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Interactive comment on "Water vapour and ozone profiles in the midlatitude upper troposphere" *by* G. Vaughan et al.

G. Vaughan et al.

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Reply to referees' comments on paper ACPD-2004-si05009

Referee #1

1. General aspects: 'Summer and winter distributions of Rhi above saturation seem to differ .. if true more research should be addressed to clarify the origins of this difference'. Also Major comment 2: 'It is not probable that the summer/winter difference in the present data can be explained by a cloud bulge .. since the absence of such a bulge would imply the absence of cirrus over Aberystwyth in summer'. 'The authors should also examine whether this difference is statistically significant. How many data are represented by each bin in fig 6 and how large is the corresponding statistical

noise? .. If the difference is real .. (it should be) stated in an outlook paragraph at the end of the paper.'

We have removed the reference to cloud in section 5 and make the point in the conclusions that this observation requires further investigation.

The plots in figure 6 were constructed from a very large number of points: 12,000 for winter and 4800 for summer. The probablilities are calculated by simply counting the number of points in each RH bin. If we consider the points to be independent we can use Poisson statistics (sqrt N) to calculate a standard error in the number of points counted: this results in a standard error in the quoted probabilities (the numbers plotted in fig.6) of 0.2% in winter and 0.4% in summer. If we take the other extreme of considering all the points on each profile to be 100% correlated we can use the number of profiles (227 in winter and 96 in summer) to estimate an error of 0.5% in winter and 0.7% in summer. A paragraph has been added to the paper to make this point.

2. Major comment 1: 'Drop out section 4. Scientific results are meagre and the section is not needed for the rest of the paper'.

I disagree. The use of the ozone measurements was crucial to the selection algorithm applied to the water vapour data, and the paper would be incomplete without it.

3. Minor comment 3: 'what happens when the inlet shield devised for the SW becomes subject to icing?'

The SW flights were only launched into clear skies or medium/high cloud, to avoid icing. This point is now made at the end of the first para of section 3.

4. Minor comment 4: 'No independent information on the presence of clouds during the comparison flights. This could be stated explicitly for clarity reasons so that it is clear to the reader cloud presence had to be derived indirectly and with a great deal of uncertainty (since the threshold for homogeneous nucleation is $>$140%, why should a clear-sky layer with Rhi about 125% and thicker than 500 m not be possible?.

ACPD

4, S3815–S3818, 2004

Furthermore, ci clouds are usually thicker than 1 km.'

The point is now made that we had no independent measure of cloud. The criteria of Rhi $>$ 120 % and layer thickness $>$0.5 km were those we derived from profiles where the sensor was cleary contaminated (i.e. showed high RH in the stratosphere). We take the point that this is not necessarily a measure of cloud and have amended the text accordingly.

Referee #2

1. General remark: 'The age related contamination issue raised by Wang et al has not been addressed. RS80 sondes did not have the protective cover of more recent sondes and so were more suceptible to contamination. 'This issue needs to be addressed when applying the conclusions drawn from the SW comparison to the larger data set'.

Contamination is more of an issue for RS80H than the RS80A sondes used here. Wang et al quote a dry bias of ˜2% for 1 year old RS80A sondes but 10% for RS80H. Reference to this is now made in the paper.

2. Specific comment: 'Comparing the MOZAIC distribution for Rhics $>$ 100% and 275 mb $>$ p $>$ 175 mb with their data in the 6-8 km and 8-10 km layer does not appear to be a valid comparison.'

The paper makes quite clear the difference in altitude and our conclusion is that 'the values in Table 2 are not obviously iconsistent with the results at 250 mb'. The text has been amended to remove any impression that this is a direct comparison.

3. Specific comment: 'Fig 4 shows reasonable (not excellent) agreement on the mean behaviour between sensors. 10-13 km correlation plot shows a larger scatter which cannot be called excellent. The lines seem to be forced through (0,0). A linear regression would be more approporiate and the fit parameters could be shown in the figure.'

The 'excellent' was removed during the first refereeing stage and does not appear in

ACPD

4, S3815–S3818, 2004

the ACPD paper at all. Referee is presumably using the original submitted version.

The lines in fig.4 are not fits at all. They simply mark the 1:1 correspondence between the sensors. The figure caption now makes this clear. We do not consider that the Snow White/RS80 dataset warrants the use of linear regression: the overall question of a bias between the sensors is addressed quantitatively in Table 1.

4. Specific comment: 'Limits to which RS80-A humidity measurements can be considered reliable are somewhat inconsitent. A 50 ppmv limit .. would correspond more to a frost point temperature of -60°C and would imply that mesurements below -60°C should not be used. This would completely emiminate the altitude region 10-13 km. It is possible that the SW gives measurements to colder temperatures but this needs to be looked at very carefully.'

This is a fair point. In fact it bothered me as I was writing the paper but I couldn't see the way through at that time. The sentence referring to 50 ppmv at the end of section 4 has been removed as this is an over-simplistic interpretation of the data, and the reference to 50 ppmv in the following section (which is a sensitivity test) amended. We omit the statistical analysis in the 10-13 km range because the results were found to depend too much on criteria used to screen the data; this is not the same as saying that all RS80 data in this range are wrong. A sentence is now added to this effect.

5. Technical correction: (i) Current radiosonde production is the RS92. Noted

(ii) A-type polymer is faster than H-type, not slower Noted

(iii) Ref for Spichtinger has wrong page number. Corrected

(iv) Reference for Vance missing. Reference is there in my file (may be another problem corrected at initial review)

(v) p.10 para 5b) 50 ppbv not ppmv As iv

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 8357, 2004.