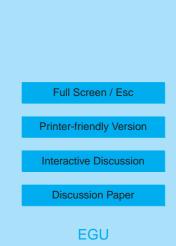**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Trends and variability of midlatitude stratospheric water vapour deduced from the re-evaluated Boulder balloon series and HALOE" *by* M. Scherer et al.

Anonymous Referee #1

Received and published: 19 November 2007

This paper presents a reanalysis of the well-known Boulder stratospheric water vapor measurement time series, including a careful reevaluation of the original data, comparisons with the HALOE satellite observations, and comparisons to some idealized model results. These data provide the longest continuous record of stratospheric water vapor, and the large positive decadal trends derived from these data have driven a substantial amount of research in the community over the past decade (including the SPARC water vapor Assessment). The Boulder time series are based on continual updates and modifications of the measurement details over almost 25 years; there are also some well-known differences in trend results with the HALOE satellite data. Hence, the objectives for this study are to carefully reevaluate the Boulder data, as-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

sess the data quality over time, and make some updated comparisons with the HALOE measurements. Overall this is a valuable study, and the results will be of substantial interest to the wider research community. I do have several comments on details of the analyses and comparisons, but overall I think this paper will be a useful contribution to ACP.

Specific comments:

Data

The balloon frostpoint measurements need to incorporate temperatures from radiosondes that fly with each instrument. Is there any information (metadata) regarding systematic changes in radiosonde instruments used for these measurements over time? There are well-known warm biases in many historical radiosonde temperature measurements in the lower stratosphere (of order 1 K), resulting in artificial cooling trends over time as instruments improve (e.g. Gaffen et al, J. Clim., 2000, p. 1776-). Should this source of uncertainty be considered in these analyses, given that a 0.5 K temperature error results in a 10% water vapor error?

What temperature data are used to interpret HALOE data to theta levels? If reanalysis data are used, how might the well-known trend biases in these temperatures influence the comparisons shown here?

I think overlaying the different subsets of Boulder data in Fig. 2 is quite confusing. I wonder if simply showing 2 panels for each level (using lower and higher quality data, respectively) might be a better option?

Statistical modeling and Results

Several comments on the statistical fits of the data: In general the regression model should include terms for which there are statistically significant effects in the data; thus the inclusion of QBO and equivalent latitude terms are reasonable (given the significant results in Fig. 4), although they result in almost no reduction in overall variance (Fig.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

3). However, I have strong reservations regarding the statistical break imposed in 2001 and the changing trends afterwards. The statistical fits of this break in the FP data is far from significant, and result in an increase for the HALOE data in the 580-620 K layer. The trends for the short record 2001-2005 are not significant for either the Boulder or HALOE data, except for a large negative trend for HALOE over 580-620 K (which is in fact linked to the positive 2001 jump fit in the statistical model). Based on these results, I think this is not an appropriate statistical model for these data, and there is no significant evidence of changing trends before and after 2001.

Comparison with idealized model and Conclusions

I think the comparisons of data with the idealized model calculations are very useful. The differences between the model results and the FP data are quite large, with many apparent outliers in Fig. 8 (not observed in the HALOE comparisons). Can the authors comment on these outliers? Are they indicative of additional questionable FP profiles, or might they be consistent with variability of individual point measurements? The trends between the idealized model and FP data are very large and significant, especially for the longer record (Fig. 9), but the authors seem to steer away from using these comparisons to critically evaluate the FP trends. What do the overall differences in FP trends compared to HALOE and the idealized model suggest regarding uncertainties in the reevaluated FP time series?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14511, 2007.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU