Atmos. Chem. Phys. Discuss., 14, C4187–C4192, 2014
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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 interesting thoughts and suggestions. In the following, the original remarks of the referee are in *italics*.

Aschmann et al. present an analysis of tropical lower stratospheric ozone for the period 1980 to 2013 using observations, and results from a chemical transport model driven with ERA-Interim winds and radiative heating rates. The main result shown is that the ERA-Interim radiative heating rates seem to have a trend-change around the year 2002, which then forces the tropical lower stratospheric ozone in the

*chemical transport model The model forced with the ERA-Interim data overestimates the observed ozone trends by a factor 2 for the pre-2002 period. The authors note that the (dis-)agreement is within the uncertainty of previous studies, but seem not concerned that the difference is a full factor 2 (i.e. -8.1% versus -3.9%). The paper provides no discussion of uncertainties in ERA-Interim heating rates - the single most critical quantity for the results presented. Given that the ERA-Interim radiative heating rates are known to give biased responses to dynamical forcing because of the use of an ozone climatology - notably the quantity central to this paper! - this is a major omission. Indeed, this problem could be the cause for the model overestimate of trends (as ozone mixing ratio in the tropical lower stratosphere decrease with an increase in upwelling, a radiative transfer calculation such as done in ERA-Interim that keeps ozone fixed and only responds to the temperature change with *overestimate* the change in radiative heating, and hence also in w^*). The topic that the paper addresses is very interesting and important, and should be addressed with rigor - such important issues concerning the methodology cannot be just ignored.*

We thank the referee for highlighting an important issue. EI heating rates play indeed a central role in our CTM simulations. However, we do not agree with the assertion that "The main result shown is that the ERA-Interim radiative heating rates seem to have a trend-change around the year 2002...". Our main focus is the fact that both observations as well as our CTM simulations show independently that there is a significant trend-change of tropical LS O3 around 2002, which ends the marked decrease in the previous decades. Given the generally good agreement between model and observations, and taking into account that EI heating rates (and the derived w^*) show a similar behaviour, we conclude that a change in tropical upwelling has occurred. We agree that the usage of a fixed O3 climatology for calculating the heating rates in Era-Interim (Seviour et al., 2012) could lead to biased responses dynamical forcing and is probably responsible for the observed overestimation of vertical transport (e.g., Ploeger et al., 2012). Nevertheless, the diabatic representation of vertical transport seems to give more realistic results in the stratosphere compared to the kinematic approach (e.g., Ploeger et al., 2011; Diallo et al., 2012). Regarding the difference between the modelled and observed pre-2002 trend, it is clear that the primary cause is the high-bias of the model relative to SAGE II in the pre-Pinatubo years (<1991) as there is remarkably good agreement between model and observations in the later years. In particular, both datasets are consistent around the critical inflexion year (2002), the central point of our study. To address the referee's concerns, we extended the discussion about the model bias and the impact of possible EI heating rate deficiencies in the revised manuscript (Sect. 3.1 and 3.2).

Further, I might challenge the authors presumption that the BD circulation was increasing before the year 2002 - the evidence for this presented in the literature is weak. I am familiar with the data and don't deny that there are indications in this direction, but what we need is firmer evidence, and this paper does not deliver this. As said above, the "observed" ozone has a much smaller trend than the model. Indeed, it could also be that the fact that the two have the same sign is coincidence (there is a fifty-fifty chance that an error in the ERA-Interim assimilation system induces a positive/negative trend). I would think that were it not for the CCM results that show an increase in the BD circulation, we might look differently at the observations (Figure 2).

We agree that it is challenging to pinpoint changes of the BDC given its complexity and inaccessibility to direct measurements. We further concur that more analysis and in particular more observational data is definitely needed to get a clearer picture. However, from the reported observations and simulation studies there are more indications for an acceleration of the (lower-branch) BDC at least in the last decades of the 20th century than against this hypothesis ("... authors presumption that the BD circulation was increasing before the year 2002"). On this basis, this study does not try to "prove" an acceleration of the BDC, as we are only show well-known data. Our focus is on the consistency of recent observations with a postulated acceleration

of the BDC (e.g., Butchart et al., 2010; Randel and Thompson, 2011), which has not been discussed in detail in present literature so far.

I think the paper needs major revisions, and would encourage the authors to substantially strengthen the discussion of the following aspects: (i) What do ERA-Interim radiative heating rates represent, what are sources of biases, and how do they potentially affect the model results? (ii) Make it clear (also in abstract) that the key to the model result is the ERA-Interim heating rate - no more, no less. (iii) Fidelity of ozone observations - in particular, I would like to see a proper error calculation for the impact of merging data from different data sources. That is, metrics should be provided how well different observations agree in periods of overlap, the uncertainty in the offset, and how that affects the trend estimates. (iv) The link to the warming hiatus in the troposphere is really not established at all in this paper. While we all "hypothesize" (Line 21) that this is the case, we also expect that a paper that discusses this aspect provides the mechanism and evidence.

Regarding points (i) and (ii): As stated above, we have strengthened the discussion about the impact of Era-Interim heating rates.
Regarding point (iii): Thank you for this suggestion. We agree that in principle a complete error calculation would be beneficial for the analysis. However, in practice, this step is often omitted as the most relevant potential problems such as instrumental drifts are difficult to assess and require a dedicated analysis on its own (e.g., Jones et al., 2009; Rahpoe et al., 2013; Eckert et al., 2014). Therefore we follow a similar approach as in previous studies (e.g., Randel and Thompson, 2011) and briefly discuss offset and correlation in the overlap period (p. 9958 l. 3f), showing the data in Fig. 3. Regarding the fidelity of the O3 data in general: We present two sources for the post-2002 period (SCIAMACHY and SHADOZ), which both show no significant negative trend as one would expect for a continued acceleration of the BDC. This feature is also present in the records of other instruments as pointed out in the paper

(p. 9958, l. 21f). This agreement prompts confidence that the observed trend-change in LS O3 is indeed real and no instrumental artefact.
Regarding point (iv): We agree that the paper does not establish a link between the SST warming hiatus and tropical upwelling. A detailed analysis is way beyond the scope of this paper, however, we would like to point out that there are good arguments that the observed trend-change in O3/upwelling is possibly related to this phenomenon. Although the connection between SST and upwelling is well established in the literature, the possible impact of the recent cooling of the Eastern Pacific on LS O3 has not been discussed so far, to the best of our knowledge. In either case we agree that the conclusion section needs rephrasing, which will be done for the revised version.

P1/L41: They are consistent with, but not really proof that the BD is accelerating.

We agree.

P1/L63: Meridional mixing is probably not a secondary effect, but of similar importance. To the best of my knowledge no study has been conducted to quantify a possible trend in the mixing contribution, so I don't think that you can rule it out without any analysis of the problem.

The first referee raises a similar concern. Therefore we conducted an additional sensitivity simulation to assess the impact of in-mixing. Details are given in the corresponding reply; furthermore we added an additional paragraph to Sect. 3.1. The bottom line is that in-mixing does indeed impact the trends, but its contribution is small compared to changes in vertical transport.

P2/L35: See above - The absolute offset mentioned is less critical than the degree

of agreement during the overlap period; please provide a careful error propagation calculation.

Please refer to our reply above (re. (iv)).

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