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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative impact of mineral dust on monsoon precipitation variability over West Africa" *by* C. Zhao et al.

Anonymous Referee #2

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Review of: Radiative impact of mineral dust on monsoon precipitation variability over West Africa

Overview: It well established that the abundance of mineral dust aerosols over West Africa has major implications on the regional climate. This paper focuses on the interplay between mineral dust and the West African Monsoon during the warm, wet season. The authors simulate the interplay using the WRF model and show that radiatiative forcing of the mineral dust changes the diabatic heating at the surface and middle of the atmosphere, in opposite directions as a function of the hour of the day. The authors assert that this radiative forcing reduces instability during the day and increases it at night, leading to a decrease in convective precipitation during the day and an increase at night. The aforementioned results explain the mechanisms by which

and address prior ambiguity in which previous studies to have shown both an increase and a decrease in precipitation in the region. A secondary conclusion of the paper is that the majority of sensitivity of the simulation is a function of the optical properties of mineral dust, which requires additional attention and work from the scientific community.

General Impression: The results of this work are interesting, and the paper is well written and thoughtfully presented. This work represents an improvement and a contribution to understanding of the West African climate. Ultimately I hope that the editor chooses to publish this paper, however I have a two critical concerns that I would like to see addressed before this paper is published.

âĂć The authors on occasion make subjective conclusions from their figures and results that some readers may disagree with and could be improved by providing some simple quantification. âĂć The stability proxies provided by the authors are inappropriate and overly simplistic choices measures of vertical stability and do not prove that the atmosphere is made more and less stable during the course of the day as a function of radiative forcing. A more rigorous analysis of the stability of the atmosphere is warranted to prove the key conclusion of the paper.

In the following sections I expand on my above enumerated concerns and also provide some other concerns and suggestions that are not critical, but may lead to a stronger paper in the end should the authors choose to address them.

Critical Concerns:

(1) There are at least two cases in which the authors make highly subjective arguments:

(a) Referring to figure 3. On page 14, Line 21-23 "The simulation with 22 dust well captures the upward SW fluxes." I am not sure that I agree with the authors's characterization of the figure. To my eye there are some deviations between observation and the model and a clear positive bias in the modeled results. A bias of even 20 W/m represents a 5% error, and would have major impacts on the results. As such I feel that this statement needs to be quantified with statistical tests.

(b) Referring to figure 5. Page 16, Line 4-5. “WRF-Chem generally well captures the seasonal migration of precipitation.” While I can see what the authors are arguing, others may disagree. Here again the authors’s assertion can easily be quantified by some statistical tests.

The paper would be stronger and the results more rigorous if the authors would provide some simple statistics to back up the arguments they are making.

(2) The crux of the paper appears in section 4.2.1 “Dust impact on precipitation.” In 4.2.1 the authors argue that the diurnal cycle of precipitation is what is changed by the presence of mineral dust, and this alteration to the cycle is driven by changes to the thermal/vertical stability of the atmosphere. Given that this is the key conclusion of the paper, I think that great care needs to be taken when discussing (and perhaps most importantly quantifying) the stability. As currently written the authors’s argument is undercut by a poor choice of stability criteria, and the central tenant of their paper is not actually supported by the evidence presented.

On page 16, lines 16-23 the authors begin a discussion of convectively available potential energy (CAPE), arguing that low level diabatic heating (cooling) increases (decreases) the amount of CAPE, and in turn increases (decreases) the amount of precipitation in the WAM. As CAPE is a vertically integrated quantity looking at only one layer of the atmosphere is insufficient to explain it. CAPE is explicitly calculated in the WRF, yet there is no analysis of this quantity presented in the discussion. The authors’s argument would be significantly strengthened by including either include a figure or table showing how CAPE changes with and without dust. CAPE is a technical quantity, with a mathematical definition, it should either be quantified or removed from the discussion, replaced with a more generic term.

On page 16, lines 18-20 the authors note: “18 heating of the surface can directly af-

fect the convective available potential energy 19 (CAPE) in the planetary boundary layer and increase convective activity, leading to late 20 afternoon precipitation (peak around $\sim$ 5 pm).” Classic thinking on convection is that CAPE is found aloft, and that convection is initiated when CIN in the lower levels of the atmosphere is eroded away as daily diabatic heating occurs. Instability aloft is as important if not more important than instability at the surface in driving tropical precipitation, but the authors do not discuss any instability above the boundary layer. CAPE is a vertically integrated quantity, not something measured at any one level, and required a more detailed discussion. Figure 9 implies that diabatic warming is occurring simultaneously as surface cooling is occurring. This warming aloft (not discussed by the authors) during the daytime hours may be as important as diabatic cooling (discussed by the authors) near the surface at the same time in reducing the frequency and intensity of convection. The current discussion of CAPE needs to include a discussion of how changes aloft affect it, in addition to the current discussion of the surface heating. This can be easily shown with vertical thermal profiles generated from WRF output.

On page 18, lines 19-23 the authors introduce the equivalent potential temperature (specifically at 925 hPa) and argue that it can be used as a proxy for atmospheric stability. Atmospheric stability is not a function of temperature at one level, but rather a function of the vertical gradient of temperature or differences in temperature between multiple levels. The use of equivalent potential temperature at one level in the atmosphere cannot alone show changes in stability. If the background environment remains constant, warming the boundary layer would increase instability. However the authors clearly show that the thermal profile aloft is changing (Figure 9), so one cannot simply look at warming or cooling at the surface and say that this is changing the stability. Some measure of the vertical gradient in potential temperature must be used, not a single value at one level.

It is not sufficient when talking about stability to focus on any one level in the atmosphere, as it is generated by vertical differences. This is particularly important when all levels in the atmosphere are being altered by the presence of mineral dust (Figure 9), often changing in different directions. The authors's current argument appears valid, and the conclusions not likely changed, but this discussion is yet incomplete.

Suggestions and Concerns: Page 7-9. How are Sea Surface Temperatures handled in this simulation? Other work (Giannini et al., 2003) has shown that precipitation over Western Africa can be affected by fluctuations in ocean temperatures in the Atlantic and Indian Ocean. One could certainly argue that to examine the effects of mineral dust aerosols that SST should be fixed, but either way I think in the least that your choice of SST conditions needs to be mentioned in the paper. The prescription of SST is noted on page 17, line 17-18, but the source is never fully described.

Page 13. Use of local time should be discouraged. GMT or Zulu time should be used. The WAM region includes two time zones so it is unclear to what exact time is being referred when local time is given. Using GMT or Zulu time removes all ambiguity.

Page 13, Line 11-13. "WRF-Chem generally reproduces 12 the spatial distribution of satellite retrieved AOD, except for the low bias at the southern 13 boundary that may result from the idealized chemical boundary conditions used."

Specifically it appears as if the model simulation either excludes or cannot handle biomass burning aerosols from Central Africa. I feel as if the above statement should be amended to address this.

Page 22, Line 7-8. "Although changes in the upper level winds are small, significant changes (>5%, 8 up to 40%) of 10m-wind speed are found over the Sahara desert."

Is this statistically significant changes or subjectively significant changes? A contour on figure 11 should be added showing areas where the difference is statistically significant.

Minor Suggestions and Concerns:

Page 4, Lines 2-4. "2 The West African Monsoon (WAM) system is a major climate system and an important component of the regional hydrological cycle on which the

livelihood of a 4 large and growing population over Sahel depends.

While not required, citations to the above statement would be useful.

Page 4, Lines 5-7. "On the other 6 hand, the Sahara desert is the largest source of mineral dust aerosol in the world [e.g., 7 Woodward, 2001; Prospero et al., 2003]. The expression "on the other hand" appears to be used incorrectly in this sentence.

Page 4, Lines 7-11. "The Saharan dust uplifted during the WAM 8 season can significantly affect the WAM development and precipitation, because it 9 interacts with both shortwave (SW) and longwave (LW) radiation, and modifies the 10 radiative and physical properties of clouds [e.g., Miller et al., 2004; Yoshioka et al., 11 2007; Konare et al., 2008; Lau et al., 2009; Kim et al., 2010]." Uplifted is a very ambiguous and possibly inappropriate term. Emitted or transported are both more specific than "uplifted."

Page 4, Line 23 to Page 5, Line 1. "23 effect may strengthen the WAM, which is manifested in a northward shift of the West 1 Africa precipitation over land." This statement was a bit confusing to me. Are the physical location and intensity of the WAM two separate quantities or one in the same?

Page 8, Lines 13-15. "In this study, the model domain covers West Africa (36.15oW-13 40.15oE, 9.2oS-37.0oN) using 200ïČŮ150 grid points at 36 km horizontal resolution 14 centering at Niamey (Niger) (2.0oE, 13.6oN), and 35 vertical layers with model top 15 pressure at 10 hPa." It might be helpful to readers to have this domain related to the WAM, but perhaps this is not necessary.

Page 13, Line 23. "The domain averaged" Are you referring to the entire model domain or to the WAM region?

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C12400/2011/acpd-10-C12400-2011-supplement.pdf

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