Atmos. Chem. Phys. Discuss., 14, C4178–C4182, 2014
www.atmos-chem-phys-discuss.net/14/C4178/2014/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "On the hiatus in the acceleration of tropical upwelling since the beginning of the 21st century" *by* J. Aschmann et al.

J. Aschmann et al.

jaschman@iup.physik.uni-bremen.de

Received and published: 27 June 2014

We thank the referee for her/his positive and thoughtful comments to improve the manuscript. In the following, the original remarks of the referee are in *italics*.

1) Structural change of BDC:

My first major comment concerns a potential change in the structure of the BDC after the year 2000, as has been recently discussed by Boenisch et al. (2011), who argued for an increase in only the shallow circulation branch. The transition region

between the shallow and deep branches of the BDC is located exactly within the 17–21 km layer considered by Aschmann et al. Hence, from the perspective of such a structural change the weakening tropical upward mass flux across 70 hPa after the year 2002 (see Fig. 4) is not necessarily in contradiction to a continued increase in tropical upwelling, if only the shallow circulation branch is strengthening. Further, if the ozone partial column between 17–21 km is dominated by the upper levels (what is very likely due to the strong increase of tropical ozone with altitude), the related ozone response could likely show the observed decrease before 2002 and a hiatus of this decrease afterwards.

Therefore, linking the trends of ozone partial columns as well as of mass flux across 70 hPa to changes in tropical upwelling in general, seems oversimplified to me. I think it could be beneficial for this study to separate the effects of changes in the shallow and deep circulation branches, to clarify whether a general weakening or a structural change in the circulation has occurred. A simple step into this direction could be to consider different levels and to infer the shallow branch from the difference, similar to the study by Lin and Fu (2013).

Thank you for this helpful suggestion. We adopted the approach of Lin and Fu (2013) and analysed the changes in the different branches of the BDC. An additional figure and discussion has been added to the revised manuscript. According to the definition by Lin and Fu (2013), we find that the acceleration of the shallow branch of the BDC (70–30 hPa) has stopped after about 2002. On the other hand, we find an increase of upwelling in the transition branch (100–70 hPa) after 2002, which is consistent with the findings of Boenisch et al. (2011).

2) Impact of mixing on tropical ozone:

My second major comment addresses the impact of horizontal mixing with mid-latitudes across the subtropical mixing barrier on tropical ozone. Such mixing *transports ozone-rich mid-latitude air into the tropics and is not a 'secondary effect' (P9953/L15), as first shown by Konopka et al. (2009). The controversial debate about the impact of such in-mixing has been reconciled in the recent paper by Abalos et al. (2013). At levels of about 17–18 km about 50% of tropical ozone during summer has been in-mixed from mid-latitudes, and even at 21 km this fraction amounts to 20(Abalos et al., 2013, Fig. 4b), although the mixing itself becomes very weak above the TTL.*

Consequently, a slight trend in mixing, even if occurring below the considered layer, could likely have a significant impact on the partial ozone column between 17–21 km. Hence, linking the tropical ozone trend to upwelling without taking mixing into account seems problematic to me. If the mixing effect could be diagnosed within the model, this could 'proof' the dominant role of upwelling in causing the ozone trend - or shed more light on a potential additional role of mixing. At least a critical discussion is needed to make the reader aware of this source of uncertainty in the analysis.

The impact of in-mixing on LS O3 is an important point. To address this question and elucidate the role of ODS phase-out for the observed trend-change we conducted a sensitivity simulation with identical setup but with ODS emissions fixed to 1980 values. Consequently, the O3 field evolves significantly different compared to base run. However, in contrast to higher latitudes we find only minor changes of LS O3 in the tropics. More important we see a similar trend-change as in the standard simulation. This result is a strong argument that the observed trend-change is not primarily related to ODS-involved chemistry. Although we don't assess in-mixing directly, the small O3 changes in the tropical LS suggest that in-mixing is not the main driver of the observed trend-change. We added a discussion of these findings in the revised manuscript.

P9955, L11:
Is there a particular reason to use all-sky and not the total diabatic heating rate from

ERA-Interim to drive the model vertical transport? The total heating rate would include all diabatic processes, although above about 100 hPa both heating rates are very similar.

No, there is no particular reason for the usage of all-sky vs. total heating rates (aside from availability issues). We made sensitivity calculations of w^* with total heating rates and found no significant differences in the tropical LS, which confirms the reviewers statement that the remaining diabatic processes (mainly latent heat release) are not relevant at the top of the tropopause and above.

P9959, L1ff:
Can effects related to chemistry totally be neglected? The authors state that 'neither process is sufficient to explain a short-term change'. It would be good to present results supporting this statement. Further, horizontal mixing with the extratropics (see comment 2) provides a pathway for chemistry related changes in extratropical ozone to affect tropical ozone. Again, these issues have to be critically discussed.

We apologise for the unclear phrasing, we would like to point out that Meul et al. (2014) do not find indications for short-term chemical impacts on LS O3, only on longer time scales. For the role of horizontal mixing please refer to our answer above.

Fig. 4a:
Wouldn't it be better to show also the tropical heating rate as an average between turn-around latitudes, analogously to the mass flux case, and not as an 20S–20N average?

We agree that using turnaround latitudes appears to be more consistent to the rest of the figure, however, we would like to keep the 20S–20N margin for the following reasons: First we would like to retain the direct comparability to the LS O3 columns shown before, which have also been averaged between 20S–20N. Secondly, the presented heating rates are an average value over the vertical range of 17–21 km, therefore there is no clearly defined turnaround latitude, which is representative for the whole altitude range.

Technical corrections:
P9953, L5: Age of air changes indicate...
P9957, L23: ... illustrate ...
Fig. 4 caption: ... LS all-sky heating rate ...

We have fixed all of the editorial comments above and thank again the referee for the careful reading.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9951, 2014.