Atmos. Chem. Phys. Discuss., 5, S4810–S4814, 2005
www.atmos-chem-phys.org/acpd/5/S4810/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

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 #1

Received and published: 1 January 2006

Review of "On the possible causes of recent increases in NH total ozone from a statistical analysis of satellite data from 1979 to 2003" by Dhomse et al. 2005

GENERAL COMMENTS

This is a well written and interesting paper that presents some important results. There are a few issues that need to be addressed and these are detailed below.

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

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.

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.

SPECIFIC COMMENTS

line 22: 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.

Figure 1: It is very difficult to see all of the time series in Figure 1. For example, I can hardly see the 'GOME (annual mean)' 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 'high latitudes'. For me high latitudes means poleward of 60 degrees.

Line 46: when you say 'changes in stratospheric circulation patterns' are you including changes in stratospheric temperatures?

Line 60: What waves are you referring to? Gravity waves, planetary waves etc.?

ACPD

5, S4810–S4814, 2005

Line 94: 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.

Line 106: chlorine and bromine containing species.

Line 108: 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.

Line 121: What 'regression models'? This is the first mention of regression models in this paper and you are referring to THE regression models. What models are these?

Line 128: You have omitted the effect of energetic solar particles on mesospheric NOy and subsequent transport of the NOy to the stratosphere.

Line 136: But the large ozone deficit following the eruption of Mt. Pinatubo was observed only in the northern hemisphere.

Line 186: 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?

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.

Line 210: 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.

Line 215: 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

ACPD

5, S4810–S4814, 2005

sure that the 1991/1992 and 1992/1993 deviations are a result of Pinatubo?

Line 223: 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?

Line 228: 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?

Line 234: Where does this nitric acid profile that you are using come from?

Line 259: Please provide a citation or source for where you obtained the MgII solar flux indices.

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

Line 275: 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.

Line 294: Not just 'somehow' linked. The ways in which they are linked are very well understood and have been modelled in detail.

I am very happy to see such a concise and thorough discussion of the treatment of autocorrelation in the model residuals.

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's other way around. You may also need to expand the vertical scales in Figure 5 - as it stands it's quite difficult to see what is going on.

ACPD

5, S4810–S4814, 2005

Interactive Comment

Line 344: You state that 'For all latitude bands residuals do not show any significant auto-correlation 345 after one Cochrane-Orcutt transformation'. This is very difficult, if not impossible to see form Figure 7. Is the reader supposed to be able to see this in Figure 7?

GRAMMAR AND TYPOGRAPHICAL ERRORS

Title: Please replace the NH acronym in the title with "Northern Hemisphere" and please expand the acronym in the second line of the abstract.

Sometimes you refer to EESC as effective equivalent stratospheric chlorine and at other times as equivalent effective stratospheric chlorine. At least be consistent.

Line 281: Replace 'was traditionally associated' with 'is traditionally associated'.

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