Atmos. Chem. Phys. Discuss., 7, S1860–S1861, 2007
www.atmos-chem-phys-discuss.net/7/S1860/2007/


Atmospheric Chemistry and Physics Discussions



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

**Anonymous Referee #3**

Received and published: 17 May 2007

While the HIBISCUS in situ results are extremely interesting the authors have diverged from these results and over generalize their results and have produced a significantly flawed manuscript. I hope that they will work toward a more acceptable manuscript since the results are of interest to the community at large. In general, I find myself in close agreement with the the first reviewer and I will not repeat what would be very similar comments.

My biggest qualm with the manuscript is the use of AIRS as if it is fully usable in the stratosphere. The most recent review of its data quality rejected its usability at mixing ratios below 10 ppm (and hence pretty much everywhere in the stratosphere) yet the

authors use it as the fulcrum of their comparisons. This is a fatal mistake. If they wish to do AIRS validation and disprove the conclusions of Gettleman et al, this is their option. However, they have not done this adequately in this paper. AIRS in the stratosphere is mostly if not entirely the product of their a priori model. It would be nice if the AIRS group did not publish data where there is no science value; it would avoid problems such as this. I can understand the temptation to use AIRS since it provides sampling and opportunities for coincident comparisons. Since I believe that they should focus on the in situ measurements and comparisons with the space-based instruments. They could easily reference away the agreement between the satellite instruments since they have been compared ad naseum in the refereed literature.

It is certainly true that there is no spaceborne or (for that matter) in situ system for the measurement of water in the UTLS and the stratosphere in general and there are significant issues that need to be resolved. SAGE II results are, as the other reviewer pointed out, weakly correlated to HALOE since the latter's data was used to help solve an instrument response issue. In general, the dryness of the UTLS, aerosol clearing, and residual issues with the instrument response makes the use of SAGE II in the UTLS problematic. The correlation with HALOE has been overstated on some occasions but it is certainly worth noting when using SAGE II data. There are also significant issues with vertical resolution differences between the different instruments that the authors note and then ignore. One can hardly expect the instruments with 3-5 km resolution to observed a hygropause seen by instruments with much smaller vertical resolution.

There are some very minor grammatical issues that can be corrected in a new version.

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



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