Atmos. Chem. Phys. Discuss., 10, C7793–C7797, 2010
www.atmos-chem-phys-discuss.net/10/C7793/2010/

Atmospheric Chemistry and Physics Discussions

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.

W. Gong et al.

wgong@climate.cestm.albany.edu

Received and published: 21 September 2010

We would like to thank the reviewer for his/her very important and constructive comments. We adopted many of his/her ideas and suggestions in order to improve our paper. However, we believe that some of the comments might reflect misinterpretation of the basic objectives of our research. Those points will be discussed below. Below is our item by item response to the reviewer's comments and suggestions with a detailed description how we addressed them. For convenience, the original comments of the reviewer are repeated in italic.

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 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 there simulation for far longer (days), as for example is done for GCSS simulations (e.g. TWP-ICE; Fridlind et al. 2009).

We agree with the reviewer that utilizing a detailed bin-microphysics for analyzing a single cloud is not sufficient for obtaining final conclusions on the impact of mineral dust particles on cloud development processes and precipitation. There are other factors that play a role such as the dynamics, land-atmosphere interaction, radiation and cloud chemistry, which our 2-d model does not handle. Moreover, the longer run, even for a 3-d model, is facing challenges in at least two aspects. First, how to set up the time-varying tracer configuration to represent the realistic scenario, in addition to the initial condition? Second, how to recycle the tracer and to include follow-up effects of sublimation/evaporation on the convections? The latter question becomes complicated when considering that right now it is difficult to trace whether a ice particle is formed by heterogeneous process related to IN or by homogeneous process in which IN do not play any role. Therefore, as stated in the first paragraph of the abstract, in this study we explicitly studied the dust-cloud interactions in the tropical deep convection, focusing on the dust role as Ice Nuclei (IN). We also stated at the Introduction that our objectives were to separate influences of dynamical and cloud microphysics and to provide detailed information for understanding the mechanisms how mineral dust impacts the

ACPD

10, C7793–C7797, 2010

tropical deep convective clouds by using the model outputs, including all cloud condensates and tendencies due to all physical processes at various spatial, time and size scales. We believe that at this stage separating microphysical and thermodynamical effects will help us to easily understand how the dust influence the convection through microphysical processes, and lay a solid foundation for our future studies in dealing with a more completed aerosol-cloud interaction in terms of microphysical-dynamical processes. It has also been stated in the manuscript that for carrying out this research we made major improvements in the representation of ice nucleation to account for the present of mineral dust particles. These improvements enable us to conclude about the relative contributions of different mixed-phase microphysical processes such as drop freezing or homogenous and heterogeneous ice nucleation to convection and precipitation. By the attempt to follow and microphysical processes, our research becomes complementary to Min et al. (2009) but it does not try to reproduce the case discussed in Min et al. (2009) paper. In order to reflect the deficiencies of our approach and to be more unbiased in our arguments we modified few of the conclusions of our study. Those changes are also quoted in this response letter.

Further comments: 1) You must run longer runs and let secondary convection form. Otherwise the simulations and comparisons are not statistically meaningful. As a follow up to our previously mentioned arguments, the benefits of simulating the case for very long time so secondary clouds will form are balanced by the difficulties to gain any meaningful information about the interaction between the dust particles and the clouds. Following the lessons learned in this study, we will apply the new schemes in a more sophisticated tool (WRF) and the results will be reported in the second part of this set of papers.

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. Further to this comment any reader can also claim that in any initial conditions used the simulation is not realistic because the model

ACPD

10, C7793–C7797, 2010

is two-dimensional. Our study is aimed at studying sensitivity of cloud microphysical processes to dust loading. It is certainly not intended to predict exactly how much rain will be produced in reality. However, the cloud and other atmospheric properties during the simulation time are reasonable and enable us to draw some conclusions about the investigated sensitivities. For instance, the 15 km cloud top height and 22 mm/hour (Fig. 10a) surface precipitation is in agreement with the 15 km and 15 mm/hour of satellite retrievals in SAL region, respectively.

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. We agree with the reviewer. References and comparison with the Min et al. (2009) paper were moved to the discussion

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

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. We replaced the title with the following one: The effect of mineral dust on tropical deep convection – A numerical modeling study using a Detailed Cloud Resolving Model

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

ACPD
10, C7793–C7797, 2010

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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. We carried out significant changes in grammar.

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! We removed the word “spectral”

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

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

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

5) Do you really trust aerosol measurements to this accuracy (108.5 /cc and 87.32/cc)?? We fully agree and numbers were rounded to 110 /cc and 85 /cc

---

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

ACPD

10, C7793–C7797, 2010

Interactive Comment