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Atmospheric
Chemistry
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Discussions

Interactive comment on "Regional modelling of tracer transport by tropical convection – Part 1: Sensitivity to convection parameterization" *by* J. Arteta et al.

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General comments: You pointed out the lack of quantitative results in the paper. This is in agreement with the other referee's review. This general remark has been taken into account in the revised version, in particular by providing more quantitative results for TRMM comparison (see details below), for radiosounding and aircraft comparisons and a more focused discussion.

Description of the model Boundary conditions: 3D-fields at the initial date/time for pressure, temperature, water vapour and horizontal wind come from ECMWF operational analysis. At the lateral boundaries of the domain a zero gradient condition is used for

inflow and outflow. On top of this, a nudging procedure is applied to constraint the model towards ECMWF 6-hourly operational analyses with a relaxation timescale of 1 hour. At the top of domain, we used a rigid lid with a high viscosity layer above 25 km altitude to damp gravity waves. Soil moisture initialisation is obtained by providing satellite TRMM precipitation estimates to a simple hydrological model (Gevaerd and Freitas, 2006). Sea surface temperatures (SSTs) are constrained using weekly SST analyses derived from satellite data on a 1°x1° grid.

Water vapour, microphysics and radiation: We use the one-moment bulk microphysics parameterization which includes cloud water, rain, pristine ice, snow, aggregates, graupel and hail (Walko et al. 1995). It includes prognostic equations for the mixing ratios of rain, of each ice categories and of total water and for the concentration of pristine ice. Water vapour mixing ratio is diagnosed from the prognostic variables using the saturation mixing ratio with respect to liquid water. Shallow convection is parameterized as described in Grell and Devenyi (2002). The parameterizations used for deep convection are presented in section 2.3. All radiative calculations were done with the Harrington (1997) scheme. It is a two-stream scheme which treats the interaction of three solar and five infrared bands with the model gases and with liquid and ice hydrometeors. Therefore, it is sensitive to changes in water vapour and hydrometeor spatial distributions linked to the behaviour of shallow and deep convection parameterizations.

As suggested we have modified the text in order to shorten the long sentences in the paper. We did our best to correct the spelling and grammar errors but since our native language is not English this is likely to be still not perfect.

PART 1:

TRMM: As you suggested, a more objective comparison of the model with TRMM rainfall has been done. We now use common measures for the precipitation forecast accuracy: the equitable threat score, the probability of detection and the false alarm ratio We have plotted the daily evolution of these measures (see Figure 5 in the revised version) and of the accumulated rainfall rates from both the model and TRMM to characterise the model behaviour as a function of time (see Figure 2 in the revised version). To evaluate quantitatively the monthly mean rainrate, we added a distribution plot of TRMM rainrates versus model (see Figure 4 in the revised version) in order to highlight strength and weakness of each closure for different rainrate regimes. These new results show that the different closures clearly provide results that can be sorted into two different groups (AS, KF and EN (group 1) and GR, LO and MC (group 2)). All of the closures model fairly well rainrates both on a daily and a monthly basis. They show small differences when the convective activity is weak and larger for active periods. The new material provided in the revised version clearly shows that the 6 closures provide similar convection triggering times and locations. The two groups mainly differ in the amount of total and convective (=produced by the convective parameterization) precipitation as shown by Figure 9. All these results are discussed in sections 3.1 and 3.4.

No comparison was done with the ECMWF driving fields since we used low spatial resolution ECMWF fields to force the model. Therefore the comparison would not be meaningful. This choice of low resolution ECMWF fields for forcing is to provide to the mesoscale only the large scale forcing. Nevertheless we have compared the dynamical fields produced by the 6 simulations. At the local scale significant differences are found within and in the vicinity of convection. But there are only small differences on average because convection is not trigger at all times and locations. This information is given in section 3.4 in the revised manuscript.

Station comparison: we agree that from the figures the results are very similar. We added a table in the revised version giving the statistical results (mean bias and standard deviation for temperature, wind speed and direction and specific humidity) for the two radiosounding stations (Manus and Darwin). It shows that for both stations, the differences between one closure/parameterization to another are much lower than the differences between the model and the measurements. Even at Manus the probability

that the radiosoundings launched sampled convectively perturbed air masses is fairly small since convection only occurs a small fraction of the time. This leads to a small probability for the model simulations to provide differences between each others.

Distinction between different convection parameterizations/closures: AS, GR, MC, LO and KF are all based on the same convection scheme, but with different closures. Only EN can be considered as a different parameterization. This is why when we speak of all the experiments except EN we use “closures” while when we speak about all the experiments we use “parameterizations” because it includes EN.

We agree that the final summary of the flight comparison is not very useful and we have removed it.

Link between the TRMM conclusion and the local (radiosounding and aircraft): The new TRMM results clearly show the six simulations generally trigger convection at the same locations and times but provide different surface rainrates. The only significant difference is found on the rain produced. Nevertheless local comparisons show larger differences in meteorological fields. But we feel that an analysis of the local results is out of the scope of the paper since we aim at evaluating regionally and on a timescale of one month the behaviour of the different closures/parameterizations.

Introduction of section 4. The “two types of behaviours” was unclear. The new introduction has been changed to: “The analysis of the results showed that the EN, AS and KF simulations (Group 1) provide results for the tracer transport that are very close. GR, LO and MC runs (Group 2) give very similar tracer results that are different from Group 1”. This separation in two groups is obvious on the TRMM comparison when using the new results and from figure 9 (revised paper) showing the ratio between total and convective (produced by the convection scheme) precipitation ($\sim$45% for Group 2 and $\sim$77% for Group 1). This is why the simulations shown and discussed hereafter are only EN and GR since they illustrate the Group 1 and Group 2 results, respectively”. “meridian mean”. The meridian mean is the average over the model latitudes (between 20°S and 20°N). This is explained in the revised version. Tracer fluxes: The fluxes are calculated a posteriori using the 3-hourly archived fields. For a given time, the tracer flux is calculated at each horizontal grid point at the chosen vertical level using the vertical wind and the tracer mixing ratio. The values given in table 4 are mean values determined by averaging for the whole model domain at a given altitude and for the whole 3-hourly outputs. This was explained in the caption of Table 4. To make it clearer in the revised version, this is also explained in the text. Large scale convergence: we have compared the large scale averaged convergence from the 6 simulations. We found only small differences although significant differences are found locally. This shows that the differences in the mean tracer transport are mainly linked to the convection closure/parameterization used. This is explained in the revised version Time evolution of the tracer differences: we compared the evolution of spatially integrated tracer mixing ratio differences during the simulation period. The results show more or less the same ratio between GR and EN. For tracer 1, maximum concentration remains at the same mean value while it increases with the infinite lifetime tracer, due to its accumulation in the atmosphere. Since these results are in accordance with the monthly average ones, they are not shown, but explained in the revised version. The conclusion of this experiment is that although there are small changes on average on the meteorological variables (except for surface rainrates) between the two groups the tropospheric tracer transport is very different. The EN and GR simulations provide different intensity of the upward convective flux leading to a more efficient uplift in EN simulation. This is consistent with the analysis of the rainrate results showing a more efficient production if precipitation in EN linked to stronger convective ascents. The conclusion as been revised accordingly Conclusion. We agree that the beginning of the conclusion was presented in a way that was not consistent with the introduction of the paper. It is changed in the revised version following your suggestion.

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do anything about it.

New figures are available in attached supplement zip file.

Please also note the Supplement to this comment.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5889, 2009.