Atmos. Chem. Phys. Discuss., 10, C11147–C11152, 2010
www.atmos-chem-phys-discuss.net/10/C11147/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of HCl and ClO time series in the upper stratosphere using satellite data sets" *by* A. Jones et al.

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Received and published: 15 December 2010

1 General comments

In general the paper on chlorine species in the upper stratosphere based on 4 satellite instruments is of high scientific quality and worth to be published in ACP, except for the poor analysis and text of sections 3.1 and 4.1 on HALOE which need major revision. Is it really necessary to use a 10km altitude region? The vertical resolution of all the used satellite instruments is better and the region most interesting for homogeneous catalytic ozone destruction by chlorine is around 40 km.

We see it fit to use a 10 km altitude range, simply because it stays consistent with the previous analyses by Jones et al [2009], Steinbrecht etl al [2006], and Newchurch et

al [2003] for ozone, water vapour, and HCl. Furthermore, the altitude coverage used is 35-45 km, hence, the 40 km altitude limit is included in this range.

Section 3.1.: Using 'monthly' values for deseasonalizing HALOE-data is not appropriate because of the sparse coverage, which is strongly dependent on the individual year,due to a temporal progression of the 'sweeps' from one pole to the other (about -5 days per year, see http://haloe.gats-inc.com/home/index.php), and a decreasing number of available profiles with time. Data for the big monthly latitude bins are from different latitudes (circulation regimes) and times for each year which is the most likely reason for the odd unexplained spikes around 2001 in Figures 3 and 7. These problems are obvious in Figure 2 (e.g. the few HALOE data for 60oS are not representative for the 30o to 60oS belt!) but not discussed. To have adequate coverage, at least bi-monthly data should be used, however, it might be better to analyse individual time serieses for smaller latitude bins based on daily data separately, omitting the ones without data at the end of the measurement period and the ones in high latitudes which have no coverage in winter. Averaging can be done later. If the HALOE data time series for a bin between e.g. 5oS to 5oN is analysed, the artifact around 2001 does not happen.

We agree with the reviewer that the use of a large tropics band between 30S-30N is susceptible to differences in sampling for both HALOE and ACE. After examining smaller bins, we find however that there is insufficient ACE data in order to remove the QBO contribution when making a trend estimation. At current, we do not have the necessary time to look at another instrument, and while the MLS would be a perfect fit, there are known issues with the current MLS HCl product. As a note, one should be aware that the analysis by Newchurch et al (2003) examined HALOE HCl between 30S-30N with no apparent problems. Other studies of other trace gases, such as water vapour and ozone using HALOE data have also examined 30S-30N with a reasonable degree of success (for example, Jones et al, 2009). After examining HALOE in 5 degree latitude bands from 30S to 30N we found very similar trend magnitudes ranging from -4% to 6% after 1997 when using the reinsel method (although in each case we did not remove any QBO contributions). It was our conclusion that the use of HALOE HCl between 30S and 30N is valid, but due to the insufficient ACE data for a smaller bin width, as suggested by the reviewer (say 20S to 20N), we calculate a trend estimate for the tropics, but we do not remove the QBO contribution. We agree however for this study, that there are too few 60 degree HALOE measurements and is thus not a good representation of the 30-60 degree bins. Hence, we have made a new trend analysis for 30S-50S and 30N-50N instead for the mid-latitudes. We thank the reviewer for this insight

We also argue that it is not necessary to make bi-monthly averages for HALOE, based on previous analyses, which use single monthly values for similar trend estimates, for example Cunnold et al, 2004, Steinbrecht et al, 2004.

There is also no problem with the year 1992 which should be included in the analysis. Pinatubo was a problem only for low altitudes (in contrast to statement on page 8626).

The reason for not including 1992, was to make sure we stay consistent with what Newchurch et al had done. The reason for their analysis not to include 1992 was due to the fact that the slope of a linear fit will be greatly affected by the beginning and ending data values. Because the HALOE data start during the Pinatubo period, they were very cautious to obtain robust regression. Hence, they omitted any suspicious data especially in the beginning of the line fit in Figure 4.

The method of Reinsel et al (2002) can be applied to regular datasets like TOMS total ozone but not to datasets which strongly change their characteristics with time. Missing data cannot be filled in by interpolation or other mathematical tricks.

We argue that this is not the case. The model can be assumed even if there is a change in the trend, as long as the transition is continuous, and the trends before and after the turning point can be "reasonably approximated" by linear trends. Based on the HALOE data alone we see no evidence as to why this method cannot be used as there is a

clear linear increase before 1997, while even though less linear after 1997, a linear estimate is still valid. Similar techniques have been applied to SAGE and HALOE data with no apparent issues. We agree missing data cannot be filled in by interpolation. This method still works even when using missing data (as long as the turning point in the data is continuous). Figures 3 and 7 were wrong and are recalculated.

It should be also not argued with the observations compiled by Rinsland et al (2003), their temporal behavior is different and has not the artifacts of Figure 7 (except for their Figure 4 which appears to be affected by sampling and averaging artifacts too). The analysis should be redone and section 3.1 be rewritten. Is there an explanation for the spikes in solar time in Figure 4?

We have omitted the Rinsland et al comments. At this time we have no answer for the strong LST spikes in Figure 4. We have investigated the sampling rate as well as the latitude at which the observations are made and find a sufficient number of measurements and no glaring biases. Currently, we cannot at this time find any explanation for the spikes in solar time in Figure 4.

Finally, the reviewer maybe wondering why our HCl trend estimates have changed from the initial manuscript? We have tried to improve our use of the FFT to determine the harmonics of the QBO for each deseasonalised set of anomalies. We believe that we were possibly overestimating the QBO contribution for HCl.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 8623, 2010.

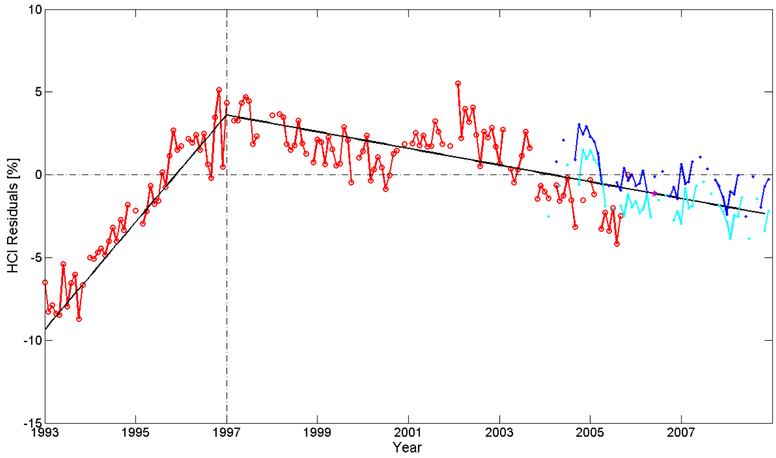
HCl Residuals [%]
10
5
0
-5
-10
-15
1993
1995
1997
1999
2001
2003
2005
2007
Year

Fig. 1.

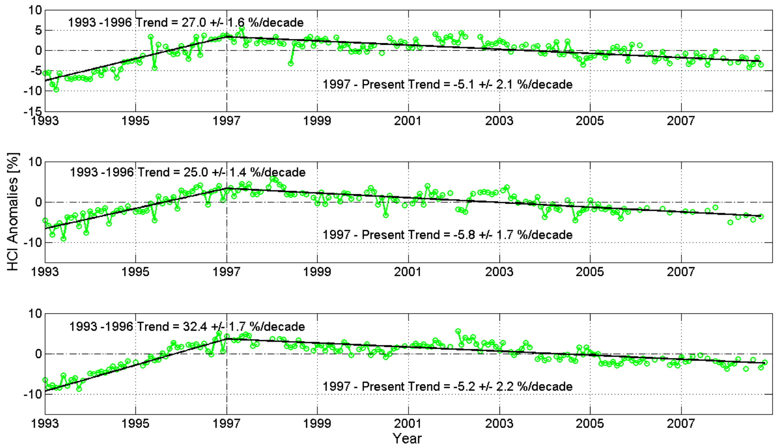
1993 -1996 Trend = 27.0 +/- 1.6 %/decade
1997 - Present Trend = -5.1 +/- 2.1 %/decade
HCl Anomalies [%]
1993 -1996 Trend = 25.0 +/- 1.4 %/decade
1997 - Present Trend = -5.8 +/- 1.7 %/decade
1993 -1996 Trend = 32.4 +/- 1.7 %/decade
1997 - Present Trend = -5.2 +/- 2.2 %/decade
1993
1995
1997
1999
2001
2003
2005
2007
Year

Fig. 2.