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Interactive comment on "On the possible causes of recent increases in NH total ozone from a statistical analysis of satellite data from 1979 to 2003" *by* S. Dhomse et al.

S. Dhomse et al.

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We thank our reviewer for his thoughtful and helpful comments. The important remarks to which we prepared a reply are shown in italics.

Specific comments

Page 11348, line 24: The 'larger downward trend' using the EESC term in figure 12 cannot be directly compared to those using the linear term in figure 11, because the former is derived for the period until 1995 while the latter is for the whole period.

We cite both numbers: the linear trend in 1979-2003 as well as the linear part of the EESC (1979-1995). Both numbers are discussed to illustrate the effect from using linear trend and EESC proxies. We think this is clear from the description in this paragraph.

Page 11348, lines 26-29: The 'recovery of up to 5 to 6 DU/decade ... associated to the turnaround of stratospheric chlorine' is derived by extrapolating the result of figure 12 (trends until 1995 with EESC term) with the help of figure 2 (EESC evolution). Two comments here: i) instead, could not just derive a 1996-2003 trend and confirm the finding ? ii) The trends in figures 11-13 were derived with a regression using a combination of some explanatory variables only (linear, EESC, v$prime$T$prime$ and VPSC. So I can see the results and findings from the discussion of these figures as an assessment of the relative role of these specific processes. But an overall quantification of the contribution from the various forcings can be made only from the multiple regression using all assumed variables (Solar, QBO, etc) like in figure 10 (since the exact contribution could be affected by the exclusion or inclusion of certain proxies in the regression).

I think there is a misunderstanding here, All results presented in Figs. 11-13 were derived using the full regression model (Eq. 3). Fig. 11 shows the linear trend terms from the fit with Eq. 3. In Fig. 12, only the linear trend terms in Eq. 3 were replaced by the EESC terms (see Eq. 5). This figure shows then the linear part of the EESC trend until 1995. Going a step further, the eddy heat flux terms were then replaced by the PSC volume terms (see Eq. 5) and the EESC fit results (with linear trends up 1995) are shown in Fig. 13.

We modified both paragraphs (after line 20 on p. 11348) and figure captions to Figs. 11-13 to clarify this.

Page 11349, lines 2-6 and 24-26: The use of PSC volume instead of eddy heat flux term in figure 13 reduces the statistical significance and the contribution of the EESC

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term. But from equations (2) and (5), the PSC volume proxy is scaled to the EESC, therefore includes part of the influence of the EESC, which is already used in the other EESC term. The multiplication of the PSC volume with the EESC to derive a proxy for the PSC induced polar depletion has a logical basis, as well as the need to account for the background gas-phase chlorine chemistry with a separate EESC term. But caution is needed for this simultaneous use of EESC twice (one directly in the EESC term and one indirectly in the VPSC term) in the regression. I would like to see some comment on this, especially in the quantitative interpretation of the results.

With regard to the scaling of the VPSC with the EESC we provided the explanation that the polar ozone loss does not only depend on the amount of PSCs as expressed by the PSC volume but also on the available chlorine that can be activated by PSCs, the latter quantified by the EESC. There is some correlation with the pure EESC term. We interpret the polar ozone loss proxy as the nonlinear chemical effect (rapid depletion during winter and spring), while the EESC (or linear trend) proxy stands for the (slower) background gas phase chemistry affecting ozone. The reviewer is correct that both processes are not completely separable as outlined above, but still we think that this is valuable for assessing the various factors on ozone variability.

To make some points more clear we added the following after line 18 (p. 11349):

′...In other words, the variability in polar (here Arctic) ozone loss suffices to explain major parts of the observed turnaround in NH midlatitude and polar ozone after decreases until early 1990s that experienced a series of cold Arctic stratospheric winters (Pawson 1999). It is important to note that the polar ozone loss is dynamically driven by temperature changes related to the wave driving as discussed before. This also means that the polar ozone loss proxy accounts in addition for dynamically driven ozone variability related to ozone transport and synoptic meteorology (Hood and Soukharev, 2005, Wohltmann et al., 2005)′

′The polar ozone loss proxy (cumulative PSC volume) is mainly associated with the

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nonlinear chemical effect that explains the rapid depletion during winter and spring, while the EESC (or linear trend) proxy stands for the (slower) background gas phase chemistry affecting long term changes in ozone. Both processes are not completely separable as suggested from our analysis, since both proxies contain the EESC function. But this separation is still valuable for approximating the various chemical causes of observed ozone variability.′

Minor comments

1. *Conclusion no. 4 about the eddy heat flux is not directly related to any results. Please connect it with some of the paper146;s findings or move it to a discussion section or in the introduction.* We removed item 4, since this has been explained at several places in the text.

2. *In conclusion no. 5 you could also compare your results to Hadjinicolaou, Pyle and Harris (GRL, 2005) findings about the dynamically-driven turnaround.* Reference is made to this paper.

3. *In figure 4, the units of h* are missing. Please insert in the horizontal axis or in the figure caption.* Has been corrected.

4. *Figures 5-9 are very small relative to the amount of the information and the number of the lines that include. Those figures used to look nice and big in the original manuscript evaluation. Please make bigger (probably this is a publisher146;s issue).* This depends unfortunately on the style file (ACPD or two-column style in ACP). Some of the figures have been revised to take this into account.

5. *Page 13339 line 17 and page 11346 line 25: there is a more recent and comprehensive overview of the ERA-40 analyses in the Uppala, Simmons et al. paper (QJRMS, 2005). Please update the reference.* Done.

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6. *Page 11340, line 18: the correlation coefficient in the text is 0.52 while figure 4 says 0.56, which one is correct?* The latter. Has been corrected.

7. *Page 11343, line 8: the a (psc)m coefficient does not appear in equation (3).* See comments earlier. Eq. 3 contains both eddy heat flux terms and linear trend proxy. The exchange with PSC volume terms (replacing eddy heat flux terms) and EESC terms (replacing linear trend terms) are explained by Eqs. 4 and 5, respectively.

8. Done

9. Conclusion No. 4 was removed (s. above)

10. *Page 11342, lines 21-22: Unless your regression model is not similar at all to the ones used and reviewed by Staehelin et al. (Ozone trends: a review (Rev. Geophys., 39, 2, 231-290, 2001), I would like to see this work cited as well, as an acknowledgement of the European contribution to the statistical trend analysis of ozone.* Has been added.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 11331, 2005.