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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnostics of the Tropical Tropopause Layer from in-situ observations and CCM data" *by* E. Palazzi et al.

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General answer to referees/editor comments

Dear editor, dear reviewers,

authors kept in regard your comments, amendments and suggestions and thank all of you for many useful remarks, that have allowed to widen and improve the analysis presented in the paper.

We do hope that the manuscript is now more readable and fluent, and the conclusions less generic and more quantitative, than in its previous version.

Since the requests to sharpen the manuscript and extend the analysis were in common

among reviews, we include this general answer within each reply, to summarize the major changes done on the manuscript.

The paper presents additional analyses, consisting in the discussion of the vertical temperature and static stability (N2) profiles, the relative vertical CO gradients, and an extension of the H2O-O3 joint PDFs to the whole observational database. The vertical profile of N2, and the relative vertical CO gradients, in addition to the ozone gradient, allow to calculate the top and bottom bounds of the tropical transition layer and provide a precise metric to accurately and quantitatively compare the model and the measurements. All new analyses have allowed to improve the evaluation of the model capabilities in the TTL.

The most important differences among the campaigns arisen from the tracer analyses have also been deepened, though they were not among the principal objectives of the paper. This bring to a deeper evaluation of the model capability in reproducing the TTL structure and its thickness, and also allows to better analyse the factors leading to the model-measurements discrepancies. Additional figures on that point have been included in the specific answers to referees.

In order to highlight the objectives and the results in the text, the abstract has been changed to describe the main findings of the paper; the introduction has been substantially modified presenting the results of previous aircraft, satellite and model studies carried out in the UT/LS region, and clearly stating the aims of the work. The section "Methodology" has been re-structured to better describe the diagnostics used (Tropopause coordinates, vertical tracer gradients, and tracer-tracer correlations) and how data have been handled to perform the model-measurement comparison. One table (Table 3) is added to resume the observed and simulated values of the TTL thickness; Figures 4 and 5 (now Figures 6 and 4) have been modified to show, respectively, the vertical temperature profile, and the relative vertical CO gradient and N2 vertical profile. PDFs analysis has been improved taking into account the vertical distribution of the number of observations, and extended to all the measurement campaigns. The

conclusions have been rewritten to summarize our findings and the new quantitative results.

Answers to referee #1

This paper compares the E39C-MESSy model with high altitude Aircraft observations from tropical campaigns with the Geophysica aircraft to examine the structure of the Tropical Tropopause Layer (TTL). Diagnostics recently developed for the extratropical Upper Troposphere and Lower Stratosphere (UTLS) are applied. The application of the new diagnostics is useful and interesting. However conclusions are thin and not quantitative. It is hard to discern if the model is 'good' or 'bad' in any objective sense from this analysis. This paper may be suitable for publication in ACP subject to major revisions. The major issues that need to be addressed include:

1. Methodology: in particular, how is averaging conducted? It looks like everything is averaged and then tropopause relative coordinates are applied. Is that valid? On the detailed scales you are looking at below the tropopause (10-40hPa, maybe 1-2km) local deviations might matter.

As mentioned above, the methodology section has been extended and (hopefully) clarified. In particular, averaging is not done in pressure bin but in tropopause relative pressure bin. The average tropopause pressure is calculated interdependently for model and observation data. If we understand well the second point, we agree with the reviewer. The local effects (transport, convection) have been analyzed in more detail and discrepancies between model and observations, due to the role of meridional transport, are identified based on different diagnostics, and discussed with reference to the meteorological context of the measurement campaigns.

2. More quantitative results: is the model good or bad? Can it reproduce the observations successfully or not? In many cases, the model variability range lies outside of the range of observations. The ozone gradients in Figure 4 seem out of range of observations. Can you make a quantitative metric using the observations (e.g.: depth of

TTL based on ozone gradient) and report model values and variance compared to observations in a quantitative way? Some more quantitative metrics of the pdfs in figures 7-9 are also necessary.

This is a good suggestion and has been raised by other reviewers. So, most of the changes and improvements are done to have more quantitative and clear conclusions. The PDFs of the correlation between H2O and O3 have been extended to the whole database and re-calculated taking into account the observational sampling (the vertical distribution of the number of measurements). The analysis of N2 and T profiles has been included. Table 3, in particular, presents the values of the top and bottom bounds of the transition layer inferred from the relative vertical O3 and CO profiles and the vertical profile of the static stability, for the model and the observations. This allows to estimate the thickness of the TTL from model and observational data with different methods. Moreover, as mentioned above, the differences between model and observations are more deeply discussed.

3. In addition to making the paper more quantitative, the conclusions need to be made sharper. In its current form, starting from the abstract on, there is really no definitive statement made about the model fidelity, quality or balance of processes going on. Again, focusing on ozone: why are the observations and model different? What does that mean. Why is ozone off for APE-THESEO, but N2O off for TROCCINOX? This paper needs more analysis.

We consider that the answer to this pertinent question(s) is included in the previous points. We invite the reviewer to check the additional figures included as supplement in the answers to referee #2 and referee #3, that support the discussion reported in the revised manuscript. Again, the new analysis of temperature profiles (subsection 4.2) and static stability profiles (subsection 4.4) helps to strengthen the conclusions on the TTL thickness.

4. There are numerous grammar mistakes that should be corrected in a revision as

well (mostly minor)

Grammar mistakes have been corrected throughout the manuscript

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11659, 2009.