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Atmospheric Chemistry and Physics Discussions

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 #4 for the careful review of the manuscript and constructive comments.

Specific comments:

"Page 18819, line 19: Around here previous studies on the annual variation of ozone are described. In relation to these studies one character which is in my mind is the latitudinal swing of the total ozone minima in the equatorial latitude, which is easily seen in a time-latitude section of the zonal mean total ozone (e.g. Bowman & Krueger, 1985, JGR). I understand the authors' standpoint of view averaging over the equatorial

latitude in this study, but the authors should be aware of such a seasonal variation of ozone; this could be closely related to a semi-annual component the authors observe in Figs. 5- 7."

Latitude-time sections of MLS observations also show a latitudinal shift in the ozone minima towards the winter hemisphere. There is also a slight asymmetry of the ozone seasonality between both hemispheres, as shown also in Krueger and Bowman (1985) for total ozone. We are aware that by averaging over a fixed range of latitudes we are unable to discern these interesting spatial features. However, for us there is not an obvious link to the latitudinal shift to a semi-annual component in Fig.6.

"Page 18821, Subsection 2.1: The dataset used here for tracers is limited to ozone and CO. Is there any possibility to additionally use water vapor as it is a tracer in the lower stratosphere? I suppose that the interpretation would be rather difficult for water vapor around the tropopause, but you may mention about this point."

The interpretation of water vapor transport across the tropical tropopause is indeed more complicated, and it deserves separate studies. Water vapor transport seems to be tied to coldest temperatures, rather than being directly driven by circulation. We will mention this in Section 2.1 (line 17 page 18821).

"Page 18824, line 13: I suppose the heating rate calculation may be sensitive to ozone amounts around this height range. The authors mentioned that they used MLS ozone, but how about the uncertainty in MLS ozone, and how about the quantitative effect from the uncertainty?"

Fueglistaler et al. (2011) accurately compute the impact of a change in ozone on the equilibrium temperature (Teq). Their results show that an increase in 20% ozone changes Teq in approximately $\sim$1 K above the tropical tropopause (see their Figure 5). The uncertainty associated to the error in MLS ozone measurements can be simply estimated based on this result. The error in MLS ozone observations is estimated to be 0.04 ppmv in the region 215-46 hPa, which at 100-70 hPa corresponds to a $\sim$20% uncertainty. Given the long radiative relaxational timescales in this region (∼1-2 months), this results in a very small (less than 5%) relative uncertainty in the heating rates. Hence, this represents a minor effect compared to other sources of error discussed in the manuscript.

"Page 18826, Fig.3: As I already mentioned, I am interested in the sub-seasonal variations in the upwelling estimates; the dominant periodicity seems about one month, which will also be seen in Fig. 8. I think that these sub-seasonal variations might be closely related to the intraseasonal oscillation such as the Madden-Julian oscillation. These additional explanations could enhance the argument of this paper."

We agree that the MJO could be related to the relative peak in power observed near 30 days, but it is beyond our purpose in the present paper to prove this. Understanding the specific forcings of the sub-seasonal fluctuations observed in the upwelling estimates is a very interesting task for future studies.

"Page 18826, Fig. 4: Height range is rather stretched up to 10 hPa in this Figure. On the other hand, it is restricted up to 50 hPa in Figure 10. I feel somewhat inconsistency in such figure representation."

We agree, and we have changed Figures 4 and 10 to show the same height range in both plots (approximately 100 to 30 hPa).

"Page 18829, line 6: I wonder why the authors leave the eddy transport terms unresolved. If there is any difficulty to calculate them, they should explain the reason why."

The eddy transport term can be computed but, given the coarse spatial resolution of the data, there is a very large uncertainty in this term, and the conclusions drawn from its seasonality would be doubtfully reliable. So it is preferable to leave the residual including the mixing term. We have explained this on line 6 page 18829.

"Page 18829, line 11: I agree that the residuals do not have large annual variations, but they are not negligible; also there is indication that the annual cycle is almost in

phase with the tendency term. Additional argument might be provided."

Our argument is the good agreement between the seasonal cycle of the upwelling term and the ozone tendency, and we are aware that the seasonal variations in the residual term cannot be used to draw any conclusion. We simply observe that the annual variation in the residual is not as large as the one in the upwelling term, particularly at 70 hPa. We have changed the sentence in lines 11-12 page 18829 to clarify this point.

"Page 18829, Fig. 7: In Figure 6 the authors mentioned that the residuals represent eddy transport terms plus uncertainties in the explicitly evaluated terms. If the transport terms dominate, I suppose that we also see large residuals even in the case for CO in Fig. 7. Why are the residuals in Fig. 7 much smaller than those in Fig. 6?"

See response to the first comment of Anonymous Referee # 1. We have included a comment on this in the manuscript (page 18830, line 16).

"Page 18831, eq (8): The authors set the equation constant. But I wonder if the vertical gradient and the static stability terms may be really constant."

Equation 8 is a simplification applied only to the sub-seasonal fluctuations. Hence the assumption is that the vertical gradients and static stability can be considered approximately constant for sub-seasonal timescales. These terms can have seasonal variations, and in fact we point out the seasonal cycle in the static stability (S) and in the vertical gradient of ozone and their effects on the vertical advection terms at 100 hPa (line 22, page 18828 and line 18, page 18829).

"Page 18831, Fig. 8: As already pointed out, with use of this figure the authors should mention about dominant periodicity seen in the time series of Fig. 3. Also I feel this figure may be moved in the earlier place of this paper; for example, after Fig. 3."

We refer our answer to the previous comment of the referee. On the other hand, we think the Figure 8 should not be moved to Figure 3, given that the scope of the figure is to show the different behavior among the upwelling estimates on very fast timescales, and hence justify the 6-days high frequency cut in Section 3.2 (as explained in line 13, page 18831).

"It would be helpful if mean vertical profiles of ozone and CO would be drawn somewhere."

In our opinion including the time-mean vertical profiles of ozone and CO is not essential or particularly useful to clarify any point of the paper, and they are already shown in previous works (e.g. Froidevaux et al., 2008; Randel et al., 2007).

References

Froidevaux, L., Jiang, Y. B., Lambert, a., Livesey, N. J., Read, W. G., Waters, J. W., Browell, E. V., Hair, J. W., Avery, M. a., McGee, T. J., Twigg, L. W., et al.: Validation of Aura Microwave Limb Sounder stratospheric ozone measurements, Journal of Geophysical Research, 113(D15), 1–24, doi:10.1029/2007JD008771, 2008.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18817, 2012.