

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

1. *At no point do the authors quantify the improvement obtained over classical regressions (which use only trend, QBO, and solar cycle) by including their heat-flux or modified V_{psc} proxies in the regression. This improvement could easily be*

quantified, e.g. by comparing the R^2 of regressions without and with these proxies. Such differences should be reported and an improvement should be stated clearly, especially in abstract and conclusions.

We carefully selected various proxies that stand for different physical and chemical processes that contribute to ozone variability and justified their use. Many earlier studies tended to neglect dynamical processes in their regression analysis or ignored the cumulative effect (see eddy heat flux). All variables that have been included improve the R^2 values, the QBO term is very important for tropical latitudes, while the eddy heat flux terms (or PSC volume) improve more strongly at higher latitudes. The various contributions to ozone variability are shown in Fig. 9.

- The authors do not attempt to answer in a quantitative way the question, whether part of the recent ozone increase can be attributed to decreasing chlorine/EESC. This question is important. It is addressed in Reinsel et al. (2005), who do a very similar analysis of the same SBUV data set, but use a change of trend term. Reinsel et al. (2005) find a significant change of trend which indicates a significant contribution from decreasing chlorine. Since the authors presumably use better proxies, I strongly recommend that the authors also include a similar change-of-trend term in their regression and test for a significant change of trend. This would be very important, e.g. for the upcoming 2006 WMO ozone assessment, and would constitute an important check of the Reinsel et al. (2005) paper. It would also make an important statement about a possible beginning recovery of total ozone. Obviously such a statement can currently not be made by the authors since they completely prescribe the form of the trend term - either linear throughout, or exactly like the EESC time series. Therefore, the authors statements, e.g. in point 5 of their conclusions have to remain vague. Include a change-of-trend term, and report if there is a significant change of trend!*

We indeed tried to use a trend change term in our analysis. When eddy heat flux

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or PSC volume terms are included in the regression, an analysis with a change of trend term become statistically insignificant. Reinsel et al. (2005) also used a dynamical proxy, but they only accumulated this proxy over the three previous months as opposed to our accumulation over the entire winter. The combination of dynamical proxies such as the eddy heat flux and EESC term standing for gas-phase chemistry show that we may observe indeed a modest recovery of up to 5DU/decade (Fig. 12), but more important is the reduction of polar ozone less (heterogeneous chemistry) related to increases in eddy heat fluxes that have contributed in a major way to the NH increase. Due to the large variability in total ozone, a *recovery* due to changes in background gas-phase chemistry is difficult to observe (see Fig. 13) within the short period since 1996. The change of trend term is in our opinion difficult to interpret and is just a measure of a missing or not so well described process.

We added after line 26, p. 11349, the following: 'In the linear trend model from Eq. (3), so-called change of trend terms (beginning in January 1996) have been included as proposed by Reinsel et al. (2005). In our case the change of trend terms were statistically insignificant at 1σ (not shown here), most likely due to the too short period after 1995.'

Additional comments

For Figures 5 and 6, I would strongly recommend to omit the bottom panel where the S4755 data are plotted with their full annual cycle. The annual cycle results in a very wide spread of the data and completely obscures the interannual variations, which are the main topic of the paper. So drop the data with annual cycle, and expand the ozone scale. The authors may even consider to do this in Figure 1 as well. The ozone annual cycle is known to everybody by now. With the annual cycle removed, the interannual variations will appear much clearer. Also: Are correlation coefficients in the figures

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given for the data with annual cycle, or are they for the anomalies, without annual cycle? When the annual cycle is left in the data, the correlation will come out much higher, but this higher correlation has little meaning for the interannual variations.

We removed the annual cycles and show only the anomalies (Figs. 1, 5, and 6). Correlation coefficients were always indicated for the anomalies.

Fig. 7 could be omitted completely. It is good that the authors address auto-correlation in the residuals, but I see very little information from Figure 7. The main points stated in the text are sufficient.

We like to leave it in. Reviewer 2 particularly liked the discussion on auto-correlation. This figure is also helpful in showing that the various SBUV data sets do not show significant jumps when changing between satellite platforms.

In nearly all Figures, but particularly in Figs. 5 and 6, the axis labels and other text are very small and almost unreadable. I urge the authors to enlarge the labels in the ACP revised manuscript.

As discussed in the reply to Reviewer #2, the difficulties come from the various style files that allow different scaling of the plots. We will try to improve upon it.

Point 1 of the conclusions: replace 'mainly due to increases' with 'highly correlated with increases'. 'Due' implies a causal relation, which is assumed a priori by the authors. The paper shows this correlation, but does not present a physical proof for a cause and effect relation.

We prefer to change it to 'related to'.

Point 6 of the conclusions: Coupling between solar-cycle, QBO, and ENSO (Labitzke et al.) is likely to be an important factor in polar variations, i.e. important for the Vpsc and heat flux terms used in this paper. It may be of similar importance as climate change and should be mentioned here.

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This point was not the major focus of this paper, but when we discuss the various factors contributing to ozone variability in the Introduction section we hint at the links of solar variability to dynamical changes in the lower atmosphere (see p. 11337, line 7 and 8) and we also mention the Holton-Tan effect (p. 11335, line 20) that describes the QBO link to mid-latitude atmospheric dynamics. I think that this suffices.

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