Atmos. Chem. Phys. Discuss., 6, S3156–S3158, 2006
www.atmos-chem-phys-discuss.net/6/S3156/2006/


Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Mesoscale modelling of water vapour in the tropical UTLS: two case studies from the HIBISCUS campaign" *by* V. Marécal et al.

**Anonymous Referee #3**

Received and published: 17 September 2006

This paper reports an effort of using the mesoscale model BRAMS to simulate the water vapor distribution in the upper troposphere and lower stratosphere (UTLS). In order to evaluate the model's performance, two sets of balloon-borne measurements and also the results of ECMWF model that has comparatively coarse-resolution than BRAMS have been compared against the BRAMS results. In addition, several sensitivity simulations, formulated by either adopting simplified microphysical scheme in the BRAMS model or changing the resolutions of ECMWF model, have been carried out. The authors demonstrate that the BRAMS is able to reproduce to a certain degree the variability as well as the actual profiles of measured temperature and water vapor in the lower and middle TTL, attributed primarily to its fine resolution. Quite interestingly,

however, both models (even the better equipped BRAMS) still cannot explain water vapor behaviors revealed by the measurements particularly in the upper portion of the TTL and in the lower stratosphere.

This is an original result and I believe it should be very informative to the atmospheric research community. The paper appears to be well-organized and carefully prepared.

A major comment is that the authors did not discuss in the paper the modeled results of condensed phase at all. Providing such results would allow the reader to better understand why the models failed in reproducing the water vapor profile in the upper TTL and lower stratosphere. Ice particles would form and continually grow (should the supersaturation exist) along the upward moving path of the air parcel. On the other hand, these processes would lower the water vapor concentration while increasing temperature through latent heat release and thus form a restriction on the supersaturation inside the parcel. An overestimate in the ice water content by the model (I believe the model does not predict the concentration of aerosols) will lead to an overestimation in temperature and an underestimation in saturation status. In addition, the modeled profile of radiative flux is also useful information. I would like to suggest the authors to provide these results along with related in-depth discussions in their revised version of manuscript.

Specific Comments

Page 8245, line 1, “However, specific processes...sub-visible cirrus, may not be well captured by mesoscale models”: Why?

Page 8246, line 13: Delete “is” after “11.8 km where”.

Page 8250, section 4.1: The authors need to briefly describe the radiation schemes used in both models.

Page 8253, the last sentence, “The reason...”: What exactly is this “correlation”? Do the authors refer to the numerical filter? There should be advection between two adjacent



ACPD

6, S3156–S3158, 2006

grids. Other than that, the model would also allow turbulent mixing among grids. These are real processes in the atmosphere.

Page 8254, line 23 to the bottom of the page, “For the tropical UTLS...(green curve)...”: The derivation of the green curve described in the figure caption and later text seem not consistent with the explanation here.

Page 8255, line 9: Were the balloon data averaged using 1 km vertical interval in the comparison for this 1-km sensitivity run?

Page 8255, line 24-26: Why is the temperature result worse than the coarse resolution run?

Page 8261, line 19-20: Actually, the modeled result and TRMM data differ quite significantly in both pattern and strength (same in the SF2 case). The authors might want to further discuss this issue.

Page 8261, line 24-26, “The model fails to reproduce...due to the vertical correlation...”: Again, I don’t quite understand what exactly the authors are referred to by saying “vertical correlation” here? Should this be interpreted as that the model underestimates the vertical mass flux?

Page 8266, line 7-20: some of those sentences describing future experiments can be removed or at least simplified. The paper should only document the current results.

Tables: I’d suggest combining Table 1 and 3. The same can be done for Table 2 and 4.

Figures: Figure 2 and 7 need to be plotted together to allow the reader to compare the TRMM data and modeled results.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8241, 2006.



ACPD

6, S3156–S3158, 2006