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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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Response to referee#2 (M. Salzmann)

Specific comments

Thanks to you, we found a mistake in the calculation of the RHI correlation and RHI RMSE given in Tables 1 and 3. The RHI correlation and RHI RMSE values given in these tables were calculated using the observed temperatures instead of the modelled ones as we had intended to. The corrected results are similar to those you gave in your review (see tables below). They are deteriorated compared to the original ones mainly for SF2 for the runs using a 250 m vertical resolution. For SF4, the changes are small since the temperature is better simulated 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 are updated in the revised version of the paper.

New Table 1:

Temperature RMSE (K)– RHI correlation– RHI RMSE(%)

ECMWF analysis 1.69 0.905 26.5

ECMWF 48h forecast started on 12 Feb. 2004 at 00 UTC 1.84 0.827 36.4

Run with 1 km vertical Resolution 1.60 0.937 16.4

Reference run 1.76 0.850 26.0

Run with 50km horizontal Resolution 1.72 0.839 26.1

Run with 5 km resolution (2 grids) 1.84 0.842 26.9

Run with simplified microphysics 1.85 0.767 34.6

New Table 2:

Temperature RMSE (K)– RHI correlation– RHI RMSE(%)

ECMWF analysis 0.80 0.801 48.5

ECMWF 48h forecast started on 12 Feb. 2004 at 00 UTC 1.15 0.615 63.7

Run with 1 km vertical Resolution 1.19 0.716 38.4

Reference run 1.26 0.778 33.9

Run with 50km horizontal Resolution 1.29 0.769 35.1

Run with 5 km resolution (2 grids) 1.45 0.748 37.1

ACPD

6, S4677–S4681, 2006

Run with simplified microphysics 1.63 0.499 54.9

Concerning your remark on the fact that BRAMS profiles are smoother than the observations, we agree with you and we have included it in the new version of the paper (new sections 4.3 and 5.3).

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. Since the spatial pattern of the model rainrates agrees well with the observations, it is possible to use the trajectory method proposed by Freitas et al. (2000) which takes into account the subgrid effects of wet convection. The text was changed to explain more clearly that the rain pattern agrees well but not the precipitation amount (new sections 4.4 and 5.4). cas de 2001 plutot le contraire qui se passit

P. 8254, line 23: 100% saturation with respect to water. Yes it is an ad-hoc assumption. It is related to the microphysical scheme used in BRAMS in which the mixing ratios of water vapour and of cloud water are diagnosed from the mixing ratios of rain, cloud ice, snow, aggregates, graupel, hail and total water which are prognostics quantities (Walko et al., Atmospheric Research, 1995). The diagnostic is based on a threshold which is 100% saturation with respect to liquid water for any air temperature, allowing supersaturation with respect to ice up to this threshold. This will be mentioned in the new version of the paper (new section 4.3).

Trajectory analysis: Following your suggestion, we have included in the revised manuscript two figures illustrating the trajectory analysis for each case study. We have plotted the trajectories in lat/lon coordinates, the corresponding altitudes, water vapour mixing ratio and ice mixing ratio. We have removed Tables 2 and 4 which were no longer useful since the results given in these tables are now in the trajectory figures.

ACPD

6, S4677–S4681, 2006

Interactive Comment

For clarity, we have decided to use in the revised version three layers that are identified using the TTL base and TTL top determined from the model results. Layer 1 is located below the TTL base where the air has tropospheric characteristics. Layer 2 is the TTL and layer 3 is the lower stratosphere (above the TTL). This does not change the interpretation of the results and makes the text clearer in the revised version. In the ACPD paper, the TTL (Tropical Tropopause Layer) from the BRAMS model was compared to that obtained by Huret et al. (2006a) from micro-SDLA data. It was chosen to use the same definition as in Huret et al. (2006a) to determine the BRAMS TTL top. We found that the definition they used for the top of the TTL was wrong (the lapse rate was defined as /z instead of -/z). This is now corrected in Durry et al. (2006) and in the new version of our paper submitted to ACP. This does not change the interpretation of the results. Dire que huret et al remplacé par durry

P. 8253, line 28: 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. The advection scheme is the forward-upstream scheme (Tremback et al., MWR, 540-555,1987) for the velocity components and the standard leapfrog-type scheme for all other variables. The turbulence parametrization is from the Mellor and Yamada (Rev. Geophys. Space Phys., 20, 851-875, 1982) level 2.5 scheme which employs a prognostic turbulent kinetic energy. 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. 8255, line 28: The profiles from the run with a 5 km horizontal resolution are not shown because they are very similar to the reference run profiles with a bit more vari-

ACPD

6, S4677–S4681, 2006

Interactive Comment

ability. The comments on the 5 km and 50 km runs for SF2 have been changed since the corrected statistics does not show anymore significant changes with a finer or a coarser resolution (new sections 4.3 and 5.3).

P. 8264, line 18-24. We agree that the horizontal resolution does not have a very significant effect on the results. Nevertheless, it is difficult to change the order of the points by order of importance since: - the first sensitivity test (on the vertical resolution) is related to the analysis of ECMWF results which are discussed first in the paper. - the sensitivity tests on the horizontal resolution cannot be discussed before the reference run. This is why the sensitivity tests on the horizontal resolution appear at the end of the section although they produce similar results compared to the reference run.

P. 8244, line 25: We agree that, in principle, global models can be run with a fine vertical resolution and a detailed microphysical parameterization. In practice they are usually using a coarse vertical resolution and a simpler microphysical scheme compared to mesoscale models. We have changed the text so that we now say that global models generally use a coarse vertical resolution and a simple microphysical parameterization.

Your suggestion for changing the section order has been taken into account except for the titles “Trajectory analysis for SF2” and “Trajectory analysis for SF4” since the corresponding part does not only include the trajectory analysis. Thus the original titles were kept: “Analysis of the reference run for SF2” and “Analysis of the reference run for SF4”.

All technical corrections have been done. As for the figures 5, 6, 8, 9 and 10, we will make sure that they are big enough during the editorial process if published in ACP.

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