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Interactive comment on "Evaluation of balloon and satellite water vapour measurements in theSouthern tropical UTLS during the HIBISCUS campaign" *by* N. Montoux et al.

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The authors combine balloon-borne in situ measurements of water vapour in the tropical tropopause region during the HIBISCUS campaign with observational data of several satellite instruments. Although the performed measurements are of large scientific value, they are not presented well in the paper. The approach of evaluating different types of water vapour measurements is diluted by the way they compare the data. The largest part of the comparison describes the different measurements relative to AIRS, which has been found unreliable at mixing ratios below 10 ppmv by Gettelman et al.(2004). Here, AIRS may have been selected as a reference due to its high spatial and temporal sampling, but the current presentation implies AIRS as a suitable reference in the stratosphere which very much confuses the picture. While the evalua-

tion of stratospheric H2O measurements clearly is of scientific importance, the applied method of comparison is not convincing. The comparison itself is very descriptive, and leads to no conclusion on which satellite instrument provides the most reliable data in the tropical UTLS region. The authors describe the performances of one instrument relative to another, but they do not give an evaluation. From the formal point of view, the paper is well structured with some minor linguistic corrections necessary. Overall, I recommend major revision of the manuscript with regard to the points mentioned below.

While balloon-borne in situ instruments have the advantage of high spatial resolution allowing the detection of strong gradients, they are locally very limited and surely not representative of e.g. a zonal mean of the tropical tropopause region. Instead, satellite instruments have the advantage of observing globally, but due to the lower spatial resolution they have problems to capture strong gradients, both vertically as e.g. the hygropause, and horizontally as e.g close to large convective systems. Furthermore, they are limited by the presence of clouds. From the scientific point of view, the two observing systems complement each other for the study of local events and small-scale processes as well as climatological questions. When combining them for the evaluation of the different instruments though, one should be well aware not only of the different observing techniques, but also of the fact that different air masses are observed. This mismatch is apparently responsible for the large differences between in situ and satellite instruments in the first approach, when comparing individual collocated profiles. The in situ measurements have been performed in the vicinity of active convection where humidity can vary by large amounts within short distances, while satellite instruments can hardly detect such sharp horizontal gradients. Additional limits occur due to the presence of supersaturation and clouds. In any case, when discussing single high resolution profiles, adding a temperature or frostpoint temperature profile would provide valuable information about e.g. tropopause height and supersaturated layers. Overall, I agree with the authors who state this part of their analysis as not very conclusive. To acknowledge the scientific value of the in situ measurements, I recommend

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to use them e.g. for the evaluation of the REPROBUS model. The REPROBUS model seems a strong tool to analyze small-scale structures as well as the global distribution of H2O. Although supersaturation is not represented in the model, a comparison with the in situ profiles may give insight to these processes. In the paper, the model is introduced but applied only for the evaluation of variability. As my major critique concerns the use of AIRS profiles as a reference, could model profiles instead be used as a neutral reference for the comparison of the satellite instruments ?

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6037, 2007.