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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.

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We thank the referee for her/his helpful comments and the invested effort. In the following, the original remarks of the referee are in *italics*.

p9953 l25 ff: Recently strong indication has been found that the large SCIAMACHY ozone trends which are in disagreement with the other satellite observations mentioned above may be due to an instrument drift (EGU 2014-5678, SI2N assessment of vertical ozone trends: Stability of limb/occultation data records over 1984-2013 against ground- based networks Daan Hubert). This drift was detected by

Daan Hubert in the ESA data product, not the Bremen data product, but since drifts are often an instrumental, not a retrieval problem, one might suspect that also the Bremen data may be affected by a drift. In the discussion paper drift issues are not discussed at all. During the post- 2002 period nearly all trend information comes from SCIAMACHY data. Admittedly the SAGE data are available until 2005 but in these no inflexion is visible between 2000 and 2005 in Fig.1. Thus the observational evidence for the inflexion (at least with respect to satellite data) relies fully on SCIAMACHY. This all indicates that there is some non- negligible risk that a major part of the explained phenomenon could be an artefact. At the very least the risk of a potential drift should be critically discussed in the paper.

We agree that instrumental drifts are always a potential danger for trend calculations, in particular for relatively short timeseries as discussed in this paper. However, there are strong indications that the existence of the LS O3 trend-change is no instrumental artefact. Firstly, there are the SHADOZ data, which show essentially the same behaviour as SCIAMACHY, although admittedly the horizontal sampling provided by the sonde stations is poor compared to a satellite instrument. Perhaps more convincing is the fact that several satellite-borne instruments show positive trends in the LS, for example OSIRIS, MLS (Gebhardt et al., 2014), MIPAS (Eckert et al., 2014) and GOMOS (Kyrölä et al., 2013). Despite the considerable spread in the determined trends all mentioned instruments show no further decrease of tropical LS O3 during the last decade, as one would expect with continuous acceleration of tropical upwelling. We make this point more clear in the revised manuscript.

p9955 Eq 1: It is not clear what X_{1t} and X_{2t} are. This, however, is crucial to understand how Eq 1 can produce an inflexion point. I suspect that X_{2t} is zero before 2002 but this must be explained.

We added the definitions of the trend functions to the revised manuscript.

p9956 Eq 2-4 and p9957 l1: According to the Reinsel 2002 paper, their Eq. 1 and subsequent text, it seems to me that σ_N should be the standard deviation of the fit residuals rather than the standard error. Please check.

We apologise, you are absolutely correct. We altered the manuscript accordingly.

p9957 l8: No error bars or covariance matrices are shown or discussed but χ^2 values are presented. How are these calculated?

Again we have to apologise for unclear phrasing. We made no χ^2 test but we minimised χ^2, i.e. the sum of the squared difference between the (observational/model) data and the regression as in Jones et al. (2009). This will be fixed in the revised manuscript.

p9958 l19: The SHADOZ data should be shown in one of the figures.

Done. We've added two additional panels to Fig. 3.

p9959 1st par: "However, neither process is sufficient to explain a short-term trend change." This statement needs justification. Some quantitative estimates are needed on what the competing processes can do. Have the authors ruled out that temperature trends may affect the column density but not VMR?

Please refer to our answer to referee 1 who made a similar statement. Regarding the impact of temperature trends we can confirm that the observed LS O3 trend-change is visible both in partial columns as in VMR (e.g., Gebhardt et al., 2014).

p9961 l8: The last sentence is misleading: I do not challenge that the ocean-

atmosphere interaction is important to predict the BDC but I do not quite see that this emerges from the findings of this paper. I appreciate that the findings of the paper are discussed in the context of existing work, but the phrasing "In conclusion, the accuracy..." at a very prominent place in the paper (the last statement!) does not seem appropriate to me. Please distinguish clearly what is common knowledge and what is the immediate result of your study.

We agree that this sentence should be rephrased. We reworked the conclusions in the revised manuscript and added additional discussion about ocean-atmosphere interaction.

Minor technical and language issues:
p9954 l10 have been omitted (plural)

Fixed. Thank you.

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