

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

Anonymous Referee #2

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Review of Diagnostics of the Tropical Tropopause Layer from in-situ observations and CCM data by Palazzi et al.

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 pro-

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cesses 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?") 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.

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.

Some (other) specific comments.

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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?

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.

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.

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.

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.

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.

p. 11678, line 1 : What do you mean by "small"? Smaller than what? Also, what is the

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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.

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.

(the end of the review report)

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