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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.

Anonymous Referee #4

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This is a solid paper. It investigates the question why ozone levels have been increasing over the Northern hemisphere in recent years. This is an important question, because it is related to the question whether positive consequences of the CFC-ban following the Montreal protocol can now be seen in the ozone layer. The underlying data and methodology of the paper are well founded, and the assumptions are clearly stated. The authors use the standard regression method, but introduce new and well founded proxies for ozone transport and polar processing. The conclusions are careful and are supported by the presented data and analysis. The work is put into the proper context.

Nevertheless, I do find the paper disappointing in two respects:

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.

2.) 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!

I would strongly urge the authors to try and improve these two aspects in a revised ACP version of the paper. Especially my point 2.) is important. It is almost a "major revision" that I would request.

My next points mainly adress the clarity/ necessity of the figures.

For Figures 5 and 6, I would strongly recommend to omit the bottom panel where the

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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 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.

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.

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.

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.

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 V_{psc} and heat flux terms used in this paper. It may be of similar importance as climate change and should be mentioned here.

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