

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.

Anonymous Referee #2

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General comments

This paper investigates the causes of the recent turnaround of total ozone in the N.H. by statistically analysing satellite observations from 1979 to 2003.

While a traditional mathematical framework is followed in the statistical trend analysis (multiple linear regression), a new proxy is introduced, the product “PSC Volume times EESC”, which can account for the polar ozone depletion (driven by chlorine loading and meteorology). The incorporation of the “accumulative” EP-Flux as a proxy in the regression analysis adds another useful (and better defined than in other studies) factor

involved in the change of ozone, that of the wave-driven mean meridional circulation over a preceding period.

By implementing these parameters, together with the traditional proxies (Solar, QBO), in a multiple linear regression, the authors show that the recent increase of total ozone in the N.H. is mainly driven by the stronger wave activity, which, apart from enhancing the meridional winter/spring build-up of ozone, has also resulted in some warmer Arctic winters with less PSC-driven depletion after the mid-1990s. Increased solar activity was also found to contribute (to a lesser extent) to the recent ozone turnaround.

The paper contains a very informative introduction, solid methodology and use of different satellite datasets and substantial results. It deserves to be published, subject to a few minor text corrections and the following clarifications.

Specific Comments

Clarifications

1. Page 11348, line 24: The “larger downward trends” 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.
2. 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, $v'T'$ 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

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could be affected by the exclusion or inclusion of certain proxies in the regression).

3. 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 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.

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 paper's findings or move it to a discussion section or in the introduction.
2. In conclusion no. 5 you could also compare your results to Hadjinicolaou, Pyle and Harris (GRL, 2005) findings about the dynamically-driven turnaround.
3. In figure 4, the units of h^* are missing. Please insert in the horizontal axis or in the figure caption.
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 publisher's issue).
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.
6. Page 11340, line 18: the correlation coefficient in the text is 0.52 while figure 4 says

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0.56, which one is correct ?

7. Page 11343, line 8: the a (psc)m coefficient does not appear in equation (3).

8. Page 11345, line 27: replace “explains” with “explain”.

9. Page 11351, lines 22-24: add commas as shown: “ ... heat flux, is replaced ...” and “... the stratosphere, is an ...”.

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.

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