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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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This paper presents some interesting model measurement comparisons. However, the paper suffers from trying to do too much, and therefore lacks a single core argument or strong conclusion. It would have been better to either focus the paper on much more rigorous statistical tests on the meteorological (rainfall + temperature + wind) predictability of the various convective closures, or on the mass transport aspects of their closures (STE, tracers, etc.). Though worthy, the paper lacks focus, and easily identifiable strong, clear conclusions.

Comparison with TRMM rainfall rates: The model is run for a month with six different convective closures. The mean rainfall of each model run is then compared with

TRMM. There are two problems with these comparisons. First there is no objective statistical measure of the success of the various models in reproducing mean TRMM rainfall. The six closures are simply put into two groups based on a qualitative comparison of the modeled monthly mean rainfall maps with those from TRMM. It would have been much better to show scatter-plots of modeled versus observed rainfall, and calculate correlation coefficients, or introduce some other statistical measure of model performance. Second, it would have been very interesting to examine model performance in forecasting rainfall at shorter timescales. This would have been a tougher, more interesting, and very different test of the convective parameterizations.

Radiosonde Comparisons: I again think it would be much more interesting to compare each radiosonde profiles with interpolated (in space and time) model profiles, and to introduce objective statistical measures of model performance, rather than just doing monthly means. Shouldn't this be part of the purpose of forcing a model by "real" boundary conditions? On page 5904, line 19, the paper reads, "From these differences it is not possible to get guidance on which convective parameterization performs better." I think this is likely due to a lack of statistical intercomparisons in the paper.

top page 5897, line 3: does the "Grell framework" refer to the ensemble (EN) approach or to the 1991 paper of allowing various closure assumptions.

page 5899, line 18: does "stratiform precipitation" refer to precipitation not produced by the convective scheme, or to that produced by the rainy stratiform anvil? How do you know that EN simulation underestimates stratiform precipitation? Note that extended regions of weak precipitation in the tropics are probably produced by cumulus congestus clouds (and is therefore convective), NOT rainfall associated with rainy stratiform anvil clouds. The meaning of stratiform should be clarified.

page 5902, line 16: why examine only one flight of the Falcon? It would seem much more constructive to examine the overall biases (granted that the flights in the immediate vicinity of Hector may be difficult to compare with the models), and to develop

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better statistical performance indicators.

CNRS logo: I would advise the editors of APCD to prohibit papers from including logos from sponsoring organizations, effectively allowing these organizations to insert graphic advertisements into scientific journals. This undermines the credibility of APCD, and may make referees less willing to review articles for APCD in the future.

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