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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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This paper takes a novel approach towards characterizing stratosphere/troposphere exchange using OMI data, exploiting the high horizontal resolution of OMI and using a CTM driven by pieced forecasts developed from European Center Analysis fields. The paper has interesting and new information that should be published, but also has some deficiencies that should be addressed before publication.

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 . . .'

'probably due to redistribution of pollution plumes in the 1 x 1 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).

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

'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.'

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.

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 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?

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?

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, 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 30°N is isentropic exchange near the subtropical jet.

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'.

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).

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?

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

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.

What is meant by 'a separate bias'?

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

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.

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.

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 OMIf is not measuring anything there (essentially reporting small or zero) – so how can their values be too high?

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14875, 2010.