

We thank two anonymous referees for their valuable comments to the manuscript and their constructive suggestions. Below, we explain how the comments and suggestions are addressed and make note of the revision we made in the manuscript.

Anonymous Referee #1

General comments:

In this article, the authors examine the impact of mineral dust aerosols on the West African Monsoon (WAM) climate using the regional WRF-Chem model driven by NCEP/NCAR global reanalysis. They find that the interplay between short-wave and longwave dust effects impact the diurnal stability of the atmosphere - stabilizing the atmosphere during the day and destabilizing it at night. As a result, late afternoon precipitation decreases and nighttime/early morning precipitation increases; this improves agreement compared to measurements. They also show that the impact of dust on precipitation is highly sensitive to the assumed absorptivity of dust.

I find this paper to be very well written and timely in its content. With a few mostly minor adjustments, I find it acceptable for publication in ACP. However, I would like specific attention at addressing points 6 and 7 in the Specific Comments, as I feel these points are important.

We thank the reviewer for a detailed review. Both text and figures are revised as the reviewer suggested.

Specific comments:

- *1. Model Description: Here you discuss the two possibilities for representing aerosol distributions in WRF-Chem: modal (MADE/SORGAM) and size-binned (MOSAIC). However, it is not at all clear to me which representation is used in this paper. Are you using both? I don't believe so, but why discuss both if you are not using both? If you are using both, then why? This point really needs clarification.*

We describe two aerosol schemes here, because both of them are available for dust-climate interaction simulations. We chose MADE/SORGAM in this study, since it's less computationally demanding than MOSAIC. Now It's clarified in the text "MADE/SORGAM in WRF-Chem uses the modal approach with three modes (Aitken,

accumulation, and coarse modes, assuming a log-normal distribution within each mode) to represent the aerosol size distribution, while MOSAIC uses a sectional approach where aerosol size distribution is divided into discrete size bins defined by their lower and upper dry particle diameters. Generally, a modal approach is less accurate because of its assumption of lognormal size distribution and limited number of modes, but it is less computationally demanding than a sectional approach that uses more bins.” and “The MADE/SORGAM aerosol module with the GOCART dust emission scheme is used in this study as described in Zhao et al. [2010]. Zhao et al. [2010] found ~10% difference in the dust SW radiative forcing between the modal and sectional approaches in WRF-Chem over West Africa during the dry season.”

- ***2. Section 4.1, Page 27195, L.9-12: Is the low bias at the southern boundary really due to chemical boundary conditions? What exactly do you mean by the southern boundary? (i.e. the WAM boxed region or the whole region including Southern Africa?) If the latter, could the low bias be due to a low-bias in biomass burning aerosol over this region?***

Since biomass burning aerosol emissions over the simulation domain during the WAM season are small, the low bias likely results from neglecting biomass burning aerosols that could be transported from South Africa into the WAM domain. The southern boundary means the southern boundary of the whole simulation domain. Note that the idealized chemical boundary conditions [McKeen et al., 2002] cannot account for the biomass burning aerosols that could be transported through the southern boundary from South Africa. Now it's clarified in section 4.1 “Since the biomass burning aerosol emissions over the simulation domain during the WAM season are small (not shown), the low bias may result from the idealized chemical boundary conditions [McKeen et al., 2002] that cannot account for the biomass burning aerosols potentially transported from South Africa.” and in model description “Chemical lateral boundary conditions are from the default profiles in WRF-Chem, which are based on the averages of mid-latitude aircraft profiles from several field studies over the eastern Pacific Ocean [McKeen et al., 2002].”

- **3. Section 4.1, Page 27195, L.20: Define the domain referred to by “domain averaged.”**

Now it's clarified in the text “the domain averaged (average over the entire domain (28.9°W-32.9°E, 5.0°S-32.1°N)) dust SW radiative forcing”.

- **4. Section 4.1, Page 27196, L. 17: You state the WRF-Chem captures the AMF retrievals well when dust is included. I don't really see this in Figure 3. Can you provide quantitative support of this (i.e. correlation coefficient)?**

Since the simulation is at $36 \times 36 \text{ km}^2$ horizontal resolution and the dust emission is calculated based on the source function that is $1^\circ \times 1^\circ$ resolution [Ginoux et al., 2001], the model is not expected to capture every single dust event. Now Figure 3 is changed to show the mean diurnal cycle of SW radiation fluxes at the surface, which shows more clearly the dust effect on SW radiation and how it improves the simulations. More quantitative analysis is added. The corresponding changes in the text are as follows: “The model simulated surface SW radiative fluxes are compared to the AMF retrievals. Diurnal cycle of upward and downward SW radiative fluxes at the surface at Niamey airport averaged during the simulation period from the AMF retrievals and WRF-Chem simulations with and without dust aerosols are shown in Figure 3. The AMF retrievals show noontime maximum SW radiative fluxes with daily averages of 49 W m^{-2} and 240 W m^{-2} for upward and downward fluxes respectively. The simulation with dust well captures the upward SW fluxes with a daily average of 50 W m^{-2} . Compared to the simulation without dust, dust reduces the daily averaged upward SW fluxes by about 5 W m^{-2} and improves the simulation. For downward SW fluxes, the simulation without dust significantly overestimates the values with a daily average of 270 W m^{-2} , while the simulation with dust well captures the AMF retrievals with a daily average of 244 W m^{-2} . Dust reduces the downward SW fluxes by $\sim 25 \text{ W m}^{-2}$ on daily average and up to $\sim 100 \text{ W m}^{-2}$ near noontime, and significantly improves the model simulations.”

- **5. Section 4.1, Page 27196, L. 27: Can you show how small the dust impacts on OLR are, or quantify how small they are in comparison to differences related to using the Lin cloud microphysics scheme or other schemes?**

Dust reduces OLR by up to $\sim 10 \text{ W m}^{-2}$ over the desert region (Figure 2). It's relatively small compared to the OLR of $\sim 300 \text{ W m}^{-2}$ over most part of the desert (Figure 4). The difference among simulations using different sets of microphysics and convective schemes can be 30 W m^{-2} compared to the OLR of $200\sim 250 \text{ W m}^{-2}$ in the South (south to $\sim 20^\circ\text{N}$). Now we clarify in the text "Sensitivity simulations using WRF with different cloud microphysics and convective schemes show that the OLR bias can be significantly reduced in the South (by up to $\sim 30 \text{ W m}^{-2}$ compared to the OLR of $200\sim 250 \text{ W m}^{-2}$), suggesting that part of the bias comes from the Lin cloud microphysics and Grell convective schemes used in the control simulation. Dust reduces OLR by up to $\sim 10 \text{ W m}^{-2}$ over the desert region (Figure 2). Since the effect is relatively small compared to the OLR of $\sim 300 \text{ W m}^{-2}$ over most part of the desert (Figure 4), only results from the simulation with dust are shown in this figure."

- ***6. Section 4.2.1, Page 27197, L. 15: You mention that the underestimate in heavy precipitation events results from use of the Lin cloud microphysics scheme. Later in the conclusions, you state that the Lin scheme is included to account for dust indirect effects on stratiform cloud microphysics -- even though convective precipitation dominates during the WAM season. Why, then, if you are not focusing (or paying any attention to, really), aerosol indirect effects, do you use this scheme? Would it not be better to have less bias in convective precipitation (by using another convective scheme) since this is your focus? I do not understand the reasoning here other than to preclude the inevitable reviewer question "what about the indirect effect." Most importantly, would you expect your results (i.e. dust impact on convective precipitation) to change if another scheme were used?***

WRF has many choices of cloud microphysics and convective parameterizations. However, not all the available schemes have been modified for use in WRF-Chem to account for aerosol direct and indirect effects. Some aerosol processes (e.g., cloud chemistry, wet deposition, and aerosol indirect effect) are coupled only with the Lin microphysics scheme and the parameterized convective cloud radiative feedback is only included when using the Grell scheme in the current version (v3.1.1) of WRF-Chem. All the sensitivity simulations with different cloud microphysics and convective schemes are

conducted using WRF (not WRF-Chem) to assess the effects of cloud microphysics and convective parameterizations on clouds and precipitation. Now it's clarified in the section 4.1 "Sensitivity simulations using WRF with different cloud microphysics and convective schemes show that the OLR bias can be significantly reduced in the South" and in section 4.2 "Simulations using WRF with different cloud microphysics and convective schemes show a reduction of the precipitation bias." and in model description "The Lin cloud microphysics scheme is used as described by Gustafson et al. [2007] to account for cloud chemistry, aerosol wet deposition, and aerosol indirect radiative effect. The Grell convective scheme is used to allow the feedback from the parameterized convective cloud to the radiation schemes. In the available version (v3.1.1) of WRF-Chem during this study, the Lin cloud microphysics and Grell convective schemes are the only parameterizations that are coupled with the full aerosol processes (including cloud chemistry and wet deposition) and cloud radiative feedback (WRF-Chem user guide from http://ruc.noaa.gov/wrf/WG11/Users_guide.pdf)."

The dust impact on precipitation could be sensitive to cloud microphysics and convective schemes. It's added in the conclusion "Last, the dust impact on the diabatic heating and hence the atmospheric stability is evident, however, the dust-induced changes of diurnal amplitude, diurnal phase (negligible in this study), and total amount of WAM precipitation may be sensitive to the cloud microphysics and convective schemes, which is beyond the scope of this study."

- ***7. Section 4.2.1, Page 27198, L. 22-24: I think you may need to qualify this statement. While the immediate impact of dust on precipitation over the ocean may be small due to ocean heat capacity, is it true that the dust impact on cooling SSTs would have no, perhaps longer-term, impacts?***

Now it's clarified "Even if sea surface temperature is simulated, dust effect is expected to be small within short-term because of its lower concentration over the ocean and the high heat capacity of the ocean."

Technical Comments:

- *Abstract, P. 27186, L. 17-23: These sentences should be reworded, as they seem contradictory and are hard to digest. A reword might look like: “Sensitivity simulations show that, at the surface, dust longwave warming at night surpasses daytime shortwave cooling; this leads to a less stable atmosphere associated with more convective precipitation in the nighttime. When considering weaker to more absorbing dust solar absorptivity, which is uncertain, daily WAM precipitations varies from”*

It's clarified in the text “On the other hand, sensitivity simulations with weaker to stronger absorbing dust (in order to represent the uncertainty in dust solar absorptivity) show that, at the lower atmosphere, dust longwave warming effect in the nighttime surpasses its shortwave cooling effect in the daytime; this leads to a less stable atmosphere associated with more convective precipitation in the nighttime. As a result, the dust-induced change of daily WAM precipitation varies from a significant reduction of -0.52 mm/day (-12%, weaker absorbing dust) to a small increase of 0.03 mm/day (1%, stronger absorbing dust).”

- *2. Section 3.2, Page 27192, L. 21: Change “...called ‘Deep Blue algorithm’ ...” to “...called the ‘Deep Blue algorithm’...”*

Corrected.

- *3. Section 3.2, Page 27192, L. 22: Change “...integrated with existing MODIS algorithm ...” to “...integrated with the existing MODIS algorithm ...”*

Corrected.

- *4. Section 3.3, Page 27193, L. 10: Change “... to be used not depending on ...” to “...to be used that do not depend on ...”*

Corrected.

- *5. Section 4.2.1, Page 27199, L. 14: Change “...by up to 2.5 K/d and warms ...” to “... by up to 2.5 K/d, and warms ...”*

Corrected.

- **6. Section 4.2.1, Page 27199, L. 16: Spelling, change heaing to heating**

Corrected.

- **7. Section 4.2.1, Page 27199, L. 27: This last sentence is hard to digest. Perhaps reword to something like: “CAPE has a much larger value during daytime, and convective precipitation accounts for over 90% of precipitation in the simulation. Therefore, the net change is a reduction of the daily? precipitation due to a larger reduction during daytime and smaller increase during nighttime.”**

It's clarified in the text “Convective precipitation accounts for over 90% of precipitation in the simulation and has a much larger value during daytime. Therefore, the net change is a reduction of the daily precipitation from larger reduction of daytime precipitation and smaller increase of nocturnal precipitation.”

Anonymous Referee #2

General comments:

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 radiative 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.

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.

- 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.*
- 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.

We thank the reviewer for a detailed review. Both text and figures are revised as the reviewer suggested.

Specific comments:

- *(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 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.

Since the simulation is at $36 \times 36 \text{ km}^2$ horizontal resolution and the dust emission is calculated based on the source function that is $1^\circ \times 1^\circ$ resolution [Ginoux et al., 2001], the model is not expected to capture every single dust event. Now Figure 3 is changed to show the mean diurnal cycle of SW radiation fluxes at the surface, which shows more clearly the dust effect on SW radiation and how it improves the simulations. More quantitative analysis is added in the text. The corresponding changes in the text are as follows: “The model simulated surface SW radiative fluxes are compared to the AMF retrievals. Diurnal cycle of upward and downward SW radiative fluxes at the surface at Niamey airport averaged during the simulation period from the AMF retrievals and WRF-Chem simulations with and without dust aerosols are shown in Figure 3. The AMF retrievals show noontime maximum SW radiative fluxes with daily averages of 49 W m^{-2} and 240 W m^{-2} for upward and downward fluxes respectively. The simulation with dust well captures the upward SW fluxes with a daily average of 50 W m^{-2} . Compared to the simulation without dust, dust reduces the daily averaged upward SW fluxes by about 5 W m^{-2} and improves the simulation. For downward SW fluxes, the simulation without dust significantly overestimates the values with a daily average of 270 W m^{-2} , while the simulation with dust well captures the AMF retrievals with a daily average of 244 W m^{-2} .”

Dust reduces the downward SW fluxes by $\sim 25 \text{ W m}^{-2}$ on daily average and up to $\sim 100 \text{ W m}^{-2}$ near noontime, and significantly improves the model simulations.”

(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.

Following the reviewer comment, some statistical analyses are added in the text “WRF-Chem generally captures the seasonal migration of precipitation with a temporal correlation coefficient of 0.55 with both retrievals. However, WRF-Chem simulates an averaged precipitation rate of 3.0 mm/day during the simulation period, compared to 4.0 mm/day from GPCP and 3.6 mm/day from TRMM. This low bias mostly results from the model underestimation of heavy precipitation events ($>10 \text{ mm/day}$) during the monsoon season.”

- *(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 affect the convective available potential energy 19 (CAPE) in the planetary boundary layer and increase convective activity, leading to late 20 afternoon precipitation (peak around ~ 5pm)." 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 cooling. 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.

Following the reviewer's comment, now the reference to CAPE is removed from the discussion. Instead, the vertical profiles of diabatic heating and equivalent potential temperature (ETH) are used directly as the criteria for atmospheric stability. Figure 9 and 10 are changed to show the vertical profiles of diabatic heating and ETH at 00 UTC and 12 UTC. More discussion is added for Figure 9 and 10.

Lines 16-23 of page 16 are changed to “Over land, solar heating of the surface can increase convective activity, leading to late afternoon precipitation (peak around ~ 5 pm). Over the ocean, the surface does not cool as much as the land due to the high heat capacity of the oceanic mixed-layer. As the atmosphere cools during nighttime, the atmosphere is de-stabilized by the warmer ocean surface. As a result, oceanic precipitation often peaks between midnight and early morning [Kim et al., 2010].”

The corresponding text in describing Figure 9 and 10 is changed, in section 4.2.1 to “Figure 9 shows the dust impact on atmospheric diabatic heating profiles at 00 UTC (mid-night) and 12 UTC (noontime) averaged over the WAM region (6°N-17°N and 15°W-10°E) from the WRF-Chem simulations in three cases with different dust absorption properties. In the control simulation (black line), dust cools the lower atmosphere (below 850 hPa) by up to 1.5 K/day and warms the atmosphere above by up to 1.0 K/day in the daytime, and warms the lower atmosphere by up to 1.2 K/day and cools the upper atmosphere by up to 0.3 K/day in the nighttime.” and “The dust-induced change of the surface energy and atmospheric diabatic heating profiles could modulate the stability of the atmosphere. The dust impact on equivalent potential temperature (ETH) profiles, a quantity related to the stability of a column of air in the atmosphere, at

00 UTC (mid-night) and 12 UTC (noontime) averaged over the WAM region from the WRF-Chem simulations is shown in Figure 10. In general, a decrease of ETH in the lower atmosphere and an increase of ETH above indicate an increase of the atmospheric stability. In the control simulation (black line), in the daytime, dust reduces the lower atmospheric (below 850 hPa) ETH by ~ 0.3 K and increases the ETH of the atmosphere above by ~ 0.5 K, leading to a more stable atmosphere. In the nighttime, dust increases the atmospheric ETH below 600 hPa by up to ~ 0.3 K with larger impact in the lower atmosphere (below 800 hPa), leading to a less stable atmosphere. This dust-induced change of atmospheric stability constrains the buildup of convective cloud in the daytime and fosters the buildup of convective cloud in the nighttime. Convective precipitation accounts for over 90% of precipitation in the simulation and has a much larger value during daytime. Therefore, the net change is a reduction of the daily precipitation from larger reduction of daytime precipitation and smaller increase of nocturnal precipitation.”

In section 4.2.2, we added, “The dust impact on the atmospheric diabatic heating profile is relatively small (less than 0.3 K/day compared to 1.5 K/day in the control simulation) (Figure 9). Therefore, the dust warming effect (through SW radiation absorption) cannot offset its cooling effect (through SW radiation extinction) at the lower atmosphere. As a result, dust reduces the atmospheric ETH below 500 hPa by up to ~ 0.6 K with larger impact in the lower atmosphere (below 800 hPa) (Figure 10), leading to more stable atmosphere and hence less convective precipitation throughout the day.” and “The dust cools the lower atmosphere (below 850 hPa) by up to 2.5 K/day and warms the atmosphere above by up to 2.0 K/day in the daytime, and warms the lower atmosphere by up to 1.4 K/day and cools the upper atmosphere by up to 0.5 K/day in the nighttime (Figure 9). As a result, dust reduces the lower atmospheric (below 850 hPa) ETH by ~ 0.5 K and increases the ETH of the atmosphere above by ~ 1.5 K in the daytime, and increases the atmospheric ETH below 500 hPa by up to ~ 1.0 K with larger impact in the lower atmosphere (below 700 hPa) in the nighttime (Figure 10).”

Technical Comments:

- ***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.

Now it's clarified in the model description part "Large-scale meteorological fields are assimilated with lateral boundary and initial conditions from NCEP/NCAR Global reanalysis data, which also provide the prescribed sea surface temperature (SST) for the simulations."

- *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.*

Now all is changed to UTC time.

- *Page 13, Line 11-13. "WRF-Chem generally reproduces the spatial distribution of satellite retrieved AOD, except for the low bias at the southern 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.*

Since the biomass burning aerosol emissions over the simulation domain during the WAM season are small, the low bias likely results from neglecting biomass burning aerosols that could be transported from South Africa into the WAM domain. Note that the idealized chemical boundary conditions [McKeen et al., 2002] cannot account for the biomass burning aerosols that could be transported through the southern boundary from South Africa. Now it's clarified in section 4.1 "Since the biomass burning aerosol emissions over the simulation domain during the WAM season are small (not shown), the low bias may result from the idealized chemical boundary conditions [McKeen et al., 2002] that cannot account for the biomass burning aerosols potentially transported from

South Africa.” and in model description “Chemical lateral boundary conditions are from the default profiles in WRF-Chem, which are based on the averages of mid-latitude aircraft profiles from several field studies over the eastern Pacific Ocean [McKeen et al., 2002].”

- ***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.***

In terms of the day-to-day variation, the change is not significant based on the Student’s t-test. But the change of the diurnal variation is 95% significant based on the Student’s t-test. Now it’s clarified in the test “Although changes in the upper level winds are small, relatively larger changes (>5%, up to 40%) of 10m-wind speed are found over the Sahara desert. These changes are not statistically significant based on the Student’s t-test with respect to day-to-day variation.” and “As a result, the diurnal variation of 10m-wind speed is significantly reduced by ~10%. This reduction is 95% statistically significant based on the Student’s t-test.”

- ***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.***

Now Sultan et al. [2005] is cited.

- ***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.***

Now the sentence is changed to “The Sahara desert over West Africa is the largest source of mineral dust aerosol in the world [e.g., Woodward, 2001; Prospero et al., 2003].”

- *Page 4, Lines 7-11. “The Saharan dust uplifted during the WAM season can significantly affect the WAM development and precipitation, because it 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.”*

“Uplifted” is changed to “emitted”.

- *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 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?*

Lau et al. [2009] and Kim et al. [2010] found a northward shift of the WAM precipitation. It indicates the WAM system moves further north, which may demonstrate that the intensity of the WAM is strengthened.

- *Page 8, Lines 13-15. “In this study, the model domain covers West Africa (36.15oW-40.15oE, 9.2oS-37.0oN) using 200x150 grid points at 36 km horizontal resolution centering at Niamey (Niger) (2.0oE, 13.6oN), and 35 vertical layers with model top pressure at 10 hPa.” It might be helpful to readers to have this domain related to the WAM, but perhaps this is not necessary.*

The WAM region (6°N-17°N and 15°W-10°E) corresponding to the model domain is shown in Figure 1.

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

Now it’s clarified in the text “the domain averaged (average over the entire domain (28.9°W-32.9°E, 5.0°S-32.1°N)) dust SW radiative forcing”.