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

Anonymous Referee #3

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I apologize to the authors for submitting my review late - I hope you will still find it useful.

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

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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.

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).

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

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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.

Some minor comments:

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

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.

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.

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

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