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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.

**Anonymous Referee #2**

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**Genetal comments:**

The paper by Aschmann et al. explains observed tropical lower stratospheric ozone trend changes by changes in vertical transport. The topic of the paper is interesting and well within the scope of ACP. My main concern is the robustness of the obseration data used. The presentation is adequate. Reference to relevant existing work is made.

**Specific comments:**

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.

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.

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

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

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

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?

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.

**Minor technical and language issues:**

p9954 l10 have been omitted (plural)

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