

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.

Major comments

The authors appear to be unaware of the recent paper: Hadjinicolaou, P., J.A. Pyle, and N.R.P. Harris, The recent turnaround in stratospheric S4810 ozone over northern middle latitudes: A dynamical modeling perspective, Geophysical Research Letters,

32, L12821, doi:10.1029/2005GL022476, 2005. which arrives at the same conclusion but from a modelling perspective.

We were aware of the paper, however we do not discuss analyses with CTM or GCM model results. There are numerous trend studies based upon model data and it would be beyond the scope of this paper to cite them all. However, since this particular paper came to similar conclusions we cite this paper in the Conclusion section (Item 4 in new version and Item 5 in ACPD version). This suggestion was also made by Reviewer # 2.

The introduction (section 1) is far too long. Many other papers and many ozone assessments have discussed in detail the processes that contribute to ozone variability. The 3 page review of these processes is not required here and citation of a few key papers and/or ozone assessments would be sufficient. There is text in there that is extremely basic knowledge that most readers will have.

We disagree here. The individual processes may be basic knowledge in some aspects, but very rarely all processes were included in such an analysis. We also try to point out at the links between the various processes that are important when interpreting our results. Reviewer #2 particularly liked this introduction, so we decided to leave as is.

Specific comments

Unfortunately the specific comments are referring to an older version of the manuscript (even before Quick Report), so that after revision some of it were already changed. Also the line numbers have changed since then.

line 22 (ACPD: lines 15-25, p. 11332): Do you mean attribute the changes in ozone to changes in EESC? You haven't specified what you are attempting to attribute the changing ozone to.

This is referring to the older version. The abstract was changed in the ACPD version.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

Figure 1: It is very difficult to see all of the time series in Figure 1. For example, I can hardly see the 146;GOME (annual mean)146; time series at all. Rather than showing the monthly means and the annual means, I would suggest showing the mean annual cycles from the 3 data sets in a separate panel on the right of the figure, and then the monthly mean time series with the mean annual cycles subtracted. See Figure 2 of Struthers, H., K. Kreher, J. Austin, R. Schofield, G.E. Bodeker, P.V. Johnston, H. Shiona, and A. Thomas, Past and future simulations of NO2 from a coupled chemistry climate model in comparison with observations, Atmospheric Chemistry and Physics, 4, 2227-2239, 2004 for an example of what I mean. This would also help your discussion since on line 34 you refer to the seasonal cycle explicitly anyway. Is it correct to refer to 50N-60N as 146;high latitudes146;. For me high latitudes means poleward of 60 degrees.

This figure has been changed. We now show the total ozone anomaly (after subtracting seasonal cycle, e.g. monthly means) and separated the annual mean from the monthly time series. In the figure caption we now refer to that latitude band as *higher* latitudes.

Line 46 (ACPD, line 19, p. 11333): when you say 'changes in stratospheric circulation patterns' are you including changes in stratospheric temperatures?

This was remarked upon in the quick reports and was already modified in the current ACPD version. We replaced 'circulation pattern' by 'ozone transport'.

line 60 (ACPD, line 6, p. 11334): What waves are you referring to? Gravity waves, planetary waves etc.?

We clearly refer to planetary waves in this subsection.

Line 94 (ACPD, line 18, p. 11335): You need a citation here to support the statement that the QBO response to ozone has largest amplitude in winter and spring. I think that one of the Tung and Yang papers will do the job.

We added Randel and Cobb (1994), Baldwin (2001), and the first Tung and Yang paper

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(1994).

Line 106 (ACPD, line 3, p. 11336): chlorine and bromine containing species.

We changed the sentence as follows: '... to reduce emissions of chlorine and bromine containing species. The various halogen compounds have different ...' In addition the subsection title was changed to 'stratospheric halogen loading'.

Line 108 (ACPD line 9, p. 11336): HCl is not a measure of the total amount of chlorine in the stratosphere. It is at around 55km but lower down you need both HCl and ClONO2 to get a handle on the total chlorine.

We say now 'total amount of chlorine in the upper stratosphere' (added: upper).

Line 121 (ACPD line 25, p. 11336): What 146;regression models146;? This is the first mention of regression models in this paper and you are referring to THE regression models. What models are these?

We modified this sentence as follows: 'Significant influences of solar variability on different meteorological quantities have been identified (Rind, 2002)'. Literature regarding regression models are listed in the beginning of Section 6.

Line 128 (ACPD line 5, p. 11337): You have omitted the effect of energetic solar particles on mesospheric NO_y and subsequent transport of the NO_y to the stratosphere.

We complemented the sentence as follows: '...decreases in high altitude ozone concentration related to downward transport of NO_y from the mesosphere down to the upper stratosphere'.

Line 136 (ACPD, line 17, p. 11337): But the large ozone deficit following the eruption of Mt. Pinatubo was observed only in the northern hemisphere.

This is indeed an important point that remains not well understood, why a similar ozone deficit was not observed in the southern hemisphere. We added a remark in the paper.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

Line 186 (ACPD line 17, p. 11339): I understand that there are some discontinuities and perhaps some errors in the data when switching from the ERA-40 data to the operational data post 2002. Would these errors affect your results in any way?

We checked the overlap period between both data sets in 2001/2002, but we did not see any significant changes, so they can be combined.

Figure 3: Is this figure really necessary or does it simply show a result which has already been reported on in Fioletov and- Shepherd 2003.

This figure is related to the findings from Fioletov and Shepherd (see figure caption), but it provides the main motivation for using the cumulative proxies based upon monthly mean PSC volumes and eddy heat fluxes as used in our regression analysis. In our opinion it should remain in the paper.

Line 210 (ACPD line 13, p. 11340): I would not refer to what is shown in Figure 4 as a compact relationship. In fact the eddy heat flux only explains 25% of the variance in the winter-time ozone build up.

With a compact relationship we were only referring to Figure 5 of the Weber et al. (2003) paper. In Figure 4 of this paper we show the ozone gain for a longer time period (25y) and the correlation is not as pronounced than for the GOME years after 1995. In one of the older manuscript versions we were also referring to a compact relationship in Figure 4, but not in the final ACPD version.

Line 215 (ACPD, line 20, p. 11340): The data point for 1990/1991 is at least as far as the 1991/1992 data point from the regression line. You are quick to explain that the 1991/1992 and 1992/1993 deviations are a result of Pinatubo but what about the other data points that are even further from the regression line. How do you explain them? How can you then be so sure that the 1991/1992 and 1992/1993 deviations are a result of Pinatubo?

The reviewer is right. We now state that only the winter 1992/93 stands clearly away

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from the regression line.

Line 223 (ACPD paragraph after line 23, p. 11340): Are you using southern hemisphere eddy heat flux proxies for the northern hemisphere? This wasn't clear to me from what you had written. I am just a bit confused from this sudden switch to talking about the southern hemisphere when the paper concentrates on northern hemisphere ozone. What's going on?

We think that the use of the SH eddy heat flux is clearly explained. The tropics are the raising branch of the residual circulation in both SH and NH winter hemispheres. It is, therefore, common to use wave driving from both hemispheres to investigate tropical variability of, for instance, water vapor.

Line 228 (ACPD, line 6, p. 11341): It's not clear to me what you mean by that stands for;. Do you mean that V_{PSC} is a proxy for heterogeneous chemical ozone loss?

We modified the phrase as follows: 'The PSC volume is a suitable proxy for heterogeneous chemical (i.e. polar) ozone loss'

Line 234 (ACPD line 13, p. 11341): Where does this nitric acid profile that you are using come from?

This is a typical profile measured by ASUR, see Kleinböhl et al., J. Geophys. Res., 2003. Reference has been added.

Line 259 (ACPD line 15, p. 11342): Please provide a citation or source for where you obtained the MgII solar flux indices.

From ftp://ftp.ngdc.noaa.gov/STP/SOLAR_DATA/SOLAR_UV/NOAAMgII_dat.htm. Has been added to the text.

The presentation of equation 3 is unnecessarily complicated. It could be made considerably shorter and more transparent if you just said 'The following equation is fitted

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independently to the data for each calendar month' and then drop the summations, the deltas etc.

Some of the term like solar flux and SOI are one constant that is the same for each month (without summation sign).

Line 275 (ACPD line 6, p. 11343): The 12 monthly regression coefficients from your regression model account for more than the seasonal variation in ozone - they also account for seasonality in the factors affecting ozone.

They are monthly constants that removes the annual cycle. In the regression we are mainly interested in the interannual variability so that seasonal (year-to-year) constant effects are of no interest to us. To make it somewhat more clear we change monthly coefficients to monthly constants in the text.

Line 294 (ACPD line 11, p. 11344): Not just 146; somehow 146; linked. The ways in which they are linked are very well understood and have been modelled in detail.

Agreed.

Figure 5: In the caption it says that the results for April are shown in Blue and the results for September are shown in violet though the labeling in the plot suggests that it 146; s other way around. You may also need to expand the vertical scales in Figure 5 - as it stands it 146; s quite difficult to see what is going on.

Figure has been changed for better clarity. The requirements unfortunately depends on the style file used, so they may change again in the two column format of ACP. see also comments by Reviewer #2 and #4.

Line 344 (ACPD line 6-12, p. 11346): You state that 'For all latitude bands residuals do not show any significant auto-correlation after one Cochrane-Orcutt transformation'. This is very difficult, if not impossible to see from Figure 7. Is the reader supposed to be able to see this in Figure 7?

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

We changed the figures to make that point more clear. The residuals after transformation look more random (month to month variability). We also cite the autocorrelation value in the figure (before and after transformation).

Grammar and typographical errors

All agreed.

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

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