

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

Anonymous Referee #2

Received and published: 27 May 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 10 km 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.

C3247

2 Specific Comments

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 60°S are not representative for the 30° to 60°S 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. 5°S to 5°N is analysed, the artifact around 2001 does not happen. 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 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. 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?

For further details I agree with Referee 1.

C3248

3 Technical

page 8630, l9: Isn't the HCl validation paper Russell et al (1996)?

page 8642, l10: Cunnold et al (2004) is not cited in the text.

page 8644, l7: Newman et al (2006) is cited also but not listed here; l10: Typo, O'Doherty; l22: Typo, it should be Russell III.

page 8645, l14: Typo, it should be Brühl; l26: use subscripts; l33: Typo, it should be Waters.

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