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9, C5524–C5532, 2009

Interactive Comment

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

ACPD

9, C5524–C5532, 2009

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

Answers to referee #2

Using four tropical European aircraft campaign data and ECHAM5/MESSy model data, this paper investigates some tracer profiles and tracer-tracer correlations in the tropical tropopause layer (TTL). Tracer profile details, together with simultaneous temperature profiles, may provide information on the average structure and transport/mixing processes in the region, and production/loss processes that are specific to each tracer. Tracer-tracer correlations may also provide information on mixing processes and other processes with a different, sometimes clearer view. Applying the same analysis methods to chemistry climate models can be one of the ways for evaluating dynamical, physical and chemical processes in the models.

The aircraft data presented here are relatively new and cover various regions of the tropics. Therefore, I believe the paper (if revised appropriately) will be valuable to the community.

However, I have some major concerns/comments, which I would like the authors to consider.

Major comments.

1. The purpose of the paper, and thus the new findings and their implications are not very clearly written. What is newly known about the TTL in this paper? Abstract misses the findings and implications. Introduction misses (1) statements about what will be uncovered with the tracer data analysis, and (2) results from previous measurements. For the latter, I think there were some important aircraft campaigns in the 1990s such as STEP (JGR 1993), CEPEX, etc. and several satellite data analyses. Brief explanation of these results would be necessary to highlight the results from the four European aircraft campaigns. The same question ("what are the questions and the answers?")

ACPD

9, C5524–C5532, 2009

remains through the rest of the paper. For example, in Section 6, Conclusions, the authors finally mention "TTL thickness." (This is probably one of the key "questions.") However, it seems the "TTL thickness" is not explicitly discussed in Sections 3-5.

The abstract, introduction and conclusions have been rewritten, following the suggestions/requests reported above. References to previous studies based on aircraft and satellite measurements in the UT/LS have been cited and briefly discussed in the Introduction (they are: Russell et al., 1993; Weinstock et al., 1995; Froyd et al., 2009; Hegglin et al., 2008). A more quantitative analysis is provided in Sections 3-5. In particular, the key question “TTL thickness” has been addressed: Table 3 presents the values of the TTL top and bottom bounds, inferred from the relative vertical CO and O3 gradients and the vertical profile of the static stability (N2). All these quantities are plotted in Figure 6 (Figure 4, before), for the different campaigns and the model.

2. Temperature profiles for the four campaigns should also be presented. Model outputs as well as aircraft measurements should be shown. Profiles of lapse rate and buoyancy frequency also provide important information on the thermodynamical structure of the TTL. The authors analyze the tracer data with respect to the "tropopause coordinate" so that the background temperature profiles and their differences among the datasets (if any) are what should be shown first. Note also that temperature values are the key for water vapor distribution. If there is a bias in the model temperatures, we need this bias information in the interpretation of water vapor data.

Very pertinent suggestion. The modelled and observed mean temperature profiles have been added in Figure 5 (now Figure 4) in addition to the vertical profiles of H2O. As explained in the text (section 4.2), the temperature model-measurement agreement is not satisfactory in the case of TROCCINOX, with higher temperature values (from 20 hPa below the tropopause upward) in the observations than in the model. This is attributable to mid-latitude intrusions, as also corroborated by the low N2O layers found at corresponding altitudes and enhanced water vapour concentrations. Mid-latitudes intrusions during TROCCINOX are also dealt with in the cited study by Konopka et

ACPD

9, C5524–C5532, 2009

Interactive Comment

al., 2007. Attached to this answer Fig.1 (described but not shown in the manuscript to keep a reasonable number of figures) showing, with the same color code, the model PV contours in the TROCCINOX area and the PV values calculated on the observations from ECMWF analyses: note that the latter cover a wide range of PVs, representative of non-tropical latitudes. The vertical profiles of the static stability have also been presented in the revised manuscript (subsection 4.4), and shown in Figure 6. They are used, together with the relative vertical O3 and CO gradients, to calculate the transition layer thickness. This help to strengthen our TTL estimate. An interesting point that is mentioned but would require more analysis (in a future work ...) is the coherency between the N2 and tracer gradients profiles that has been also mentioned (briefly) by Fueglistaler et al., Rev. Geophys. 2009 (now discussed in the paper).

Some (other) specific comments.

p. 11665, Table 1 : What is "H2O (total)"? Does it mean water vapor plus cloud ice water? If so, the uncertainty profile information when this data is viewed as pure water vapor should be given. In other words, what is the contribution of cloud ice water as a function of altitude? Negligible, 10%, or can be 100% or much more? How about the "H2O" from the model? Is it also H2O-total?

H2O total accounts for the H2O gas plus condensed phase. The error on the FISH instrument lies within 10% range (see reference in Table 1). Nevertheless, the accuracy of water vapour measurements in the UT-LS is still an open (and debated) issue (see for instance the results from recent AIDA laboratory campaign). Model supplies total H2O as well.

p. 11670, QBO : Why did the authors choose 40 hPa to specify the QBO phase? 40 hPa may be too far from the TTL. QBO has an effect of vertical displacement in the lower stratosphere at, e.g., 10N-10S, but it should be noted that the latitudinal dependence of the role/phase of the QBO can be very large particularly in the subtropical region. How are the measured tracer profiles affected by the QBO phase? There is no discussion on the role of QBO in ozone and water distributions.

40 hPa (22.5 km) is one of the levels commonly used to look at the QBO phase, at least in the NH. As also highlighted by referee #3, the QBO has been used as a criterion to “classify” data and perform the model-measurements comparison. This has been explained in the methodology section (subsection “data handling”). 40 hPa is the pressure level that has been used to identify the QBO phase during the Geophysica aircraft campaigns and the model data. So, we assume that the observations and the model are in the same QBO-phase. On the other hand, to the authors point of view, discussion about the role of the QBO in the distribution of ozone and water vapour in the UT/LS region is out of the purposes of this paper.

p. 11673, lines 21-24 : The tape recorder effect (Mote et al., JGR, 1996) should be the primary factor for the water vapor vertical gradient in the tropical lower stratosphere.

The role of the tape recorder in the water vapour vertical distribution and propagation from the tropopause upward has been discussed in the paper (introduction, section 2.2 and 4.2). In the sentence at p. 11673, lines 21-24, reasons leading to enhanced water vapour concentrations above the minimum at tropopause levels are reported. The role of tape recorder is an underlying issue since it determines the average structure of H2O vertical profile in the Lower Stratosphere for different campaigns and, since the model reproduces well the tape recorder phase and intensity (see Fig. 2), the agreement can be satisfactory.

p. 11675 and Fig. 6 : The vertical gradients of N2O in the lower stratosphere differs in different campaigns. Is this caused by difference in season or in latitude of the campaigns? It seems from their way of writing that the authors may consider that the model (not the observation) is the reference. Why? Or, if it is not the authors’ intention,then the careful re-writing (that the model is being validated with the observations) is necessary throughout the paper.

The difference among the campaigns between the observed vertical tracer profiles is not necessarily the main objective of this paper. However, to put the conclusions in a coherent frame, authors give an explanation of the inter-campaigns differences between the N2O vertical profiles. This is the case of APE-THESEO and TROCCINOX: the former campaign was performed well inside the tropical pipe, the latter one at the edge of the tropical pipe and for that it was more influenced by mid-latitudes than APE-THESEO. Concerning the model-measurement approach, the fact that the observed profiles of nitrous oxide have been scaled with a factor for each campaign to take into account the fact that this model run does not include the actual N2O tropospheric growth rates (reported in the text), could seem misleading. But, to the authors point of view, the model-measurement comparison results in this way more meaningful, since the aim is the evaluation of how the model, compared to the observations, reproduces through the vertical N2O profile, the transition between the troposphere and the stratosphere. Moreover, the additional results on model evaluation should, to our point of view, stress the fact that the model is not considered as the reference.

p. 11677, line 4, and Fig. 7 : Why does the "empirical" tropospheric O3-CO relationship become like what is shown? What is the data source for this "empirical" line? For example, upper tropospheric CO levels would substantially differ in different biomass burning activity below.

Empirical lines for the correlation CO-O3 are taken from a toolbox for tracer correlations analysis provided by Laura Pan (http://acd.ucar.edu/∼liwen/ExUTLS_cookbook.htm). These have been applied extensively to mid-latitude regions; their use in tropical regions could therefore be questionable. But, in this frame, empirical lines are just used to roughly separate the tropospheric and stratospheric regimes (and they are in good agreement with observations).

p. 11677, line around 20 : The difference between the measurement results and model results may have arisen from inappropriate surface emission of CO in the model.

CO emissions due to e.g., biomass burning activity, have not been discussed in the

ACPD

9, C5524–C5532, 2009

paper. However it is true, inappropriate surface emission of CO in the model could account for differences between modelled and observed CO-O3. Nevertheless, the use of relative CO gradients, now presented in the revised manuscript (Figure 6), helps to circumvent this issue.

p. 11678, line 1 : What do you mean by "small"? Smaller than what? Also, what is the approximate altitude/pressure level for 1000 ppbv of ozone and for 4 of log_e (H2O)? The altitude region shown in the scatter plots might be too wide to investigate the TTL closely.

Authors refer here to a discussion published in Hegglin et al., JGR 2009 (reference added in the manuscript), where “small” is intended with respect to the extra-tropical mixing. The sentence has also been re-phrased.

p. 11679, line 8 (and subsequent lines) and Fig. 9 : How is the sampling bias treated? If there is a sampling bias, then the maxima in the joint PDFs may just indicate the sampling bias. For example, the hydrated layers above the tropopause could be captured only during the SCOUT-Darwin campaign because of a bias in flight track.

We consider this an important point. For that, the PDFs have been re-calculated keeping into account the observational sampling, by weighting them with the number of observations within each pressure bin. Therefore, Figure 9 is modified (also in that it shows the PDFs for all campaigns rather than for SCOUT-Darwin only, as suggested by referee # 3).

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ACPD

9, C5524–C5532, 2009

Interactive Comment

ACPD
9, C5524–C5532, 2009

Interactive Comment

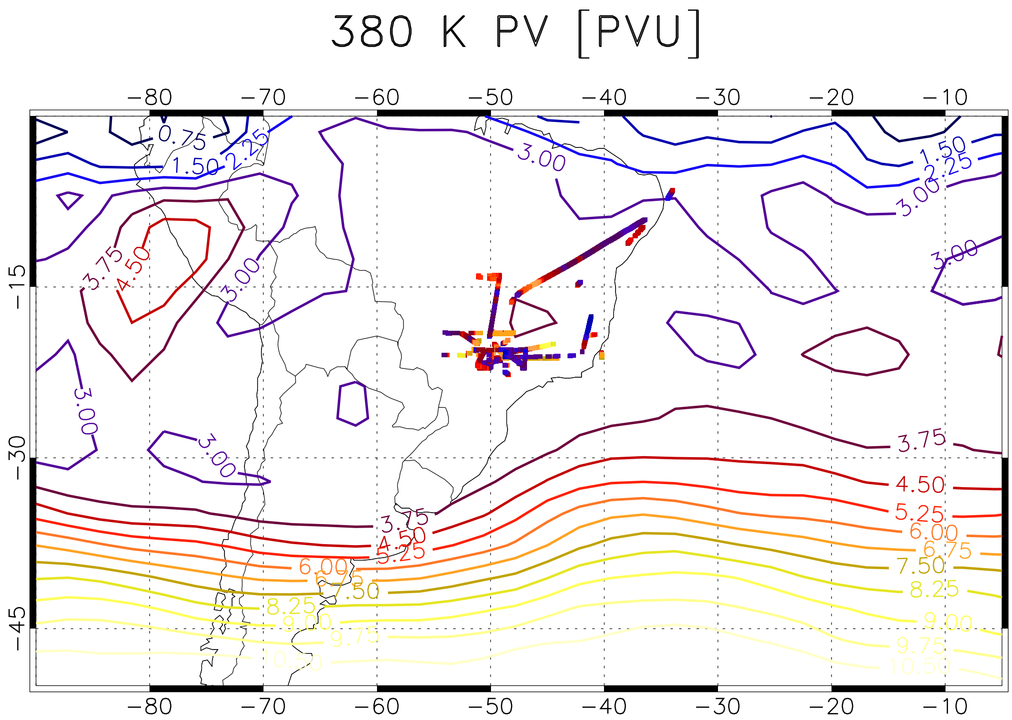

Fig. 1. ECHAM5/Messy PV contours in the TROCCINOX area and the PV values calculated on the observations from ECMWF analyses

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