

***Interactive comment on* “Detailed cloud resolving model simulations of the impacts of Saharan air layer dust on tropical deep convection – Part 1: Dust acts as ice nuclei” by W. Gong et al.**

Anonymous Referee #2

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Overall comments

The paper uses a numerical model to explore the effects of dust on the microphysical properties of a single cloud for short duration (100 min) and concludes that the dust has a significant effect on the amount of rain, distribution of rain (between convective and stratiform), cloud top height, and size of ice particles.

The trends in these parameters seem to be consistent with our conceptual view of dust effects on deep convective clouds based on previously published work. But the study is inherently flawed by its reliance on a single cloud, short-duration simulation and therefore the conclusions are much too strong and border on being fanciful. It will

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need a significant amount of work for it to be worthy of publication.

The work of van den Heever and Cotton has shown that dust signatures flip-flop with time and that a brief simulation is of little relevance. Strong conclusions cannot possibly be drawn from a brief model run like this.

It is unclear to me why the authors did not run their simulation for far longer (days), as for example is done for GCSS simulations (e.g. TWP-ICE; Fridlind et al. 2009).

Further comments:

1) You must run longer runs and let secondary convection form. Otherwise the simulations and comparisons are not statistically meaningful.

2) With a strong initial temperature perturbation, these simulations never forget the initial forcing and are therefore not realistic representations of natural rainfall. Even more reason to run for much longer.

3) Frequent reference is made to earlier observational work by Min and coworkers in support of the arguments. This is strongly overstated. You should show your model results and then use a discussion section to draw parallels with the appropriate caveats. You are, after all, comparing 100 min model runs with observations that come from different environments. Overstating the case weakens the paper. (E.g., the "profound implications" - conclusions). In addition, you should bear in mind the huge uncertainties in ice-forming mechanisms and the way they are incorporated in the model.

4) Conclusions: Climate models cannot possibly address these issues because they don't resolve convection.

5) The title implies that this is part 1 of a multi-part series. All the more reason to take care of the main issues listed above before pursuing this research further. The title is also not very informative, and even misleading. If this were about impact of the Saharan air layer, it would also include dry Saharan air along with the dust and yet the dusty simulations have exactly the same initial conditions as the dust-free simulations.

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Comment

6) The paper needs a great deal of work from the perspective of English usage and grammar. While I appreciate that some of the authors are not English speakers, I assume that some are. You would be doing yourselves (and the reviewers) a great service by correcting these issues before submission. I had to read the abstract a few times and even then had a hard time understanding the main points.

Minor points:

1) What is spectral bin? This is very strange usage. I think you mean "bin" or "sectional". "Spectral" implies that you resolve a spectrum of sizes, but that is what a bin method does. Spectral also has the connotation of light spectrum or power spectrum. Confusing!

2) Fig. 1 would be much more useful if you showed theta (or theta) and mixing ratio q_v (g/kg). Fig. 4 has fonts that are so small I can't read them.

3) Your labeling on Fig 15 (DF, DS) is wrong. What does it mean?

4) Your usage of "domain" is non standard and confusing. You are using a subdomain.

5) Do you really trust aerosol measurements to this accuracy (108.5 /cc and 87.32/cc)??

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12907, 2010.

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