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

Anonymous Referee #1

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General aspects

The authors used a data set of 324 ozonesonde/RS80A profiles over Aberystwyth to derive statistics of the distributions of water vapour and ozone. The RS80A data needed correction after Milochevich before use. The functioning and reliability of the RS80A was tested by means of a set of comparison flights where it was flown together with an improved version of the Snow White frostpoint hygrometer.

Frequent occurrence of ice supersaturation was found in the UT. Summer and winter distributions of RHi above saturation seem to differ, but the reason for this difference is neither known nor becomes clear from the paper. If it is true, more research should be

addressed to clarification of the origin of the summer/winter difference (see below).

To my opinion the main contribution of the paper is to show that RS80A profiles can be used for climatological studies after the necessary corrections are applied. Thus it is a useful contribution to the topics of COST 723 and to its special ACP issue.

Nevertheless, I have some comments.

Major comments

1. I suggest to drop out section 4 “Correlation between water vapour and ozone” and Fig. 5. The scientific results of this section are at best meagre, and the section is not needed for the rest of the paper. Even the authors seem to share my view, at least there is no mention of the water vapour vs. ozone correlation in the abstract. Its content could be condensed into a few sentences, if included at all. I am sure that much more could be done with a water vapour vs. ozone study, but this would deserve a paper on its own.

2. In the discussion of Fig. 6, in particular the summer/winter differences in the distributions for the range 100 to 120%, the authors mention the cloud bulge in the corresponding distribution for Lindenberg (Spichtinger et al., 2003). It is not probable that the summer/winter difference in the present data can be explained by a cloud bulge, since then the absence of such a bulge in the summer distribution would imply the absence of cirrus over Aberystwyth in summer. I do not know whether cirrus is a rare phenomenon in Wales during summer. If this is the case, the authors should say it; otherwise the cloud bulge is not an explanation for the summer/winter difference.

The authors should also examine whether this difference is statistically significant. It is not clear to the reader how many data are represented by each bin in Fig. 6, and how large the corresponding statistical noise is. In each case, it would be good to know whether this difference is true. If it turns out true, then there is reason to investigate potential origins of the summer/winter difference and to see whether or not similar

ACPD

4, S3272–S3274, 2004

differences are present in other datasets, to what an extent, etc. This could be stated then in an outlook paragraph at the end of the paper.

Minor comments

3. What happens when the inlet shield devised for the Snow White becomes subject to icing?

4. Seemingly there was no independent information available 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 (Snow White RHi > 120% for more than 500 m) and with a good deal of uncertainty (since the threshold for homogeneous ice nucleation is > 140%, why should a clear sky layer with RHi about 125% and thicker than 500 m not be possible. Furthermore: cirrus clouds are usually thicker than 1 km).

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