Atmos. Chem. Phys. Discuss., 12, C9228–C9231, 2012
www.atmos-chem-phys-discuss.net/12/C9228/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnosing the transition layer in the extra-tropical lowermost stratosphere using MLS O_3 and MOPITT CO analyses" *by* J. Barré et al.

Anonymous Referee #2

Received and published: 12 November 2012

In this paper, the authors diagnose the extratropical tropopause transition layer (ExTL) features such as mixing, the ExTL position, and its thickness using a CTM results constrained by MLS and MOPITT observations. The authors also present differences in analysis using only pure modeled fields, mixed modeled and analyzed fields, and combined analyzed fields. The authors state that model's results are improved by assimilating satellite observations, and a combination of two analyzed fields (O3 and CO) is better than only one analyzed field. However, the analysis is poorly presented, and I have strong concerns with the analysis technique in this study. Therefore, I think the paper needs substantial improvements before being published in ACP.

Here are my main concerns:

1. The relative altitude coordinate used to diagnose the ExTL position and thickness. The authors show the 360 K follows the ExTL in Fig 2. However, I do not think this is a generally feature. The authors want to use a coordinate referring to this level in order to avoid the complexity in double tropopause structures. However, the authors should know there is only one 360 K level, and their coordinate is fundamentally the same as the absolute altitude coordinate (referring to the surface). This coordinate really skews the analysis when there is a double tropopause structure, especially one tropopause is above and one tropopause is below the 360 K. Moreover, the authors try to diagnose positions of the ExTL relative to the thermal tropopause using their 360 K relative altitude coordinate. Obviously, discussions would be more direct if the tropopause coordination was used.

2. The correlation between O3 analysis and CO analysis shows strong mixing in the ExTL. Is this a case for a strong STE event, or is this a general feature for analyzed chemical fields? In other words, what are the effects of data assimilation for studies in UTLS region regarding to the strong STE events and regions without such strong events? The discussion is unclear.

3. The authors diagnose the ExTL using analysis chemical fields constrained by satellite observations. The advantage of that is that model results are constrained by observations. The disadvantage of that is, however, that these satellite observations have coarse vertical resolutions (even coarser than model's resolution), which would blur the UTLS features. When the analyzed data are used for UTLS studies, the balance of the advantages and disadvantage should be assessed. In addition, the paper does not provide a solid evidence (e.g., other observations) demonstrating data assimilation really "overcomes" the shortcomings associated with the coarse resolution in their model.

Here are other comments:

1. The introduction is too brief. Many important points are missing. For example, how is the ExTP thickness determined? What is the advantage and disadvantage of the method(s)? Are there any studies besides Pan et al (2007) and Hegglin et al., (2009, 2010)? How do you assess these estimates? As to data assimilation (DA), what is the status of current DA activities regarding to UTLS studies? What are the improvements in analyzed data when they are compared to pure model results in literature? A solid introduction of current studies and the scientific questions is essential for a research paper.

2. Page 22025, Line 4: I do not think I have seen people citing WMO for the dynamical (PV) tropopause definition. This needs to be double-checked. There are many papers using various PV values: Holton et al., 1995; Haynes and Shuckburgh, 2000; Highwood et al., 2000; Scott et al., 2003; Schoeberl, 2004; etc.

3. Page 22025, Line 12: The correlation method is not an ACCURATE method to locate the ExTL. It is effective to diagnose mixing in the ExTL. However, it is a really empirical process to choose the values of tracer abundances where the "L" shape correlation is truncated at the branches. In addition, different correlations (O3 vs H2O) would give you different results (Hegglin et al., 2009).

4. Page 22026: The authors says is the first study to assimilate both limb and nadir space-borne measurements. However, the authors should know that the nadir technique is good at total column abundances. It has broad average kernels when profiles are retrieved from these observations. The actual resolution is much coarser than model's vertical resolution. As a result, this statement does not provide a merit to this UTLS study.

5. Page 22032: Is there any meaning to specify the "convex" and "concave" correlations?

6. Page 22034, end of Sec. 3.2: The upper boundary of ExTL is decreased by 1 km and the lower boundary of the ExTL is reduced by 2 km by assimilation MLS and

MOPITT observations. Therefore, thickness of the ExTL is increased by about 1 km. Thickness values indicated by the stand deviation in Sec. 3.2 and Table 2, however, have little difference between O3 and CO analysis case and the modeled O3 and CO. Obviously, these analyses do not reconcile.

7. Sections 3.2, 3.3 and 4: see my concern on the relative coordinate at the beginning.

8. Page 22036, line, 15-16: do you have any quantitative criteria for this? In the Table 2, I see the standard deviations are 1.42 km and 1.43 km for model CO and O3 and for combined O3 and CO analysis, respectively. Clearly, the thickness is not narrowed. The position of is lowered by about 1.5 km in the analysis. However, how low is the ExTL that it can be close to that in the real atmosphere?

9. Page 22037, line 14: Why is the monthly-averaged model output used in this study? The ExTP features have already been smoothed in this averaged dataset.

10. Page 22038, Line 1: MOPITT CO is useful and helpful for UTLS studies, but I don't think it is "well" suited for improving model's performance in the UTLS region.

11. Page 22039, line 15: By which criteria can you say the combined analyses have the "best" ExTL representation? Evidences are needed to support this analysis as commented above.

12. Page 22040, line 6-8: this statement seems to be contradicting to the analysis in section 3.2 and Figure 4.

13. Page 22042, line 16 – 22: What's relation between the ExTL thickness and mixing in it? If the authors think the thicker of the ExTP, the stronger of the mixing in it, then the authors need explain the stronger mixing demonstrated in O3/CO analyses and little difference in ExTL thickness compared to these shown by modeled O3/CO data.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 22023, 2012.