Atmos. Chem. Phys. Discuss., 10, C5566–C5569, 2010
www.atmos-chem-phys-discuss.net/10/C5566/2010/

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 #1

Received and published: 20 July 2010

The paper presents a combined CTM-satellite based investigation of a process (tropopause folds) which contributes to the tropospheric ozone abundance. The method uses statistical relationships and compares the variability of fold occurence and ozone. The CTM data are evaluated particularly against sondes as well as ozone observations from satellite. To determine the tropospheric ozone column from the satellite observations (OMI) the authors apply the model tropopause to the satellite data. They construct an index which is based on the variability of ozone and fold occurrence in both data sets, which could be in general an original and useful way to combine both types of data sets. According to their analysis the variabilities of both data are related and can be linked to tropopause folds. The latter are solely deduced from CTM

ozone profiles applying a combination of thresholds particular for the altitude (> 5km) to indicate folds.

However the criteria and the discussion of some points is a bit short and some results might be biased by the thresholds or criteria (details below): 1) The use of a high resolution tropopause on coarse resolution satellite observation might lead to artificial variability and should be discussed 2) The biomass burning effect on ozone is mentioned, but not accounted for, 3) The discussion of the fluxes should be deeper and related to other studies

In general the paper shows a very valuable method to compare data and fluxes and clearly merits publication in ACP after the following questions have been considered.

1) What is the effect of applying a tropopause from a relatively high resolution data set to the OMI ozone profiles with low resolution in the troposphere? Doesn't this lead to an artificial ozone enhancement for the satellite data in regions of steep tropopauses or even in the whole data set (Figs 5d and 6d)? In cases of a high tropopause on top of a fold one would include relatively more stratospheric ozone from OMI due to the smooth non-linear OMI ozone vertical gradient compared to the model (compare Fig.1). This would also directly affect variability and SV (the variability index).

2) The authors are aware of a biomass burning effect on ozone as stated in the manuscript, but they do not try to disentangle it. Why did the authors not exclude biomass burning ozone by simply using a second tracer (e.g. CO) which should be available in the CTM? Those events could be marked differently in the plots and accounted for in the analyses or at least coincidence to folds could be indicated. Since the tropics and subtropics are affected by both biomass burning and folds, the authors could differentiate and calculate e.g the percentage of high TOC events, which are also affected by biomass burning (or exclude them).

3) It is surprising to see very low ozone fluxes in the winter hemispheres (Fig.7). Also the patterns of fold occurrence do partly not coincide with the fluxes (Fig.7) and the au-

thors discuss a few reasons, including summer convection in the northern hemisphere (Fig 7). Fold occurrence for deep folds in the extratropics should be strongest in winter / spring at least according to the Lagrangian studies (e.g. Sprenger, Croci-Maspoli, Wernli, 2003, SCW03 in the following).

Note, that the TF frequency largely matches the summer shallow exchange case in SCW03 (their Figure 3b), but not for winter. Note also that the Lagrangian mass fluxes in the summer NH (SCW03, Fig.7) are not too different from the June patterns of ozone flux in Tang and Prather, whereas the winter fluxes and patterns are totally different (of course mass and ozone fluxes are not the same, but should be related to some extent).

Is it possible, that the fold occurrence frequency is biased by the method of fold detection and/or the choice of the ozone thresholds? Eventually the 5km criterion is too high as lower cut-off since it systematically removes deep folds, which are of potential importance. Are there any sensitivity studies possible?

Note also the large differences of fold frequency occurrence (Fig.7) around south America compared to SCW03, which might be from biomass burning highlighting the importance to account for the latter.

Further, if convection is the reason for the continental summer ozone fluxes, could the authors give reference to measurements which clearly show ozone enhancements from convection due to associated downdraft (due to mass conservation) as suggested at the end of section 4? It is correct that upward mass flux must be balanced by downdraft somewhere, but what is the experimental evidence for locally associated tropospheric ozone enhancements from the stratosphere?

Minor points:

p.14883, l.1-5 (Figs 5d) 6d): The 2D PDF's are stated to be 'generally symmetric and on the 1:1 line'. At least for June the statement is a bit oversimplified given Fig 5d. Is there a reason for the seasonality of bias and the tendency to higher northern hemispheric

TCO OMI values in June? It seems to be driven by the northern hemisphere (see also Figs. 4a, 4c)? Is it the mismatch of OMI ozone profiles and the better resolved model TP, which drives the bias?

p.14883,l.16/17: Clearify the sentence: Is the mean of the squared difference of the residuals really the '...then the variance of the error less the means...'? Maybe something like the variance of the model residual to the observational residual.

References: Sprenger, Croci-Maspoli, and Wernli; Tropopause folds and cross-tropopause exchange: A global investigation based on ECMWF analyses for the time period March 2000 to February 2001 ; JGR, 108, D12, 8518, doi:10.1029/2002JD002587, 2003.

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