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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating the Impacts of Saharan Dust on Tropical Deep Convection Using Spectral Bin Microphysics, Part 1: Ice Formation and Cloud Properties" *by* Matthew Gibbons et al.

Anonymous Referee #1

Received and published: 28 June 2016

General Comments: This paper investigates the impact of dust acting as IN on tropical convection – specifically its impacts on ice nucleation and particle size distributions - using numerical simulations. The impacts of dust on tropical convection are still not well understood and studies such as this one are needed. Some aspects of the analysis and discussion need clarification, particularly regarding Figure 7 and some of the physical reasoning for the differences seen in the simulations. These are detailed below.

Specific Comments: 1. Page 2, Line 10: It is confusing to state the increased condensation results from greater droplet nucleation since those are two separate processes.

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More accurately, the higher droplet concentrations induce the greater condensation and heat release.

2. Page 5, Lines 12-17: Hasn't the additional IN prognostic variable been available in the SBM for a number of years now?

3. Page 6, Line 10: Why is the simplest homogeneous nucleation option being used? Presumably the other options represent homogeneous nucleation more accurately.

4. Page 8, Line 20: If the authors feel that our ability to represent ice nucleation in models is limited and poor, how do we know that the results of this study are meaningful and applicable to the real world?

5. Page 10, Lines 20-25: I found this part to be confusing. Are the authors simply trying to state that differences in simulated cloud properties are entirely due to dust impacts? I think it is a given that the environment and sea surface are initially the same and therefore do no contribute to differences in the clouds.

6. Page 11, Line 9: Max updraft speed or mean updraft speed?

7. Page 11, Lines 14-to end of page: The logic here is circular. First the authors state that the weaker updrafts limit ice nucleation (also, why is this the case?), but aren't the changes in nucleation ultimately the reason for the weaker updrafts?

8. Page 12, Line 12: What is meant by "three per unit magnitude increase"?

9. Page 13, Line 13: What is meant be "increases up to 30%, proportionally to IN number"?

10. Page 13, Line 25-26: Does this ratio appear in your simulations?

11. Page 14 and Figure 7: I don't understand what is shown in Figure 7. The authors state it is the difference in location before and after gravitational sedimentation. Are both of these fields output by the model? The titles on the figures say "number flux" but the units are 1/L which is not a flux. Lastly, the text suggests that the authors examine

this quantity in order to understand how particles are transported, but are the particles not also transported by the winds? And thus this figure does not really tell us where the regions of formation are?

12. The authors distinguish between heterogeneous and homogeneous ice throughout the paper. This is based just on the air temperature? But it is possible for heterogeneously nucleated ice to be transported higher in the atmosphere where homogeneous nucleation is dominant, yes? And likewise homogeneously nucleated ice can fall to lower levels. It seems to me that the two types of nucleated ice can't be easily distinguished and that the labels are perhaps misleading.

13. In Figures 5 and 6, I understand why showing values on a log scale is useful, but I don't understand why the authors add 10 – this just makes the values more difficult to interpret. Also, it is worth pointing out that in the difference of two log10 values is the log10 value of the ratio, i.e. log(x)-log(y)=log(x/y), in order to give more physical meaning to these plots.

14. The changes in relative importance of nucleation mechanisms is interesting. I would suggest moving this discussion to earlier in the results section. Changes in nucleation is the first step in the chain of events that lead to the changes in cloud properties, so it seems natural to include this discussion first rather than last in the results section.

15. There have been several studies examining the impacts of dust on tropical convection, particularly in hurricanes, yet in general only those studies by Min et al. are cited. Better citation of other relevant literature is needed.

16. The lower cloud top heights despite stronger updrafts is a bit confusing, though the authors do give some reasons. I'm wondering if the cloud tops are lower only in the stratiform regions, whereas the strongest updrafts are of course in the convective cores? Perhaps the cloud tops in the convective cores are more similar?

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