

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

Anonymous Referee #2

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Preamble:

The review is for both parts of a two part paper. I will cover some general aspects of the two papers first and provide separate sections for each paper. The review is provided twice (for each individual paper) for convenience.

General comments:

Both papers are generally well written and cover a subject suitable for ACP(D). Part 1 investigates the sensitivity of a mesoscale model to differences in convective closure approximations and part 2 investigates issues relating to the models' resolution. Unfortunately both papers present very much qualitative results only and do not provide

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more quantitative or objective analyses of the model integrations performed. The model description provided in part 1 (and extensively referred to in part 2) is too sparse. Literature is cited for the different model components, but how the model was integrated in this specific case study remains unclear. What are the boundary conditions and how have they been applied? Is the model fully free running or constrained by some data assimilation/relaxation somewhere? How is water vapour treated? Is the changing water vapour feeding back on the radiative calculation and how much does this effect meteorology within the modelled domain? There is a quite generic sentence in section 2.2 (last sentence of the first paragraph), but this information is not enough! One minor point: Some sentences are on the long side, but I trust that the authors may find ways to shorten some sentences while trying to incorporate my comments below.

Part 1:

The discussion of the TRMM data in comparison with the model results is hard to follow. No quantitative measure is provided to justify the two proposed groups of the convective closure schemes. The differences discussed are hard to spot (at least on my printout). I would suggest to plot the differences between modelled and TRMM observed rainfall rates and to provide scatter plots or correlation measures to investigate how well the model captures the observations. In addition, the whole model characterisation should start with a more general statement of how the modelled meteorology compares to the driving ECMWF meteorology (temperature, winds, and e.g. the role of low level convergence and how it is modelled), before rainfall rates are discussed.

The station comparison was a surprise. Given the scales used, there seems to be very little difference between the models with different closures. In particular water vapour seems to be fairly unaffected. Is this a useful comparison? Another point: Maybe the statement of "using different convective parameterisations" is too strong - in principal it is the same scheme, but with different closures? The final summary for the flight comparison (p5904) is very generic and not useful. The authors should try to quantify to what extent the TRMM conclusions correspond to the flight track comparisons. In this

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respect it might be useful to make a much clearer distinction between the instantaneous behaviour of the model (at each time step) and the averaged monthly mean behaviour (TRMM).

Section 4 starts with talking about the "two types of behaviour" seen with the different closures. I think, I have a feeling for what those two types are, but it would certainly not hurt to define the types clearly (again). A handful of clarifications are required in the tracer section: Please define the "meridian mean" (p5906). How are the tracer fluxes diagnosed? Is there a change in large scale convergence that impacts on the general uplift in addition to the flux changes directly caused by the different closures (how interactive is the used system)? What is the time evolution of the tracer differences (is the average representative)? What are the conclusions from this experiment?

The conclusion has an unlucky start. The authors should start with an explanation on how they try to fill a gap in an established hierarchy of models. P5910: The authors need to explain what "better results" mean - this comes back to the point that their assessment is too qualitative.

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Part 2:

Much of the criticism collected above for part 1 applies to part 2 as well. Unfortunately the paper does not motivate well what it is trying to do. The model description is too thin (partly because the reference is not really there, see comment to part 1) and for much of the analyses quantitative measures are not derived. Many other questions remain: Have the flight tracks been chosen for any particular reason? Why are the model differences small compared to the difference with observations (the authors have a brief discussion of this, but more is needed)?

The statistical analysis is rather poor and not well explained. Even though I can understand a mean bias I am not sure what the authors mean when they refer to a "mean

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bias standard deviation" - is this supposed to be their flavour of "RMSE"? Are the differences in table 3 significant and how do they rate the models based on the numbers provided? Much more explanation and discussion is required here.

Figure caption 5 is wrong. The discussion of tracers here (under different resolutions) does not add that much to the previous discussion in part 1, posing a question if a two part paper is the right choice (given that TRMM results are repeated as well). Again the conclusions are very generic (e.g. "positive impact"). I am not quite sure what to suggest. Maybe it would be possible to merge the papers? Part 1 is certainly already stronger but requires work, part 2 is currently quite weak and does not have a clear direction or conclusion. A more quantitative analysis of the tracer behaviour would help.

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

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