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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.

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General comments: This work presents a set of diagnoses to characterize the effect of chemical data assimilation on the representation of the transition region between the stratosphere and troposphere in a global chemical transport model (CTM). Ozone and CO fields in a CTM, MOCAGE, and the associated data assimilation system, MOCAGE-PALM, are examined. The paper concludes that the representation of the extratropical transition layer (ExTL) is improved when the model includes assimilation of satellite data, MOPITT CO and MLS Ozone in particular. An adeauate representation of chemical gradients across the tropopause in global models is an important

issue. The work presented in this paper is both important and interesting. The discussion paper, however, has significant conceptual confusions. The analyses presented are not rigorous enough to support the conclusions. The issues and suggestions for revisions are detailed below.

Major issues: This work follows closely the method of model diagnostics described in Pan et al (2004, 2007). At several points, however, the physical concept behind these diagnostics are not correctly understood and applied by the authors. The analyses and discussions are often lost in details and several key points are made in a hand-waving fashion.

1. One major distinction between this work and the diagnostics shown in Pan et al., 2007 is that the comparisons are between model runs and observations in the latter but between the model with and without data assimilation in the former. Since the assimilation and analyses system in this case is also to be evaluated, and the satellite data involved have limited information and their own issues, the conclusion from he comparisons is not as straightforward. Physical arguments need to be invoked in each case to justify if assimilation/analyses result is better than the free running CTM. A major weakness in the paper is the lack of conclusive physical arguments in the discussions of the comparisons. In Figure 3, for example, the data points from the selected vertical cross section in the model produce a "concave" (subject to the definition) fit after assimilations of MLS ozone. What is the physical meaning of the "concave" behavior in the tracer-tracer space versus the "convex" behavior in most previous observations? The "convex" behavior can be understood by the source reservoirs involved in mixing, as explained in Plumb (2007). What are the mechanisms for producing the "CONCAVE" behavior? Is this an effect sampling?

Our reply: We now invoke physical arguments for each assimilation/free model cases. The curvature and the compactness of the tracer-tracer relationships are now discussed. The section 3.1 is now enhanced with more scientific contents. Please see revised text for details.

Additional ambiguity is in the discussion of "increase in the ExTL region" (P22032 L25). The meaning of the "region" here needs to be clarified. These discussions are connected to the tracer correlation plots. Do you imply an increase of ExTL depth in the geo-space? Or simply a wider scatter in the tracer-tracer space? If both, there need to be a rigorous connection between the two.

Our reply: The text is now clarified. We now reduce the use of the term "region" to its strict minimum. "Region" is now only used to define a geographical location in latitude, longitude space. Please see revised paper for details.

2. A significant change in the method of analysis made by the authors is to use 360K surface, instead of the tropopause, as the reference point to construct the relative altitude distribution for the ExTL. The argument used for this change is that based on the inspection of figure 2, the 360 K isentrope appears to follow better the chemical tracer variability. There are a lot of problems with this line of reasoning:

a. First, the hand-waving argument of 360 K is better is based on an inspection of a contour plot where the choice of color scale can change the perception entirely. The current color scale highlights the variability around 300 ppbv of ozone. The authors should try to see what the figure looks like when the scale is shifted to lower end, near the 100 ppbv of ozone. In any case a more quantitative analysis is required to support such choice. b. Second, the idea of reference surface is to choose a physical boundary and examine the tracer behavior relative to the physical boundary. The 360 K surface is not known as a physical boundary. What is the physical meaning of the relative coordinate profile in this case? c. This line of analysis leads directly to the problematic conclusion, largely drawn from Figure 5, where the two distributions, the ExTL points distribution and the tropopause height distribution, are compared in the 360 K relative coordinates. The better alignment of the two distributions in the 360 K relative coordinates does not support the conclusion of a better coincident (correlation) between ExTL and the Thermal tropopause.

Our reply: We now use the thermal tropopause as the reference level for relative altitude calculations. The figures and the text are consequently modified.

3. The paper is titled ". . . the transition layer in the extra-tropical lowermost stratosphere". Similar wordings appeared a number of places in the paper. This wording shows a significant confusion and contradiction, since the "transition layer" is by definition between the stratosphere and the troposphere.

Our reply: We have changed the title for: "Diagnosing the transition layer at extra-tropical latitudes using MLS O3 and MOPITT CO analyses.". Confused wording is now improved. See revised article for details.

4. The authors used the monthly average tracer fields in this analysis. The STE activities near the extratropical tropopause are largely associated with synoptic scale dynamical processes that are of a few days to a week time scale. The variability in tracer fields induced by these activities is largely smoothed out in the monthly mean fields.

Our reply: We now use for the global analysis tracer fields at a precise time (15 August 2007) and tracer fields from August average. See section 4 for details.

Not as major but still important issues: 1. The information content of MLS O3 and MOPITT CO are very different due to their very different spatial sampling. MLS is a limb sounder and the O3 retrieval represents 3 km vertical layers that are several 100 km in horizontal scale. The MOPITT is a nadir instrument that has 20 km x 20 km foot prints but 2-3 pieces of information vertically. Its information content is largely concentrated in the mid troposphere 500 hPa and not very much near the tropopause. How would these sampling differences impact the assimilated field? This issue deserves some investigation and discussion.

Our reply: This issue is now discussed in section 3.1 in the results from assimilation experiments where only one of MLS or MOPITT is assimilated. The issue also opens perspectives in the conclusions.

2. The case study for the STE event on 15 August 2007 has been presented in detail in a previous publication (El Amraoui et al., 2010). The repetitions in this paper should be limited to a minimum level necessary for introducing the follow up work.

Our reply: We now reduce the beginning of the section 3. Please text for details.

Minor and technical issues: The labels for the figures are too small and very difficult to read.

Our reply: Fixed

Reference: Plumb, R. A. (2007), Tracer interrelationships in the stratosphere, Rev. Geophys., 45, RG4005, doi:10.1029/2005RG000179

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