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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the structure of the extra-tropical transition layer from in-situ observations" *by* I. Pisso et al.

Anonymous Referee #1

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This paper presents an analysis of the extra-tropical transition layer by using in-situ measurements of O_3 and CO of a large set of airborne measurement campaigns from the tropics toward the extratropics. By using the equivalent latitude and potential temperature coordinate system, mixing ratios and their respective vertical gradients are presented to discuss the structure of the extra-tropical transition layer in the seasonal mean.

This is an interesting study which is clearly relevant to ACP. The data used and the techniques are well described. Nevertheless, at the moment I am not entirely convinced by the discussion and interpretation of the high CO gradient as upper limit of the extra-tropical transition layer that increases in height toward the poles in potential

temperature coordinates relative to the dynamical tropopause. I therefore recommend publication with consideration of the comments below.

Major comments:

1. *Sloping isopleths of CO gradient*
   One result of the authors is that their analysis would provide a working definition of the upper limit of the ExTL based on the 'upper limit of the region of high vertical CO gradient' sloping towards the poles relative to the dynamical tropopause. The authors conclude that the 'maximum gradient in CO' can be used as a definition of the upper limit of the ExTL (page 28051, line 5). I have the following comments on the sloping isopleths of CO gradient:

   a) Is the top of the ExTL defined by the maximum of the CO gradient or by the upper limit of the region of high CO gradient? Both terms are simultaneously used in the study.

   b) Recent papers show that the vertical CO gradient reveals a pronounced maximum at the thermal tropopause (e.g., Pan et al., 2004, Hegglin et al., 2009). In principle, the mixing ratio of CO in equivalent latitude and potential temperature coordinates of your plot (e.g., Fig. 4a, DJF) does indicate that, too. Based on this and on your comments that sloping isopleths are displayed in the temperature field (page 28051, lines 18ff) and in the amount of trajectories in the UTLS visiting the boundary layer 30 days ago (Berthet et al., 2007), I'm wondering if the enhancing isopleths still remain or if the degree of sloping would be reduced when taking the thermal tropopause as a reference and not an individual value of PV?
   When comparing Fig. 4 panels g and h (or Fig. 5 panels c and d) with each other, I have the feeling that maximum trace gas gradients clearly represent the location of the thermal tropopause (black line). For a clarification of that issue,

you might illustrate the location of the thermal tropopause relative to the 2 PVU dynamical tropopause in Fig. 4h (or Fig 5d), too, or show a similar plot with distance in altitude with respect to the thermal tropopause. This would further indicate the rather tropospheric branch (based on the thermal tropopause) above the 2 PVU iso–surface.
Wouldn't the fact that the Lagrangian based tropopause (Berthet et al. 2007) also reveals sloping isopleths with respect to the 2 PVU iso–surface (see also Fig. 7) imply that the sloping isopleths rather represent a barrier in the UTLS, in particular the tropopause, than an upper limit of the ExTL?

c) There is a dependence of the sloping isopleths of trace gas mixing ratios on the values used for the dynamical tropopause, as you tested for values between 2 to 3.5 PVU (page 28050, lines 12ff). In general, the distribution of tracers would remain qualitatively unchanged. According to Kunz et al., 2011a, I'm also wondering if using the PV-gradient, or at least seasonal and isentrope dependent PV values, would be better suited for the estimation of the degree of sloping isopleths. See also Kunz et al., 2012b, who analyzed the chemical and dynamical discontinuity at the extratropical tropopause and therefore compared the CO gradient with equivalent latitude relative to the strongest PV gradients on middle world isentropes.

2. *Tropopause relative coordinates*
When comparing CO mixing ratio between DJF and JJA, i.e., Figs. 4a and 5a, the vertical gradient in CO better fits with the thermal tropopause in DJF than in JJA. This may well be due to the fact that the tropopause acts as a barrier for cross-tropopause transport in winter, and in summer, when the barrier weakens, the gradient is on higher altitudes than the thermal tropopause indicating a flavored exchange process.
I therefore recommend, that the tropopause based presentation of the mixing

ratios and their gradients, does not necessarily allow different seasons to be analyzed together for climatological means (see your comment in the abstract, lines 17ff). I agree, a tropopause based coordinate system clearly reduces trace gas variability due to the tropopause height fluctuation. Nevertheless, variability of trace gases due to seasonal dependent dynamical processes such as convection and exchange of air masses across the tropopause will still remain. I therefore suggest a separate presentation of results for different seasons, and not together as shown in Fig. 6. You may at least comment on that issue.

3. *Accuracy and precision of the data*
Section 2.1 should include some more information on the measurement techniques used for the different campaigns. Is the measurement instrumentation of the two trace gases and the respective uncertainties the same for all campaigns? Different measurement systems of trace gases may well have an influence on the combined analysis of these trace gases. I therefore don't understand the sentence that the accuracy and precision of the data may change, but the analysis of tracer gradients rather than absolute values enhances the consistency between different campaigns (page 28039, lines 24-26). Qualitatively, this may be right, but a quantitative analysis of trace gas gradients should be influenced by varying accuracies within the used data set of different campaigns.

4. *Calculation of vertical trace gas gradients*
a) Can you please comment on the calculation of vertical trace gas gradients in more detail (Page 28044, lines 12-16)? Am I right, that the in-situ data are first zonally averaged (in geographical latitudes?) and then they are binned in equivalent latitude bins of 10 degrees. Afterward, you calculate the vertical gradients of trace gases in 2K potential temperature bins with respect to equivalent

latitudes? I don't understand why a zonal average of the data is performed at the beginning. This should imply a further smoothing of data around the tropopause. A direct separation of the measurement data into equivalent latitude bins would be more intuitive.

b) Please take a look again on the calculation of the vertical gradient of ozone. The distribution of ozone mixing ratio in Fig. 5e clearly indicates a maximum in ozone gradient at the northern hemisphere thermal tropopause. When comparing with Fig. 5g the maximum gradient in ozone is much deeper in the stratosphere which doesn't fit with the mixing ratio distribution of Fig. 5e. The same for the respective panels of Fig. 6. But this may also be influenced by your method to calculate the trace gas gradients (see point 4a).

c) You may think about a calculation of gradients based on the logarithm of trace gases. By not using the logarithm of the trace gases large–scale stratospheric processes seasonally influencing the trace gas distribution of ozone in the stratosphere (e.g., downwelling due to the residual circulation) is more emphasized. By using the logarithm the focus is rather on the tropopause region and seasonal variation of ozone in the stratosphere is largely excluded with help of a scale factor, the inverse of a mixing ratio which is implicitly included in the gradient of the logarithmic trace gases (see Kunz et al. 2011b for this issue). Therefore, especially Figs. 4 and 5, panels g and h respectively, may look different when using the logarithm of trace gases.

Minor comments:

1. *Page 28035, Lines 13-15:* Maybe you want to cite some related work on the importance of the tropopause region on the radiative forcing and climate change, e.g., Solomon et al., 2010, and Forster and Shine, 1997.

2. *Page 28040, Line 7:* You state that the difference between potential vorticity surfaces from 1.6 to 4 PVU would be 'modest'. What kind of difference do you mean? The difference in altitude?
3. *Page 28040, Lines 23-24:* Is Kunz et al 2011 really the right citation at this place?
4. *Page 28041, Line 7:* Citation of Pan et al. 2011 is missed in the references. Shouldn't it be Pan et al. 2012?
5. *Page 28041, Line 14:* Please give some more information on the ECMWF analysis fields you are using?
6. *Page 28042, Line 8:* I don't understand what you mean with 'actual lateral boundaries' in the context here.
7. *Page 28042, Line 20:* Data density *near the tropopause* ...
8. *Page 28043, Line 14:* You may compare with Kunz et al. 2011b, who calculated meridional ozone gradients for the same coordinates based on START08 data.
9. *Page 28044, Lines 6ff:* The symmetry in CO distribution around the dynamical tropopause in the northern and southern hemisphere strongly depends on the value used for the dynamical tropopause. The thermal tropopause is represented by a different PV background in the southern summer hemisphere than in the northern winter hemisphere. An according use of PV values for the dynamical tropopause may therefore not show the symmetry in CO distribution in the two hemispheres as you showed in Fig. 4b in comparison with Fig. 4a.
10. *Page 28049, Lines 4–6:* I do think a local calculation of the thermal tropopause for each measurement point would be interesting and relevant for this study. Thermal tropopause can be calculated based on ECMWF data and interpolated to the measurement location of the field campaigns.

11. *Fig. 1:* Why are there some single red points outside the regions where the different campaigns did take place (e.g, in the southern hemisphere near Antarctica or above Russia)?

12. *Fig. 2:* The total analysis is presented in coordinates relative to the dynamical tropopause based on potential vorticity. How did you calculate the distance to this tropopause on lower latitudes, especially $\pm 5^{\circ}$ around the equator? Based on the definition of potential vorticity its iso–surfaces are not represented at the equator.

13. *Figs. 3–6:* How is the location of the thermal tropopause calculated? Is that a zonal mean over ECMWF data based on single flights days? It would be further nice and helpful having a line for the location of the dynamical tropopause, i.e., the zero line, on each of these plots.

14. *Fig. 7:* Please explain in the caption what the white dotted lines in the right panels represent.

Editoral remarks:

1. *Page 28036, Line 21:* 'Hemisphere'
2. *Page 28037, Line 3:* 'taken over time'
3. *Page 28043, Line 12:* skip 'a' in front of 'there'
4. *Page 28043, Line 23:* from sea level
5. *Page 28045, Line 16:* two times 'of'
6. *Page 28045, Line 26:* CO isopleths seem to slope with respect to

7. *Page 28046, Line 3:* with respect to
8. *Page 28047, Line 21:* skip 'of' in front of 'in'

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