Atmos. Chem. Phys. Discuss., 12, C7442–C7445, 2012
www.atmos-chem-phys-discuss.net/12/C7442/2012/

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.

Anonymous Referee #4

Received and published: 25 September 2012

Review of M. Abalos, W. J. Randel, and E. Serrano, "Variability in upwelling across the tropical tropopause and correlations with tracers in the lower stratosphere"

Recommendation: Minor revision required

General comment:

This paper provides detailed analyses about upwelling variability around the tropical lower stratosphere on sub-seasonal to seasonal timescales. Three different estimates are employed from the ERA-Interim reanalysis data: one depending on the reanalysis zonal mean vertical velocity itself and the other two derived from thermodynamic and momentum balance calculations. These estimates are compared with temperatures

from the reanalysis and ozone and carbon monoxide (CO) from the Aura Microwave Limb Sounder. The main conclusions of this paper are: The vertical velocity estimates in the three ways correspond very well with each other, correlations between temperature, ozone and CO are very nice and also with vertical velocity, and the budget analyses for time tendencies of temperature and tracers clearly show the importance of upwelling in forcing tracer variations in the tropical lower stratosphere.

I found that their analyses are very detailed and I basically agree with their messages. Personally I am interested in the sub-seasonal variability in upwelling and traces, which may be closely associated with the intrasesonal variation such as the Madden-Julian oscillation (MJO). I believe that detailed analyses of the intraseasonal variation could enhance their arguments on the momentum source of upwelling in the equatorial lower stratosphere, but unfortunately they did not explicitly include such related results in this study. Though I feel a slight frustration as mentioned above, I recommend that this paper is acceptable to ACP after some minor revisions based on the following comments which the authors might consider to take into account.

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.

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

ACPD

12, C7442–C7445, 2012

around the tropopause, but you may mention about this point.

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?

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.

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.

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.

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.

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?

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.

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

ACPD

12, C7442–C7445, 2012

figure may be moved in the earlier place of this paper; for example, after Fig. 3.

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

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