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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.

**V. Marécal et al.**

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Answer to referre#3

Major comment We do agree that the microphysics and its interaction with the thermodynamics is an important point in the analysis of the results. For SF2, the model fails in reproducing the RHI profile in the upper TTL and lower stratosphere (approximately above 14.3km altitude). Even if there is supersaturation in the observations, the trajectory analysis shows that there is no saturation and no ice nucleation in the model above 14.3 km during the 12 hours preceding the flight. This information was given in Table 2. Therefore, the microphysics does not affect either the water vapour profile or the temperature profile in the model above 14.3 km. This is why there is no discussion in the paper concerning the microphysics above 14.3 km. Between 14.3 and ˜16 km altitude, this is mainly the temperature which leads to an erroneous RHI profile while above 16 km this is mainly the water vapour. This is illustrated by the solid green curve in Figure 6c giving the RHI calculated using the model rv and the observed temperature. Nevertheless, in the altitude range 10-13.5 km the model predicts well temperature, rv and RHI showing for this layer a good consistency between the dynamics, the thermodynamics and the microphysics in the model. In the 13.5-14.3km layer, rv is slightly overestimated, the temperature is significantly overestimated by the model (see Fig. 6) and there is some ice formation in the preceding hours. As shown by the solid green line in figure 6c, the temperature underestimation does not fully explain the model underestimation in the RHI profile. Therefore, as you suggested, in this case there is a possible interaction between the microphysics and the temperature leading to a modelled state close to ice saturation. The scenario could be that there is a decrease of rv due to ice condensation leading to an increase of temperature through latent heat release which restricts the supersaturation and the ice formation. This interpretation is included in the revised version (new section 4.4). For SF4, the trajectory analysis indicates that there is no ice nucleation above 14.2 km, i.e. in the TTL and lower stratosphere. Microphysical processes are only acting in the 11-14.2 km layer. In the original version of the paper it was explained how the microphysics is linked to the water vapour field in this layer: "The air mass sampled by SF4 in layer 1 comes from ascending air from the lower levels leading to a moistening during the hours preceding the flight. This moistening effect related to the dynamics competes with the removal of water vapour by ice nucleation and subsequent growth and sedimentation." This microphysical process becomes dominant in the model leading to a net decrease of water vapour mixing ratio above 12.6 km. There was an error in the original version: we wrote above 13.6 km instead of 12.6 km. In SF4, between 15.2 and 16.6 km the air comes from the convective area north of Bauru. This air is more humid in terms of relative humidity than below because previously (more than 12 hours) moistened by convection. But it is always undersaturated and there is no ice formed or evaporated within the 12 hours preceding the flight. Thus the microphysical processes do not act in this layer. The revised version will include more clearly that the air in this layer did not experienced saturation (new section 5.4).

Specific comments

P 8245, line 1 : This sentence does not reflect what we really meant. What we meant is that the mesoscale models commonly use a microphysical parameterization of bulk type in which only one type of small ice crystals is represented (pristine in BRAMS) whereas in reality cirrus, including sub-visible cirrus, are composed of a large variety of ice crystals. The manuscript was changed to make clearer this idea (section 1).

P 8246, line 13: We corrected the text.

P 8250, section 4.1: The radiation scheme used in BRAMS for longwave/shortwave radiation is from Harrington (1997). It is a two-stream scheme which interacts with liquid and ice hydrometeor size spectra. For ECMWF, the longwave radiation scheme (Morcrette et al. 1998) is based on the Rapid Radiation Transfer Model (Mlawer et al., 1997). It includes cloud effects using maximum-random overlap of effective cloud layers. The shortwave radiation scheme was originally developed by Fouquart and Bonnel (1980) and revised by Morcrette (1993). It takes into account the cloud properties for ice and liquid clouds. These brief descriptions of the radiation schemes in both models are now included in the revised manuscript (new sections 3.1 and 3.2).

Harrington, J. Y.: The effects of radiative and microphysical processes on simulated warm and transition season Arctic stratus, PhD Diss., Atmospheric Science Paper N° 637, Colorado State University, Department of Atmospheric Science, Fort Collins, CO 80523, 289 pp, 1997. Mlawer, E. J., Taubman, S. J., Brown, P. D., Iacono, M. J., and Clough, S.A., Radiative transfer for inhomogeneous atmospheres: RRTM, a validated correlated-k model for the longwave, J. Geophys. Res., 102D, 16,663-16,682, 1997. Morcrette, J.-J., Clough, S. A., Mlawer, E. J., and Iacono, M. J., Impact of a validated radiative transfer scheme, RRTM, on the ECMWF model climate and 10-day forecasts,



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ECMWF Technical Memo., N°252, 47PP, 1998. Fouquart, Y., and Bonnel, B., Computations of solar heating of the earth's atmosphere: a new parametrization, Breitr. Phus. Atmosph., 53, 35-62, 1980. Morcrette, J.-J., Revision of the clear-sky and cloud radiative properties in the ECMWF model, ECMWF newsletter, 61, 3-14, 1993.

P 8253, the last sentence: The explanation was not the right one: if the advection and the subgrid turbulence parameterizations were perfect we should be able to simulate the observed vertical variations of water vapour. After investigation, we now think that the small scale vertical variations in the observations are likely due to sub-grid isolated convective cells captured by the measurements but not by the model because of the low spatial resolution. This hypothesis is supported by the fact that the backward trajectories crossed an area in which there were several small convective cells of a few kilometres horizontal extension in the Bauru radar observations. This explanation is given in the revised version.

P 8254, line 23 to the bottom of the page. We do not think there is a problem with figure 6 and corresponding comment. In figure 6c, this is the relative humidity with respect to ice which is plotted in the horizontal axis. The green dashed curve corresponds to 100% saturation with respect to liquid water. Since the saturation pressure with respect to ice is lower than the saturation pressure with respect to liquid water, the 100% saturation curve with respect to liquid is greater than 100% RHI (relative humidity with respect to ice).

P 8255, line 9: Yes the balloon data are averaged using 1km intervals in the comparison for the 1km sensitivity run. This is clearly explained in the revised version (new section 4.3).

P 8255, line 24-26: A mistake which was found by referee#2. The RHI correlation and RHI RMSE values given in tables 1 and 3 for the BRAMS simulations were calculated using the observed temperatures instead of the modelled ones as we had intended to. The corrected results are deteriorated compared to the original ones mainly for



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SF2 for the runs using a 250 m vertical resolution. For SF4, the changes are small since the modelled temperature is close to the observations in this case. The new results still show that when using a 1 km vertical resolution for BRAMS (similar to the ECMWF vertical resolution), the BRAMS performs significantly better for RHI than the ECMWF analysis for both flights. These corrected results and associated comments will be updated in the new version of the paper. Using the correct statistics, we found that temperature results are only slightly worse for the reference run and for the 5-km run than for the 50-km run; the difference on the temperature RMSE is small: 0.04K and 0.12K respectively. The differences are found to be in the lowest part of the profile (below 8 km) and in the highest part of the profile (above 17.7 km). Below 8 km and above 17.7 km, the balloon was flown in regions associated with a large gradient of temperature. The small difference in the position of this gradient zone leads the difference in the temperature results between the coarse resolution run and the 5-km and reference runs.

P 8261, line 19-20: About the comparison with the TRMM rainfall rates, the model precipitation amount is obviously lower. But when making such a comparison, one has to take into account the uncertainty on the TRMM estimates which is particularly important over land. Nevertheless, the objective of this comparison is to check that the model is able to simulate well the location of convection. Referee#2 agrees with us and finds that the spatial pattern of the model rainrates agrees generally well with the observations. The text was changed to explain more clearly that the rain pattern is well reproduced but not the precipitation amount (new section 4.4 and 5.4).

P. 8261, line 24-26: The sharp minimum of water vapour observed comes from an intrusion of stratospheric extratropical air which started a few days before the flight. This intrusion has likely been smoothed during this period of time in the model by the turbulent mixing parametrization. This hypothesis is supported by the calculation of rv integrated between 8.3 and 11.3 km from micro-SDLA and from the model results. The 8.3-11.3 layer corresponds approximately to where the model simulates the dry layer.



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We find a difference of about 5%. Redoing the same calculation but changing the layer bottom and top by one grid point gives a difference of 9% and 4%, respectively. This means that there is approximately the same amount of water vapour in the dry layer in the observations and in the model but it is more diluted in the model. The text was modified accordingly.

P. 8266, line 7-20: Following your recommendation, this part is largely simplified in the revised version.

Tables: We prefered not merging tables 1 with table 3 in order to avoid confusion between SF2 and SF4 results. Following referee#2's remark, Tables 2 and 4 have been removed and replaced by two figures showing the results of the trajectory analysis.

Figures: Following your suggestion, we have now plotted figure 2 and 7 together.

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



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