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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Variability in upwelling across the tropical tropopause and correlations with tracers in the lower stratosphere" *by* M. Abalos et al.

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We thank the Anonymous Referee #3 for the insightful reading of the paper and the helpful comments.

As a general comment, we note that the comparison between our results and those in the works of KP (following the notation of the referee) will be further addressed in an extended version of section 4 (Summary and discussion) in the revised manuscript. We answer below to the specific comments and the technical corrections.

Specific Comments:

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1. Vertical upwelling vs. horizontal transport as ‘forcings’ of variability.

“[. . .] However, I don’t think that there has to be necessarily a conflict between RA and KP, as the frameworks in which the different results are deduced are very different. [. . .]”

We agree with the referee that there is a possibility that the results may not be actually in contrast, and the apparent discrepancy arises from the different perspectives (TEM versus Lagrangian views). In particular, in the Lagrangian analysis the ozone concentration at a given location is obtained by integration along a cross-isentropic ascending pathway of two terms: chemical production and in-mixing (Ploeger et al., 2012). The rate of in-mixing is obtained from 3-dimensional back trajectories, and shows a seasonal cycle peaking in boreal summer which, combined with the large ozone meridional gradients, results in a large increase in tropical ozone near the tropical tropopause during this season. Both terms contribute to increasing ozone over time in the ascending air mass, such that lower ascent rates during boreal summer lead to larger ozone accumulation. Although longer ascent rates could by themselves explain higher ozone concentrations during northern summer even with constant in-mixing rates, Ploeger et al. (2012) show that this effect is not sufficient to explain the observed ozone seasonality in their calculations.

In contrast with the Lagrangian view, which integrates the ozone concentration over time (and hence over theta levels), the TEM methodology looks at a fixed location and compares the amount of ozone coming from below to that coming laterally exclusively at that level. One potential explanation for the different results between methodologies is that the air being in-mixed at the lower levels is being subsequently transported upward and therefore, the same air considered vertically advected by upwelling in the TEM view could be in-mixed air in the Lagrangian view, as it was previously laterally in-mixed at a lower level. However, this effect should be quantified to prove that this is the ultimate cause for the different results. Hence, we consider necessary to clearly manifest in the present work the fact that there is a discrepancy between the conclusions of RA and KP regarding the origin of the ozone seasonality above the tropical tropopause (whether it is actual or apparent should be determined in further studies). Nevertheless, we think this option makes an important contribution to the discussion, and we will certainly include it in the reviewed version of the manuscript.

"[...] But I would encourage the authors to be more careful with their wording throughout the paper (e.g., instead of p18829/L13 ...highlights tropical upwelling as a primary forcing term... the authors could write ...as a primary forcing term in the TEM tracer continuity equation..., similar parts are marked in the 'technical corrections' part)."

We agree with the referee that it would be good to emphasize that our results show the relative importance of the terms within the TEM framework, given the possibility that this could be different under other approach such as the Lagrangian view. Instead of including such observation in sentences P18833, L16 and P18836, L4, we have dedicated a sentence in the discussion to highlight this point, as we consider this more effective to avoid the misinterpretation of the results, and changing the cited sentences would make them less clear and straight-forward.

2. Pressure vs. potential temperature coordinates

We have included a paragraph in the discussion regarding the differences between using log-pressure versus potential temperature surfaces as vertical coordinates. Indeed, the seasonality of the terms could be modified respect to that in Fig. 6 if the analysis would be done on isentropic coordinates, and it is certainly interesting to study the different behavior on this alternative coordinate system, but we argue in the new extended version of the discussion that the use of log-pressure coordinates is most adequate for the present analysis.

Technical corrections:

"P18820, L9: There is also some controversy about the role of ozone for amplifying the tropical temperature cycle (see, Fueglistaler et al, 2011)."

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Fueglistaler et al. (2011) point out that care should be taken when interpreting the results regarding the influence of the seasonality in the equilibrium temperature (Teq) on the annual cycle in temperature, given that both w* and T respond changes in TE and hence only part of the change in Teq will be reflected in T because it is partially compensated by dynamical heating. In the present manuscript we briefly mention one of their main results which we consider particularly relevant, i.e. the fact that including ozone-induced variations in the equilibrium temperature affects the seasonality of temperature in the tropical lower stratosphere, and we leave further details to be consulted in the cited reference.

"P18820, L18: Wording: ...in contrast with..., see specific comment."

We refer to our answer to the specific comment 1.

"P18820, L18: ...(CLaMS) and on back trajectory calculations,..."

We have modified the sentence in this way: "... based on back trajectory calculations using the Chemistry Lagrangian Transport Model of the Stratosphere (CLaMS), ..."

"P18822, L1: Perhaps better: ...ERA-Interim yields less noisy vertical velocities...?"

We agree and we have changed it.

"P18824, L7: Why is the neglection of the eddy transport term a good approximation?"

The calculation of the TEM vertical velocity from the thermodynamic balance equation considers quasi-geostrophic scaling and hence the eddy term can be neglected (Rosenlof 1995, and references therein). Although this is in general a small term, we find here that it is not completely negligible in this region, particularly at 80 hPa in ERA-Interim results (see Fig. 5). Therefore we explain in the manuscript that the estimate obtained by this method likely overestimates the mean and the amplitude of the seasonal cycle at this level. However we note here, as well as in the manuscript, that this statement is based on the vertical eddy heat flux computed from the ERA-Interim reanalysis, which is not necessarily reliable. In any case the overall variability of this

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estimate is in good agreement with the other two estimates also at 80 hPa (linear correlation of 0.76 with wm*).

"P18824, L15: ERA-Interim temperatures appear to be cold biased around the tropical tropopause. This could bias the vertical velocity calculation."

As shown in Liu et al. (2010), the temperatures from ERA-Interim near the tropical tropopause exhibited a cold bias of ∼0.2 K before 2007, which was eliminated with the introduction of COSMIC GPS data in the assimilation process. In addition, there is a seasonally dependent bias with zero annual mean that enhances the seasonal cycle of ERA-Interim temperatures as compared to radiosonde measurements over the period under study. However, the small effect on the inter-annual variability is not relevant for this work and the error in the seasonal cycle in temperature represents less than 10% of the amplitude. Hence the corresponding effect on wQ* should be also small.

"P18825, L13: How large is the difference in vertical upwelling velocity to the case without adjustment?"

If the adjustment is not made, the results get very unrealistic over the iterations. It is probably more meaningful to mention that the results are fairly similar to the initial guess, which corresponds to considering zero meridional velocity.

"P18826, L18: Wording: ...results primarily from forcing by vert. upwelling..., see my specific comment."

We refer to our answer to the specific comment. Note that just in the next sentence we explicitly indicate that our analysis is based on the TEM framework.

"P18829, L7: Avallone and Prather (1996) and Konopka et al. (2010) considered the combined effect of advective transport and irreversible mixing into the tropics. In order to compare Fig. 6 to their results it would be more appropriate to draw an additional line showing the sum of advective and eddy transport (residual plus vâĹŮ∂yχ). Furthermore, even if there is no seasonality of this term on pressure levels there could be

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a distinct seasonality on potential temperature levels (specific comments)."

We agree. However, as highlighted by the referee in the specific comment 1, Fig. 6 is not directly comparable to the results in KP in many other ways (e.g. Lagrangian vs. Eulerian view, potential temperature versus log-pressure vertical coordinate). Therefore, we consider that adding another line in this figure could be misleading. Regarding the vertical coordinate we refer to our answer to specific comment 2.

"P18833, L4: Perhaps the wrong Fig.-reference here? Fig. 7 shows CO."

This is right; we thank the referee for detecting this error.

"P18833, L16: Wording: ...dominant forcing mechanism ..., see specific comment."

Please refer to our answer to the specific comment.

"P18835, L20: Strictly speaking, Konopka et al. (2009,2010) and Ploeger et al. (2012) show that horizontal transport (in-mixing) is necessary to explain the annual cycle of tropical ozone, with about 50% of the summer ozone mixing ratio originating in mid-latitudes. On the one hand, there is an annual cycle in in-mixing rates which would cause an annual ozone cycle even in the absence of seasonality in upwelling. On the other hand, for time constant in-mixing rates the amount of in-mixed ozone would also show an annual cycle if upwelling is seasonally varying, because more mid-latitude ozone would be in-mixed into the slower ascending tropical air masses during summer. Neither Konopka et al. (2009,2010) nor Ploeger et al. (2012) separated these two effects. Their results show that upwelling alone in combination with photochemistry is not able to reproduce the observed ozone cycle. So be careful with the wording here."

We share the view of the referee on this subject, but in the cited works it is argued that "[. . .] the ozone annual anomaly in the upper TTL turns out to be mainly forced by in-mixing of ozone-rich extratropical air." (Ploeger et al., 2012). Hence, we believe that the sentence in the manuscript reflects the conclusions in the cited works: "Their calculations [. . .] suggest that the seasonal cycle of ozone in the tropical lower strato-

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sphere is primarily a response to horizontal transport (in-mixing), rather than upwelling." Note that we have changed "horizontal eddy transport" with "horizontal transport" to be consistent with the fact that eddy mixing is not separated from advection in their calculations (and they refer to the total horizontal transport as in-mixing).

"P18836, L4: Wording: ...central role of tropical upwelling..., see specific comment."

Please refer to our answer to the specific comment."

"P18839, L2: ... Geophys. Res. Lett. ..."

It will be changed in the reviewed version of the manuscript.

"P18839, L11: ... Grooß, J.-U., ..."

We have changed it.

"Fig. 4 caption: ... as a function of pressure."

We have changed it.

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