3 there are some vast regions with so little action that it is hard not to wonder how many points come from the low action areas. There is a lot to be said for the plot – clearly there are big regions of excellent agreement. You might consider refining the discussion with conditional (regional?) PDFs, that would address such questions – clearly the information is there to do this. I am not clear on what is meant by 'the probability of each comparison is weighted by pixel area and frequency' – in other words, how is this implemented in construction the 2D PDF? Does frequency means days per month?

Both Fig. 5c and 6c show similar persistent patterns in terms of positive areas over Africa and South America and negative regions over Pacific. These features remain for

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all the months (not shown in the paper), besides June and December. So, we still think there is little month-to-month change in Fig. 5c and 6c.

Yes, frequency is confusing. It should be written as the 2-D probability per DU^2 as plotted, includes all OMI-CTM coincidences in the month, weighted by their pixel area and normalized to one observation time per pixel per day. It is renormalized to give a 2-D integral of 1.0.

I found the discussion of switch from 1 x 1 by 40 to T42 with 60 layers confusing. Since you calculate the flux from the latter after spending a lot of effort evaluating the former – what does the latter do to the TF flux relative to the former? We must be giving up something, otherwise why not use the T42 by 60 for the whole thing?

We will fix the text concerning the switch from 1x1 40-layer to T42 60-layer fields and explain the reason more clearly. The 60-layer field has better stratospheric circulation and probably a better stratosphere troposphere exchange (STE) flux (*Hsu and Prather, 2009*) (assuming we know the correct answer, see *Olsen et al. (2003)*). The STE flux from 1x1 field is only slightly different from that calculated from T42 field.

Since OMI ozone profiles have 13 km x 48 km horizontal resolution, T42 (~ 280 km x 280 km) resolution is too coarse to compare with OMI and 100 km is just barely useful. Only 1x1 40-layer winds are available.

The remark that ‘convection extends into the lower stratosphere and drags O3 rich air into the troposphere’ seems to me to require more analysis and discussion. Do the seasonal cycles of STE flux in both hemispheres (and relative contributions) agree or disagree with previous estimates (e.g., Olsen et al., GRL, 2003)? During summer, the tropopause rises and the O3 in the lowermost stratosphere decreases to near tropical upper troposphere levels due to horizontal transport from the tropics. Strahan et al. [JGR, 1998] examine the seasonal cycle of CO2, showing that the seasonal cycle changes phase at the tropopause,

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and conclude that convective systems at mid latitudes do not penetrate into the stratosphere to contribute any significant troposphere to stratosphere mass exchange and that the most likely path of troposphere to stratosphere transport north of 30N is isentropic exchange near the subtropical jet.

The analysis of convection and STE flux will be in our next paper.

The seasonal cycles of STE flux disagree with estimates in *Olsen et al. (2003)*. But the cycles are consistent with our previous publications using the EC met-fields (*Hsu et al., 2005; Hsu and Prather, 2009*). In these studies, the method of diagnosing ozone flux is entirely self-consistent and is based on the flux across ozone isentropes. A serious study comparing STE ozone flux diagnosed from ozone vs. PV using the same met-fields would be very interesting as a collaborative effort among the different groups. We would need to diagnose differences and causes.

ER-2 aircraft typically operates at 65000 feet (~19.8 km) and thus the CO_2 measurements collected by ER-2 in *Strahan et al. (1998)* are way above the tropopause over mid-latitudes. At this altitude, the mid-latitude convective system has little impact on the atmospheric composition. Our hypothesis, however, refers to the transient layer (~1–2 km thick) near the tropopause and is based upon the coincidence of convective events and STE flux over summer continents. The mixing of tropospheric and stratospheric air within this transient layer around the tropopause has been diagnosed by the O_3 -CO and O_3 - H_2O correlations from various measurements, i.e., SPURT campaign (*Hoor et al., 2004*), and POLARIS campaign (*Pan et al., 2007*), and ACE-FTS (*Hegglin et al., 2009*).

Figures I find figures really difficult to interpret, in part because they are so difficult to see. Size and labeling add to this problem. The plusses in Figure 2 do appear to line up with the features to the extent that the features are not obscured by the plusses, but then the caption says ‘are correlated’ where from the discussion you say ‘co-located’.

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We will change the caption in Figure 2 to 'co-located' to be consistent with the discussion.

I suggest putting the SV part of figures 5 and 6 into a separate figure. I looked at the entire figures when reading the paragraph where the a-d are discussed and was confused because I didn't know what SV was (defined later in the discussion).

The reason why we put these 6 figures together is they are related and need to be seen together. For example, one of the important conclusions of this paper is that the locations with large SV and standard deviation (STD) coincide. Fig. 5e and 6e show the locations with large SV and Fig. 5 a,b and 6 a,b present where large STD are.

Figure 5 and 6: P. L 14 The large sigma observed at high latitudes cannot be explained by the model. OMI figures are b, correct – then the 'large sigma' is that small band of light yellow in 5b only (6b looks very flat) – is this what you mean?

This sentence will be changed to 'the larger sigma observed at high latitudes cannot be explained by the model'. In Fig. 5b, it refers to the yellow and blue, while in Fig. 6b, it only refers to the blue. Note that the blue in OMI figure is darker than in CTM figure.

Discussion of Figure 7 – please say how you calculate the TF frequency.

'The TF frequency is calculated by dividing the times of TF occurrence in every 2-hour sample by the total times sampled' will be added to the caption of Fig. 7.

Minor comments

Abstract 'not found in the OMI observations, likely because . . .' It should not be difficult to evaluate this effect in the body of the paper, then the abstract could make a statement.

We agree with the reviewer. 'likely' will be dropped from the sentence.

What is meant by 'a separate bias'?

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Changed to 'another difference is identified with the OMI ...'.

Does the high bias in the stratospheric column outside the tropics affect the STE estimate. (p. 14878, l 17, 590 Tg/yr)

Probably, but not by much more than 15%.

Introduction I was surprised that the 'many studies' did not include any of the STE papers by Olsen et al.; although these several of these use TOMS observations I think they are relevant.

Yes, citation to *Olsen et al.* (2003) will be added to the introduction and the difference in STE patterns with respect to *Hsu et al.* (2005) will be noted in the text.

Statement about HIRDLS 'no useful signal below 150 hPa' is an overstatement (e.g., Pan et al., JGR, 2009). The typical acronym for HIRDLS does not have a lowercase 'i'. I don't actually see the point of explaining why other data sets are not used.

The statement will be changed to 'does not have enough useful data below 150 hPa'. Although in HIRDLS documentation, O₃ useful range is 260 hPa–0.5 hPa, too many points below 150 hPa are removed after applying the "gradient filter" to screen out the unrealistic high ozone spikes. Change 'HiRDLS' to 'HIRDLS'.

P 14880 – when discussing Ziemke's product, it is good to keep in mind that his work was not attempting to consider trop folds. Ziemke's product is good for the applications for which it was used. This is part of the art to analysis of satellite data sets – to be able to understand the utility of various approaches to a dataset.

p. 14882 L. 9 Likewise, OMI does not detect the very low TCO over equatorial western and central Pacific, where the low O3 abundances are near the surface. I don't get this statement! OMI's lack of sensitivity means that OMI is not measuring anything there (essentially reporting small or zero) – so how can their

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values be too high?

OMI is insensitive to lower tropospheric O₃ (Zhang *et al.*, 2010). Instead of putting small or zeros in the lower troposphere, OMI reports the a priori values there, and thus OMI TCO is higher than that of CTM over those regions.

In the conclusions, you use frequently or frequency often and in ways that confuse your discussion.

The use of 'frequently' and 'frequency' will be revised in the final paper.

Language that should definitely be changed: 14879 I 14 'really contain only one degree of freedom'

Changed to 'contain approximately one degree of freedom for signal'.

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