Atmos. Chem. Phys. Discuss., 7, S8365–S8373, 2008
www.atmos-chem-phys-discuss.net/7/S8365/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

7, S8365–S8373, 2008

Interactive Comment

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.

M. Scherer et al.

Received and published: 14 January 2008

REPLY TO REVIEWERS BY S.FUEGLISTALER ON BEHALF OF M. SCHERER AND ALL CO-AUTHORS.

We thank the reviewers for their comments and for raising several important questions. Below, we respond to the reviews in order of 'reviewer number'; we have also extracted the main points from each review, and assigned a number to it (i.e. item 101 is first issue raised by reviewer 1; item 302 is second issue raised by reviewer 3, etc.).

RESPONSE TO REVIEWER 1

(101) Spurious drifts in ambient temperature data, and impacts on trend analysis on

potential temperature levels for both NOAA FP and HALOE. RESPONSE: (a) It is important to note that the (air) temperature sensor on board of the balloon and the temperature sensor for the frostpoint are two different instruments. Hence spurious trends in the former do no affect in any way the derived water vapour mixing ratio. The derived mixing ratio depends solely on the frostpoint temperature and ambient pressure. Each frostpoint temperature sensor is calibrated to a NIST traceable standard, so we do not expect any spurious trend in this part of the measurement. In principle, a spurious drift in derived mixing ratios could arise from the pressure sensor, which changed with the transition from VIZ to Vaisala radiosondes in 1990. However, pressure measurements are generally considered unproblematic, and the change in 1990 would not affect, for example, the comparison with HALOE.

(b) The NOAA FP ambient temperature measurement may be indeed subject to drifts similar to those found in systematic analyses of temperature trends from radiosondes. The HALOE temperature data in the area of interest in this study are provided by NCEP, which is also affected by temperature biases. It follows that both NOAA FP and HALOE potential temperature may have some drift. However, implications for water vapour trend estimates are probably not critical:

A drift of order 1K/decade in ambient temperature would imply a drift of order 2K/decade in potential temperature near 100hPa, and somewhat larger higher up. Over Boulder, and above 450K pot. temperature, the vertical gradient of water vapour mixing ratio is of order 1ppmv/200K. It follows that a potential drift in mixing ratios due to a drift in ambient temperature is of order 1/100ppmv per decade, much smaller than the variations and trends observed. The situation may be more critical in the layer 380-450K, where mixing ratios show a larger gradient due to the annual cycle in entry mixing ratios. A conservative estimate would be a gradient of order 2ppmv/50K, and a corresponding potential drift in mixing ratios of about 0.1ppmv/decade. This estimate is indeed in the range of the reported values. However, the problem is far less problematic for layer-averaged water vapour concentrations as done for several analyses in the paper. Figures 3/4 show results on single isentropic levels, and may be potentially most affected, but our trend results in potential temperature coordinates are very similar to those in geometric height (compare with Figure 1c) or pressure coordinates (compare with Figure 9 of Randel, but beware of differing periods).

To summarise, we do not find reason to believe that drifts in temperature measurements critically affect our results.

(102) Figure 2, overlaying data point is confusing. RESPONSE: Figure 2 now shows data points and running means separately.

(103) Statistical modelling. Use of statistical model with a 'drop' is not justified. RESPONSE: We agree that the data series are short, and fitting the data with an extended statistical model may be bold. However, the picture provided by the HALOE data convincingly argues for a 'drop' of entry mixing ratios in 2000/2001. Hence, one could also argue that a statistical analysis with a model that does not allow for a trend break is inadequate. It is common sense that for irregular oscillations with sufficient repetition frequency a linear trend model does a fair job, but if a timeseries shows a 'one time' change, a statistical model that allows for a break may be better. Now one can argue whether the observed low water vapour values are due to a 'one time event' over the period of measurements or not. We take the standpoint of Fuglistaler and Haynes (2005) that much of the observed variability is due to the QBO, but that Randel et al. (2006) make a convincing case that the low values since 2000 are due to changes in eddy-driven upwelling, and are unique at least over the HALOE period. The statistical model finds a 'drop' of -0.2ppmv for NOAA FP, which is statistically not significant, but the same drop in HALOE data is highly significant (-0.45, +/- 0.0008)ppmv. Hence we agree that from the NOAA FP data alone one could not make a strong case for a 'drop', but that the result is qualitatively consitent with the statistically highly significant drop in the HALOE data. We have added a statement clarifying this point.

(104) Comparison with model calculations, comment on outliers in Fig. 8. RESPONSE:

ACPD

7, S8365–S8373, 2008

Interactive Comment

It is virtually impossible to label a particular measurement as 'outlier', as it may indeed be an observation in a filament that has quite different water vapour concentrations than the mean concentration. Since only the NOAA FP has the necessary vertical resolution to resolve such smaller scale structures in the stratosphere, we have no way with the data used here to verify/falsify the existence of such filaments.

(105) Differences are large between model, FP and HALOE. Implications for uncertainties in FP data? RESPONSE: Having 2 observational and 1 modelled (which also relies on 'observed/assimilated' temperatures and winds with errors of their own) dataset makes it very hard to make a statement about the accuracy of any one dataset.

RESPONSE TO REVIEWER 2

(201) Vertical weighting function of HALOE. RESPONSE: We agree with the reviewer that applying the vertical averaging kernel of HALOE to the NOAA FP data may yields interesting results. However, such an analysis is unfortunately beyond the scope of this paper, but we include this suggestion into the outlook.

(202) Co-location of HALOE and NOAA FP. RESPONSE: The reviewer may have missed the statement on P14516/L25; the HALOE profiles are between 130W and 80W, i.e. 'near Boulder'.

(203) Cause of 0.5K uncertainty of frostpoint temperature. RESPONSE: The main source of uncertainty is the controller stability. This is described in detail by Voemel et al. (1995), and we have added a reference to that paper.

(204) Description of corrections. RESPONSE: Description of corrections applied to NOAA FP data is now in Section 2.1.1.

(205) P14519, L5: Not clear that analysis is done in separate layers. RESPONSE: Here, the data is interpolated onto isentropic surfaces. Text is slightly modified to make this point clear.

(206) P14519, L20: can you indicate the goodness of fit any better in the figures (to

ACPD

7, S8365–S8373, 2008

Interactive Comment

show that the proxies do not help at higher altitude). RESPONSE: We have moved the amplitude of the seasonal cycle to Figure 4, and Figure 3 now shows the standard deviation of the measurements and the fraction of explained variance by the regression model (R-square).

(207) P14521, L26: Does your model assume jumps? or new trends? How do you know it is a 'drop' rather than a trend reversal. Another sentence or two would help. RESPONSE: The statistical model allows for a break in 2001 *if* this yields a better solution (i.e. we allow for one additional degree of freedom in the model). As stated above, we adopt the perspective of Randel et al. (2006) that changes in upwelling caused a change in water vapour entry mixing ratios, and further assume that such a change is unique over the HALOE period.

(208) P14524, L 27: What is a 'systematic trend bias' of 2K/decade? That the trend should be that much larger or smaller? This does not seem to make sense to me. Please rephrase. RESPONSE: The sentence now reads '... as an indicator that the linear trend of the 'e' timeseries has a bias.'

(209) P14525, L5: Is there enough tropical GPS temp data for 10 years (97-2007) to take a stab at this using the GPS temperatures? This should not have a residual trend. RESPONSE: This is an interesting suggestion, but outside the scope of this paper that focuses on the NOAA FP water vapour measurements. We have added a sentence in the outlook that GPS data may provide soon a timeseries long enough to allow new conclusions.

(210) P14527: Appendix A: I would just fold this into the text. It is not too long and clearly describes the correction and points. RESPONSE: Done.

(211) P14541, Caption Fig 8: Replace '...shows a the model...' in 3rd line of caption. RESPONSE: Done.

RESPONSE TO REVIEWER 3

(300) Corrections should not be in Appendix.

RESPONSE: Done, now in Section 2.1.1.

(301) Screening process. What 'large oscillations' are too large? What exactly are the screening criteria for 'systematically lower values' during ascent? What level of mirror oscillations is too high? Also, are measurements which fail these screens clearly separated from the others, or are the exact screening levels somewhat arbitrary (as is, unfortunately, usually the case). Finally, why do these problems seem to preferentially affect soundings from 1997-2000? As far as this last question is concerned, even a statement saying something like: 'we investigated possible causes of these problems and were unable to find a clear cause'; would be better than nothing. I would think that 2 of the screens used (large oscillations and less water in descent than in ascent) could be just as easily done with the pre-1991 soundings with the chart recorder strips. If not, please explain why?

RESPONSE: Unfortunately, these strips no longer exist (now clarified in the text). The cause for the larger number of rejected measurements over the period 1997-2000 is indeed not clear, and we have added a statement to that effect. The choice of thresholds for rejecting a sounding admittedly bears a certain arbitrariness. We have added a few sentences to make this point clear, but also emphasize that the screening is applied based on a-priori information, and hence does not lead to a bias towards a subjectively chosen 'correct' water vapour concentration. We also found an error in the plotting routine for Figures 3 and 4 (old figure numbers) that led to a mix up of 'all data' and 'higher quality data' - see below.

(302) Is HALOE lower stratospheric data in June 1992 really aerosol contamination free? Is there a reference for this? Are the HALOE trends the same if you start a year later? RESPONSE: The choice of the starting date (July 1992) was motivated by the Randel et al. (2004) analysis. The timeseries over Boulder do not exhibit obvious anomalies, but it cannot be excluded that some Pinatubo aerosol contamination affects

ACPD

7, S8365–S8373, 2008

Interactive Comment

the HALOE profiles over Boulder in the second half of 1992. Removing or adding an additional year of data certainly changes the trends for these short timeseries, but the discrepancies between HALOE and NOAA FP are not an artefact of the start date July 1992.

(303) The phrase: ' Neither the QBO nor the equivalent latitude proxy shows a trend over the periods 1981-2006 or 1992-;2005, and cannot contribute to a trend in water vapour of these periods.'; is certainly not generally true. Getting oscillatory terms wrong can result in an incorrect trend (although this is, admittedly, probably not the case here). RESPONSE: Yes, exactly because of the problems associated with erroneous representation of oscillatory terms we carefully checked the QBO and equivalent latitude *proxy* for the Boulder FP and HALOE profiles near Boulder whether they have a trend, and found none. The sentence is slightly changed to make this point clear (that we refer to the behaviour of the proxy over Boulder, not QBO/equiv. latitude in general).

(304) 'Variability for the whole NOAA FP dataset (dotted lines) is slightly lower, which may be counter intuitive.'; Given the lower variability it is really not clear to me that a new screening is even justified. It would be good to find some kind of reason for this lower variability. E.g., are points being preferentially removed when the fit is generally good. The authors need to make every effort to assure the reader that their screen makes sense. Also, and somewhat contradictorily, I have to admit to being a bit surprised by the statement. Just from looking at the green datapoints in the figures it generally looks like they have more scatter. Maybe I'm missing something here.

RESPONSE: The reviewer is completely right - there was an error in the plotting routine that labelled the 'high quality data' as 'all data', and vice versa. (This problem affected Figures 3 and 4 of the original manuscript.) We are very sorry for this error. The plots are now corrected, and slightly modified (we now show R-square for the goodness of fit; and all amplitudes are shown in one figure). The text describing these figures is changed accordingly. Most importantly, the screening indeed leads to slightly *lower* variance in the data series, but the amplitudes of the fit are very similar. This suggests that the screening indeed removes some datapoints that may be 'noisier' than the rest, but that this 'noise' is purely random and the screening does not remove preferrentially datapoint that have a specific characteristic other than being 'noisier'.

(305) It seems to me that Figures 3c and 3d would go better with Figure 4, since these are all amplitudes. Also, it would be much nicer if the HALOE and FP lines could be on the same plot to help the reader to visually compare the results.

RESPONSE: We now show the standard deviations of the observations and 'R-square' of the statistical model in one plot, and all amplitudes in the second plot.

(306) The comment at the end of Section 4 in reference to the 2 FP datasets: 'the drop in 2001 is larger' is a bit strange. While it's okay to make a comment about the drop in the 'ALL' dataset, any comment about a drop in 2001 based on the HQ dataset doesn't even make sense to me, since the authors have removed almost all of the data in 1999 and 2000.

RESPONSE: We cannot fully follow the objections of the reviewer. The sparse data points in 1999/2000 certainly make it difficult to argue for a specific date of the change, but it is still true that observations before and after the 'drop' are systematically different. This point is much more obvious in the HALOE data shown by Randel et al. (2006), and we refer again to that publication.

(307) What is the dominant contribution to the change in Figure 8? Is it the change in entry level CH4, or in the stratospheric CH4? Or is there some other contributing factor here which I've missed? Is the stratospheric CH4 component calculated primarily from HALOE measurements?

RESPONSE: As stated in the manuscript, the contribution from methane oxidation is calculated from a (polynomial) fit through a tropospheric and a midlatitude stratospheric methane concentration timeseries (based on Rohs et al., 2006). No HALOE methane observations were used. We have indeed not separated the contributions of water

ACPD

7, S8365–S8373, 2008

Interactive Comment

vapour entry mixing ratios, changes in tropospheric CH4 and changes in fraction of oxidized methane in the stratosphere, and agree that such a separation should be done in future work. For the moment, we refer to the analysis of Fueglistaler and Haynes (2005) that performed a similar analysis and discuss some of these points.

It would be tempting to postulate that changes in tropospheric methane, and changes in the fraction of oxidized methane, can explain so and so much of the observed water vapour trend. Since the ccorrected NOAA FP data shows a smaller trend than the previously published data, the 'unexplained residual' trend would be smaller. However, we refrain from such a statement because - as emphasized in the manuscript - the results would differ much between HALOE and NOAA FP. Judgement which of the two timeseries is more reliable is beyond the scope of this paper.

(308) Also, it would be good to state explicitly already in 5.1 that the age spectrum has been kept constant (as is now stated in 5.2).

RESPONSE: Done.

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

ACPD

7, S8365–S8373, 2008

Interactive Comment