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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 #2

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General Comments:

The manuscript addresses several interesting hypotheses based on observations from a group of papers [Min et al. (2009), Min and Li (2010), and Li and Min (2010)] regarding the impacts of dust particles on the characteristics of tropical deep convection via nucleation pathways. Using a suite of simulations that vary the dust concentrations and the hydrometeor activation pathways for one specific case from these observational papers, the authors assess the impact of dust aerosol on ice and cloud nucleation, and subsequent microphysics. The topic of this paper makes it suitable for ACP, and the

use of spectral bin microphysics make it unique, however, there are numerous shortfalls in the manuscript that currently make it unsuitable for publication in ACP, including (1) a lack of analysis and/or justification for a number of the statements made; (2) the absence of number relevant citations as well as placing the current research within the context of past work; (3) little to no demonstration regarding how well the case study output compares with the observations; (4) issues related to the convective-stratiform partitioning and the convective thresholds; (5) little to no substantiated physical reasoning in the role of the dynamics; and (6) poor figure quality. These are outlined in detail below.

Introduction and References

1) P1 L27: In addition to convective intensity, aerosols have also been found to influence the anvil characteristics as described in papers by Fan et al (2007) and a recent paper by Saleeby et al (2016).

2) P2 L4-19: The authors have cited a lot of their own work, which is fine, however, I would encourage them to cite other relevant work here too. For example, there are numerous other observational and modeling papers examining the impacts of Saharan dust on deep convective systems including MCSs and hurricanes that are relevant to this work that have not been cited including Dunion and Velden (2004), Evan et al (2006), Lau et al (2009), Zhang et al (2009), Braun (2010), Carrio and Cotton (2011), Cotton et al (2012), Storer and Van Den Heever (2013) and Storer et al (2014). Some of these references are also relevant to the statements made on page 3.

3) P2 L12: There are numerous observational and modeling papers that have shown that cold pools are warmer in more polluted environments (Altaratz et al. 2007, Berg et al. 2008, Storer et al 2010, Lim et al. 2011, May et al. 2011, Morrison 2012, Grant and Van den Heever, 2015). These papers argue that raindrops are larger under more polluted conditions due to greater rain accretion of the higher cloud water contents produced in more polluted conditions. Please be sure to include this argument too in

your description of cold pool impacts.

4) P2 L21: It should be notes that dust can serve as CCN and/or IN as shown by various papers by Twohy et al (2009 and others)

5) P2 L23: The paper by Van den Heever et al (2006) which the authors cite earlier, was one of the first to investigate the impacts of both CCN and IN on DCCs and should be cited here.

6) P2 L26: While these are all fine references cited here, these papers did not "discover" heterogeneous nucleation per se. The authors should go back to the original statements on these processes, or alternatively state what the papers referenced here added to the field of heterogeneous nucleation.

7) P3 L24-25: The past research of Koren et al (2005, 2009) and Wall et al (2014) also utilized observations to examine convective invigoration in response to dust across the globe.

Experiment Setup

1) P5 L12-17: "sources of dust (from lateral boundaries)" – are the lateral boundaries the only sources of dust in this model setup? Please clarify. Also, it is stated that "such a layer has been added". How was this later added? What are the characteristics of this layer? Finally, how is the fractional combination of dust serving as CCN and IN parameterized? And what was it set to in these simulations?

2) P6, L10: Why did the authors choose to use the Hthr homogenous nucleation method over the other two methods described? Furthermore, what is the point of describing the H&M method if it is simply an addition to the model that was not utilized in this study?

3) P6 L20-21 (and later P8 L20): The authors state "Deposition nucleation should be important for deep convection, but there is no such a scheme developed for deep convection with a connection with aerosols." They then go on to state in the same

paragraph "To link depositional / condensational freezing with aerosols, we follow the implementation of van den Heever et al (2006), update from the Meyers et al (1992) version." It would thus seem that such a scheme has been developed before. Furthermore, from what I recall, such a scheme does exist in the RAMS model used in the Van den Heever study. Do the authors mean that no such scheme exists in this version of the SBM? Please clarify this in the text.

4) P6, L24: Please specify temperature range.

5) P6-P7, Eq (1-2): Are Nid and NIN the same variable? Can you please clarify?

6) P7-8, Eq (4-7): Is the explanation of how NCNT is calculated necessary if the authors are just keeping it set at a constant value?

7) P8, Eq (7): State what "r" stands for in Eq 7.

8) P8, L27: What is the grid spacing for the 41 vertical levels in the simulations? Also, 3km and 41 vertical levels seems relatively coarse for resolving the updrafts and cloud processes of MCSs. Have the authors tested the sensitivity to grid resolution in any way? This could be tested with a less costly bulk scheme to ensure that enhancing grid resolution doesn't have a significant impact on the case study results.

9) P9, L3: While the large scale precipitable water patterns are relatively well produced, the magnitudes are quite different. This could have a significant impact on both the amount of liquid water and ice formed by the model. Please can the authors comment on this?

10) P16, L2: Adding 12 CCN cm-3 to the dust layer for case D1.2c seems like a somewhat arbitrary number, especially given that the numbers measured were more than double this ($\sim$30/cc) as stated in the paper. Why not add 30/cc? Also, surely a more correct approach here is not to add more CCN, but rather attribute some portion of the initial dust population to CCN and some to IN, and hence remove the number that are allowed to act as CCN from the number that were allowed to operate as IN in the first

set of simulations? In the current approach, the authors have simply added more dust particles to the environment. This does not appear to represent the problem correctly, in that when dust can act as CCN or IN, when they operate as CCN this prevents them from acting as IN (unless through immersion freezing) as they are typically rained out. In the current approach, the simulations are simply being made more polluted, and hence the effects of greater dust concentrations, as opposed to a different partitioning between CCN and IN, is being investigated. Please will the authors comment on this approach?

Case Study Evaluation

1) P9, L3: The authors motivate this study and discuss the results of this study based on a suite of observational studies by Min et al. (2009), Li and Min, (2010), and Min and Li (2010). However, there is not enough analysis presented in this study to demonstrate whether the simulations are properly reproducing the MCS event on 08 March 2004. The only comparison to observations is a precipitable water comparison, which is averaged over a 3-day period. While this does provide some information about the large-scale environment, it does not provide enough information on whether the convective elements and environment are similar to those observed and discussed in the observations studies listed above. Many studies have shown that aerosol effects on convection may depend on the convective system structure and environmental conditions, some of which are cited in this document. Therefore, the authors should include additional comparison of the simulations with observations in terms of the environment and convective system structure, especially since the authors are trying to explain an observed phenomenon represented by these simulations. Given that the observations from this case study have been published by some of the authors, it seems reasonable to compare some of the observations put forth in these prior studies to the same values in the simulations.

2) Pg line 11: The authors state that dust number concentrations were enhanced within the layer between 1 and 3 km. What did the authors do for the regions not included

within the SAL? What were these concentrations set to? The SAL would not realistically extend across the entire region captured by their grid 1. Also, the authors state that these concentrations are those for IN. Can these particles also serve as CCN? The authors refer to a relative combination of serving as CCN and/or IN earlier in the paper, however, that is not indicated here.

3) P9 line 12: There seems to be an error in one of these case study names as two of them are the same.

4) P9, L24-26: The SAL is typically a dry layer, with layers that tend to be moister below and above the SAL. Why is the dryness of SAL not represented here? Also, what data are used to provide the initial environmental data, as well as the boundary conditions?

Scientific Analysis, Reasoning, Clarity and Conclusions

1) P1 L15: "Ice particle size distributions" – which ice particles are the authors referring to? Smaller ice, larger ice, all of the ice species? As ice can refer collectively to small and large ice species, this should be clarified.

2) P1 L20: "which is consistent with observations" – which observations are these? Do the authors mean observations of the current case? Or to observations more generally. If the latter, other observations have not always found this to be the case.

3) P2 L13-16: There are various definitions of convective invigoration ranging from stronger updrafts, to higher cloud tops, to more precipitation production to impacts on latent heating, with some of these being related. I would recommend that the authors make clear what they mean by convective invigoration, thermodynamic invigoration etc.

4) P4 L9: Do note that some bulk schemes diagnose the shape parameter.

5) P10, L1-3: The authors partition precipitating columns into convective and stratiform regions, using somewhat arbitrary thresholds for vertical motions and cloud depth. Furthermore, there seems like there may be a few loopholes in their classification. For example, if a column has cirrus clouds in the upper troposphere, but precipitating, shallow

warm-phase clouds below it, it seems like these would be classified as stratiform. Is this correct? If not, and these are classified as warm clouds, then the upper level cirrus would be excluded from their analysis. An example of such an instance can be seen in Figure 2, north of the equator. Given that this stratiform-convective partitioning is relied upon throughout the manuscript, additional analysis and plots would strengthen this study significantly. For example, providing plan view of the convective/stratiform partitioning and radar reflectivity within the domain may alleviate some concerns about the partitioning methods.

6) P10, L8: What is the longitude and time of the cross sections shown in Figure 2? And how do these cross-sections relate to one another? Are they at the same times in the simulations? Are we comparing the same stage of storm development, and are we comparing similar regions within the storm? How were these cross-sections chosen? Without this information, these C/S could be arbitrarily chosen rendering such comparisons meaningless.

7) P10, L19-20: "... the total cloud mass transported into the anvil regions is reduced and the stratiform height is lowered." Is this conclusion being drawn from the C/S? One cannot refer to the total cloud mass when only looking at a C/S. This statement would be better supported by integrating over the anvil area and depths and comparing times series of these masses. Only then is such a statement supported by the output.

8) P10, L24-25: The clouds occurring at the same model time in the same model region with similar SST values does not rule out that differences in dynamics may be impacting differences in cloud structure. For example, what about changes to low-level moisture and cold pool development? This is one of many instances throughout this document where the authors make statements about relative roles of dynamics and microphysics without providing a complete explanation and/or justification of their statement. A similar instance occurs on P10, L28-33. While such statements may be correct, no evidence is provided that clearly supports these comments.

9) P11, L4-5: The authors separate the simulation into periods of strong convection (Hr 10-20) versus weak convection (Hr 21-30). It is unclear how this partitioning was conducted, especially since the maximum vertical velocity continues to be very intense through Hr 23 (Fig 3f). A great deal of analysis throughout the document is based on this partitioning, so additional analysis and/or discussion should be provided to demonstrate that this partitioning is both objectively determined and representative in all cases. Plan views of maximum vertical velocity or reflectivity in the inner domain during both the weak and strong convection times may be useful in helping the reader understand the differences in these regimes. This would also make subsequent discussion of these weak and strong convection regimes much clearer.

10) P11, L5-6: Increasing dust concentrations does not seem to depict lowering cloud field in the D.12 case (Figure 3b). Can you further clarify this statement? Also, it seems like the maximum updrafts in D1.2 are weaker than those in D.12 between 18 and 24 hours, rather than stronger (although the figures are very small so it is difficult to clearly compare the times). Can the authors please comment?

11) P11, L9-10: "The corresponding changes in updraft speed for this period are ..." What do these values relate to? This is just one number and yet the plots provided are time series of maximum updraft speed. Please clarify.

12) P11, L15-15: "To illustrate this, ... case". This statement needs further clarification. Heterogeneous ice number is not very similar between the D1.2 and D12 cases, as shown in Table 3.

13) Can the authors specify what is meant by heterogeneous and homogenous ice, drop, and graupel number in Table 3?

14) P12, Line 17: The authors state that lower cloud drop numbers are due to increased heterogeneous freezing. Have the authors considered impacts of riming in the different simulations? What role does the collision of liquid droplets with other hydrometeors have in reducing the number of liquid droplets in the simulations?

15) P12, Line 33: The authors state the IN have a minimal impact on liquid nucleation here, but it would be beneficial to remind the readers here (or in the conclusions) that dust can serve highly effectively as a CCN (see papers by Twohy et al and others) and that the impacts of dust on the ice phase may therefore be overestimated by not allowing for dust to serve as CCN and hence be removed in the warm phase.

16) P13, L1-2: "within the selected temperature range of ice/snow . . ." What are these temperature ranges? Include those temperature thresholds here in the text to assist readers.

17) P13, L1-21: Throughout this section of the text, the authors state a lot of microphysical pathways without fully explaining and demonstrating these pathways in the simulations, as discussed in more detail in the next several comments. The authors need to provide a better explanation of the physical processes at play. It is also stated earlier in the manuscript that some of the processes are tracked. The whole of this section would be improved by providing actual magnitudes and/or rates of the physical processes involved.

18) P13, L5-7: Initially, there is an increase in smaller ice crystals in all 3 dust cases, but then there is a decrease in smaller ice crystals in D.12 and D1.2, but not D12. The authors state that this is due to a change in regime of having more particle collisions, as opposed to formation of new ice crystals, but if there was a regime change, then shouldn't this trend also be present in the D12 case? The D12 case continues to have increases in small ice crystal number throughout the simulation. Could the simulation be running out of IN or the convection be cut off from the source of IN during the more intense convective period for the D.12 and D.12 cases? This seems like another plausible explanation. Please will the authors address this in the text?

19) P13, L7-8: The authors attribute the increase in larger liquid drop sizes due to condensation, but what about melting of larger ice crystals that are also present?

20) P13, L9-10: The authors state that the increase in smaller liquid drops is due

to transport from below the freezing level, but this trend is not evident in all the dust simulations and at all the times. Can the authors provide more specifics here?

21) P13, L25-26: What is this ratio in the simulations?

22) P13, L30-34: I do not follow how these methods rule out dynamical effects on the system. This comment is in-line with one of my previous comments in this reagrd. Can the authors provide more explicit discussion here?

23) P14, L5: Can the authors show the stratiform region PSD averages, as well? All of your other plots have 4 columns, and thus it would seem that another column could be added to this plot too. The difference plot of the stratiform region of the Dust case from the Clean case is somewhat meaningless without showing the Clean case PSDs in Figure 6. It is very interesting that the simulations have similar structured PSD in the convective and stratiform region, given the differences in microphysical processes that typically occur within these regions.

24) P14, L11: "although larger sized particles are more slightly common above the freezing level". I am having a hard time seeing this in Figure 6. Which hydrometeor are the authors referring to with the statement?

25) P14, L21-22: Can the authors more explicitly explain how looking at number concentrations before and after the effects of gravity allows for microphysical processes to be tracked? The authors mention instantaneous versus accumulated number on L23-24, but do not provide enough details for the reader to understand what Figure 7 is actually showing. For instance, what time interval is this number flux being calculated over for instance? It takes time for the effects of gravity to operate and for the hydrometeors to fall. This may well be a useful figure and analysis, but it needs to be better described.

26) P15, L25-26: While Saharan dust may undergo less atmospheric processing than Asian dust, it still can serve effectively as CCN, as shown by Twohy et al (2009), the

paper referenced by the authors in the previous paragraph. It might be more beneficial to write this paragraph in terms of representing the thresholds of dust activation as IN and then at CCN and IN, where the studies presented up to this point represent the lowest end of CCN activation.

27) P15, L26-28: The authors mention that prior figures show that the simulations reproduce cloud effects witnessed in a suite of observational studies. Can the authors specify which cloud effects they are referring to (i.e., lower cloud top heights in dusty conditions)? Also, this statement does raise an interesting question regarding the utility of the model. If it is known that dust serve as both CCN and IN, and yet the model output compares well with the observations when dust is only allowed to serve only as IN, how well is the new parameterization actually performing? One way in which to address this would be using measurements of the CCN and/or IN activation capabilities of dust during this field campaign. Either way, a comment should be made in this regard in the manuscript.

28) P16, L19: Should "ice to liquid" be "liquid to ice"?

29) P16, L17-19: "The smaller liquid number at temperatures above freezing can be explained by the reduced ice content immediately above the melting level." Can the authors provide an additional sentence explaining the microphysical process that is causing the smaller liquid number here?

30) P16, L33: From Figure 8c, the D12 case looks to be most different from the other dust cases, not D1.2b as stated in the text. If the authors are only comparing the D1.2a-c cases with this statement, then the authors need to specify this?

31) P17, L3-4: The authors use domain-averaged OLR, but can the authors show OLR only from convective/stratiform columns? Given that cloud occurrence is only $\sim$5% of domain (Figure 8c), it is difficult to justify that the domain-averaged values are represented of the changes to the cloud system.

Figures and Tables

Most of the figures require improvement. The fonts are typically too small, and the sizes of each panel need to be increased. Line thicknesses also should be increased.

1) Figure 1: The color schemes don't seem to match in the two images, despite the fact that the images use the same colorbar. Can you please describe why there is this discrepancy?

2) Figure 3: What are the lower lines in figure 3f, g and h? Are these the strongest downdrafts? This should be made clear in the caption. Also, are these counts of updraft grid points for the whole time period shown in the other figures?

3) Captions are needed for all of the tables in the text.

4) Figure 4: subfigure letters do not match subfigure letters described in text.

5) Figure 5: the titles of the second and third rows do not match the caption. Please correct.

6) Figures 7 and 9: The lines need to be made more distinguishable and thicker. This is especially the case for Figure 9a.

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