Review #1

Review of Deroubaix et al, Sensitivity of low-level clouds and precipitation to anthropogenic aerosol emission in southern West Africa: a DACCIWA case study

DACCIWA case studies in the monsoon season of July 2016 are simulated with WRF-CHIMERE, focusing on aerosol effects on low clouds and precipitation.

The authors have produced a very well-written paper and there are opportunities for valuable scientific insights. I have some minor comments, below, that should be addressed prior to publication. In particular, if the authors can elucidate the processes in their model that are responsible for their results in more detail, in line with my suggestions in bold text below, I think this will be a very useful and well cited study.

We thank the reviewer for their positive opinion on the manuscript.

Minor comments:

A few more details on model description would be useful:

What is the vertical resolution of the WRF model at the level of the clouds?

The vertical resolution at the altitude of the clouds is about 300 m. The vertical level height is fixed and is shared by WRF and Chimere in the troposphere. From the surface to 2000 m, there are 12 levels. The altitude of the middle of the vertical levels are in averaged over the domain: Level 1 = 18 m; Level 2 = 41 m; Level 3 = 74 m ; Level 4 = 117 m; Level 5 = 176 m; Level 6 = 256 m; Level 7 = 367 m; Level 8 = 511 m; Level 9 = 714 m; Level 10 = 992 m; Level 11 = 1380 m; Level 12 = 1926 m.

This information has been added to the sentence (Page 5, lines 12-14): *"The 32 vertical levels of WRF from the surface to 50 hPa are projected onto the 20 levels of CHIMERE from the surface up to 200 hPa, which includes 12 levels below 2 km altitude where the LLC are located,"*

How does the Thompson and Eidhammer microphysics scheme do aerosol activation? What are the mechanisms by which aerosols can affect cloud lifetime in this scheme?

An explanation of how the aerosol activation is done in the coupled model has been included in Section 2.1. Moreover, we explain how a change in the aerosol load affects the cloud lifetime.

These two new paragraphs are (pages 4-5, lines 31-9): *"The scheme proposed by Thompson and Eidhammer (2014) is an aerosol aware cloud microphysics scheme including a parametrization for aerosol activation based on a single mode log-normal distributed aerosol population taken from a climatology. In WRF-CHIMERE, this approach has been replaced by using the aerosol size distribution and composition predicted by CHIMERE and the aerosol activation as cloud water droplet is parametrized following the Abdul-Razzak (2002) approach.*

Starting from the updraft velocity, aerosol size distribution and composition, the Abdul-Razzak (2002) scheme predicts the fraction of activated aerosol within each model section as a function of maximum supersaturation of an adiabatic rising parcel. According to Ghan et al. (1997), the fraction of aerosols activated in each section of the CHIMERE size distribution is calculated on a maximum supersaturation determined from a Gaussian spectrum of updraft velocity and internally mixed aerosol properties following a similar method adopted in WRF-Chem (Chapman et al., 2009). Further details may be found in Tuccella et al. (2019).

In WRF-CHIMERE, the aerosol load affects the cloud lifetime and the precipitation because it modifies the autoconversion from cloud water droplet to rain particles, which is based on the scheme of Berry and Reinhardt (1974). Moreover, it is also affected by the radiation absorption from dust or black carbon via the semi-direct effect (Briant et al., 2017). Over West Africa, the high aerosol load makes the integration of aerosol effects an important step towards understanding meteorological feedbacks during the monsoon (Menut et al., 2019)."

Page 3 line 29: is the aerosol size distribution a single variable, or are all 10 bins transferred to WRF? Is there a hygroscopicity for each bin, or does "bulk" mean that is just a single number for each grid cell? Why are deliquesced aerosols treated separately? I didn't find the reference to Tuccella et al very helpful to figure that out. Does deliquesced aerosols refer to aerosols dissolved in cloud water (referred to as cloud-borne aerosols in the MAM aerosol microphysics schemes)? Also, via the coupler, what fields come back from WRF to CHIMERE, to handle aerosol scavenging for example?

The size distribution is not a single variable, but all bins used in CHIMERE to simulate the aerosol size distribution are transferred to WRF. In our model, the hygroscopicity is a single value calculated for each grid point. The bulk hygroscopicity is based on the volumetric average of hygroscopicity using all the single model species.

The deliquesced aerosols are not the cloud-borne aerosols. In MAM, as well as in WRF-Chem, the cloud-borne aerosols are the aerosol particles activated as cloud droplets. When liquid solution droplets are activated as cloud droplets, there is a change from stable to unstable growth in response to the increase of humidity.

In the online version of WRF-CHIMERE, deliquesced aerosols are used in the homogeneous freezing process. Following the scheme of Thompson and Eidhammer (2014), the homogeneous ice nucleation is parameterized with the method proposed by Koop et al. (2000), where the homogeneous freezing is represented by introducing the water activity. In WRF-CHIMERE, the pre-existing deliquesced aerosols for this process are assumed to be constituted by a mixture of hygroscopic particles (sulphate, nitrate, ammonium, sea salt and secondary organic aerosols) with a diameter larger than 0.1 um.

Note that the homogeneous ice nucleation does not affect the low-level clouds, which are the focus of this article, because it concerns the cirrus clouds.

The fields used to handle the wet scavenging are the liquid and frozen precipitation flux, and liquid and frozen hydrometeor mixing ratio.

We have included all relevant information that were missing in the previous version in the paragraph (Page 4, Line 15-23): *"The coupled WRF-CHIMERE model integrates the direct (and semi-direct) and indirect effects between CHIMERE and WRF through exchanges via an external coupling software developed primarily for use in the climate community, namely OASIS3-MCT (Craig et al., 2017), at a 10 minutes time step. Three fields are sent to the radiative scheme of WRF to represent the direct effect: (i) aerosol optical depth, (ii) single scattering albedo, and (iii) asymmetry parameter (Briant et al., 2017). The indirect effect is taken into account thereby transferring four fields of CHIMERE to the microphysics scheme of WRF: (i) aerosol size distribution (the ten bins used in CHIMERE to simulate the aerosol size distribution are transferred to WRF), (ii) bulk hygroscopicity of internally mixed aerosols (the hygroscopicity is a unique value calculated for each grid point), (iii) ice nuclei, and (iv) deliquesced aerosols (Tuccella et al., 2019). Moreover, the wet scavenging is calculated in CHIMERE using the liquid and frozen precipitation flux, and liquid and frozen hydrometeor mixing ratio."*

Would be better to introduce the models before describing the coupling.

We agree with the reviewer. The paragraphs of section 2.1 have been modified. It is now starting with the description of WRF, followed by the one of CHIMERE, and it ends with an explanation of the coupling.

Is there a sub-grid cloud fraction scheme? If so how does it work?

No, we do not use a subgrid cumulus fraction scheme. The following sentence has been added (Page 3, Lines 29-30): "*We use the scale-aware scheme of convective parameterization proposed by Grell and Freitas (2014) without sub-grid cloud fraction scheme.*"

Page 7: I would say "the nitrate and ammonium concentrations are a factor 100 higher" rather than "are multiplied by 100" as "multiplied" implies you fixed these concentrations deliberately, while in fact, if I understand correctly, it is a model result.

The reviewer is right and it has been corrected.

Page 11 line 6 "denote" is the wrong word here.

We agree with the reviewer that this word is not well chosen. It has been changed by "stand out" in the sentence (Page 11, Line 5-6): *"From 2 to 4 km amsl, the WS observed vertical profile at Parakou and Savè stand out because the wind is higher than at the other locations, being closer to the African Easterly Jet core."*

Line 22: "the processes involving supersaturation to create liquid water are not represented in the model" - -might make sense to rephrase – in the model, the RH is always below 100%

because clouds form at 100% RH? (presumably the model produces clouds somehow, even if this is via saturation adjustment).

Cloud water condenses only when the water vapor exceeds a saturation threshold calculated with the polynomial approximation from Flateau et al. (1992). Saturation adjustment is treated by solving the Clausius-Clapeyron equation with the Newton-Raphson interactive scheme. Further details are provided in Thompson et al. (2008).

This sentence has been rephrased as follows (Page 12, Lines 15-16): *"While the observed RH may exceed 100 % at times, the modeled values are lower than 100 % (Fig. 4) because cloud water condenses only when the water vapor exceeds a saturation threshold (Flatau et al., 1992; Thompson et al., 2008)."*

Page 15 line 3: this is just a phrasing issue, but comparing cloud base height to liquid water mixing ratio doesn't make sense: maybe you compare the cloud base height and LMWR in the model to the observations, or between two time periods?

We thank the reviewer for this comment. The sentence in the previous version: *"We compare the CBH to the modeled LWMR to analyze the LLC formation, elevation and breakup time."* has been modified to (Page 16, Lines 11-12): *"We compare the altitude of the observed hourly CBH evolution to the modeled LWMR vertical patterns in order to analyze the LLC formation, elevation and breakup time."*

Page 15 line 8: what processes in the model lead to increased LWP and cloud cover? Are they indirect effects or semi-direct effects? Are there missing processes in the model that could lead to the opposite effects? Like evaporation/entrainment or sedimentation/entrainment feedbacks for example (see e.g. Hill et al (2009), https://journals.ametsoc.org/view/journals/atsc/66/5/2008jas2909.1.xml)?

The reviewer question concerns an important aspect which was not addressed in the previous version. In the study of Hill et al. (2009), the model of Albrecht (1989), where an increase in aerosol loading leads to the suppression of drizzle and a concomitant increase in LWP, is partially invalidated for non-drizzling marine stratocumulus because in this case the LWP increases due to evaporation-entrainment and sedimentation-entrainment effects. These results concern the marine boundary layer, which may be less influenced by the convection triggered by the radiation of the surface while this is very important for our studied area (Deetz et al., 2019). Moreover, it is difficult to compare a LES study (with a sensitivity test with high vertical and horizontal resolution) with a regional modeling study (with a quite low vertical and horizontal resolution).

The clouds are present in two or three vertical levels of our model, so the representation of evaporation and sedimentation may be somehow oversimplified, even though we could expect that both describe the same physics. In order to answer this question accurately, LES experiments should be conducted using a specific setup for the inland area of West Africa during the monsoon period.

The sentence pointed out by the reviewer has been moderated because we do not observe an increase in LWP everyday in Figure 7. It has been changed from: *"The duration of the LLC in the afternoon lasts longer in AE10 compared to AE1. Every day from 12:00, and 00:00 UTC, we note that the LWMR is slightly higher for the AE10 than AE1."* To (Page 16, Lines 15-17): *"It seems that the duration of the LLC in the afternoon lasts longer in AE10 compared to AE1, and that from 12:00 to 00:00 UTC, the LWMR is slightly higher for the AE10 than AE1."*

Moreover, in order to address this important aspect, we have analyzed the differences between the two experiments for temperature, specific humidity, liquid water mixing ratio and rain mixing ratio. We show that the modifications of the temperature and specific humidity are small, while there are important modifications of the liquid water and rain mixing ratios.

In Section 5, we added a new paragraph (Page 18, Lines 6-14) and a figure (in the supplement), which are:

"In our simulations, both the semi-direct and the indirect effects are taken into account, which act simultaneously on evaporation and on precipitation. An increase in the aerosol load may not result in an increase in LWMR, depending on the change in the entrainment in and around clouds due to the evaporation and precipitation of cloud liquid water (e.g. Hill et al., 2009; Toll et al., 2019). The differences between the two experiments (AE10 - AE1) are small for temperature and specific humidity, less than 1 % (with maximum hourly differences reaching 0.24°C and 0.0003 kg/kg respectively), and high for liquid water mixing ratio and rain mixing ratio, around -20 % (with maximum hourly differences reaching 10^5 and 10^7 kg/kg respectively), which suggests that the reduction in precipitation is mainly related to wetter clouds (Fig. A3). Nevertheless, with our experiments, it is not possible to distinguish the influence of the evaporation by the semi-direct effect and of precipitation by the indirect effect on liquid water because we cannot exclude possible opposing effects on temperature and humidity."

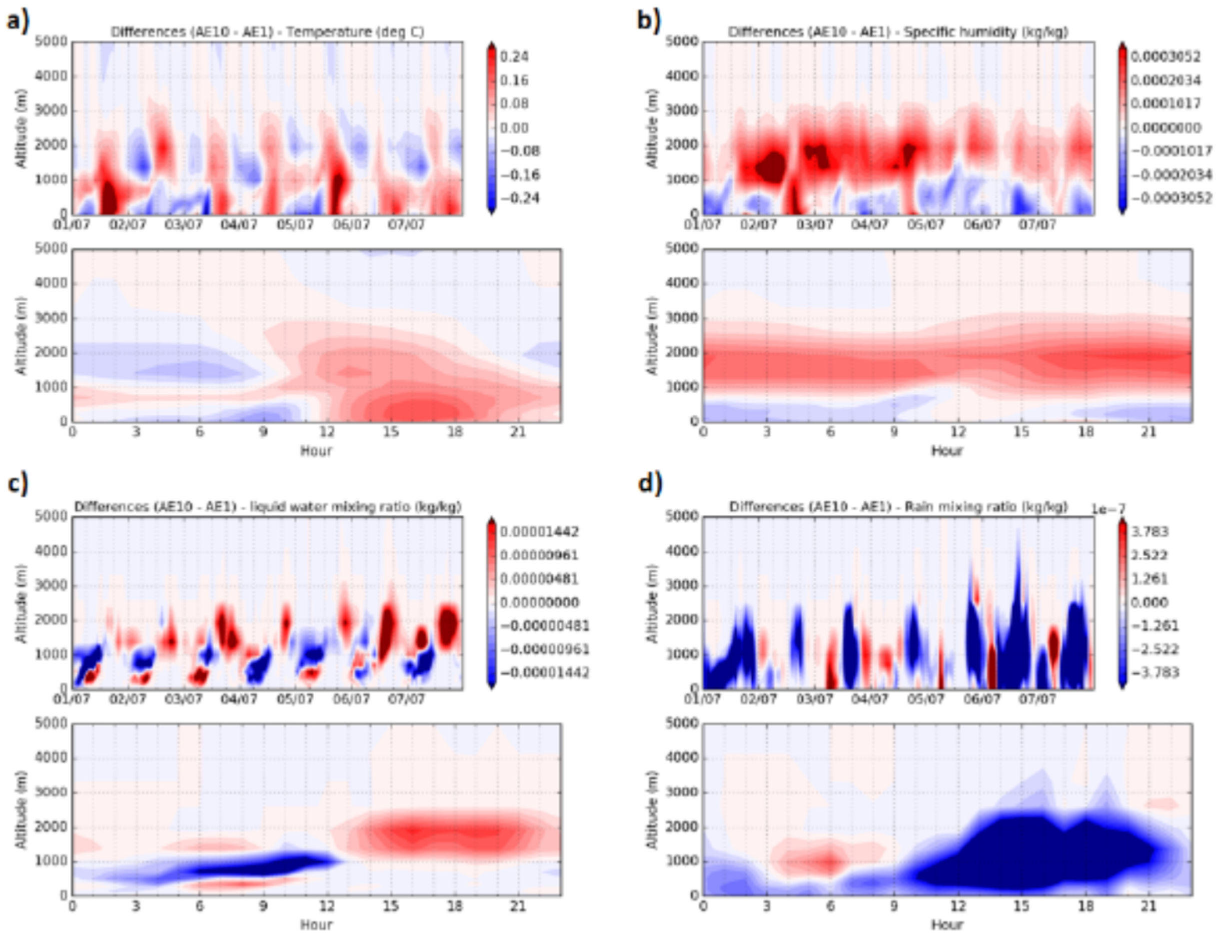

Figure A3. *Time series of vertical profiles of the differences between the two experiments (AE10-AE1) for (a) temperature, (b) specific humidity, (c) liquid water mixing ratio and (d) rain mixing ratio, averaged over an inland area ranging from 7°N to 9°N, and from 1°E to 4°E for (top) the period 1–7 July 2016, and (bottom) means of each hour for this period.*

Figure 8a: would be really nice to put MODIS or AMSR or SEVIRI liquid water path data on this plot, for times when you have the retrievals.

We thank the reviewer for this suggestion. We have included the LWP retrieved by MODIS (datasets: MCD06COSP-D3) in Figure 8.

A sentence has been added (Page 17, Lines 19-20): "*The modeled LWP agrees well with the LWP retrieved by MODIS because, for both, the minimum LWP occurs on July 2 and the maximum LWP occurs on July 6, although the model is slightly lower than the MODIS estimate.* "

The new figure is below:

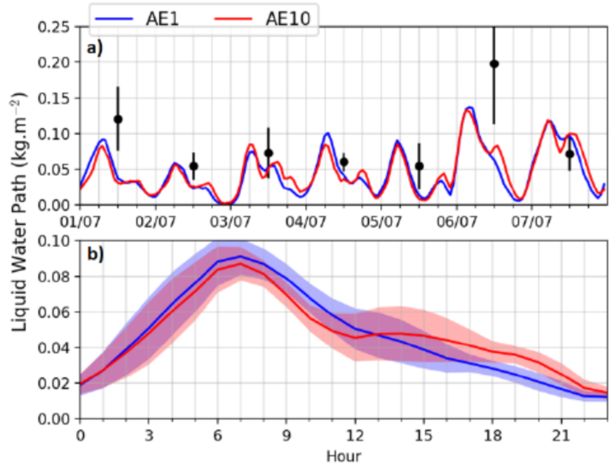

Figure 8. *Time series (a) and diurnal cycles (b) of modeled hourly liquid water path (kg.m^{-2}) for the two experiments, AE1 (blue) and AE10 (red), averaged over an inland area extending from 7°N to 9°N, and from 1°E to 4°E, and for the period 1–7 July 2016. (a) The vertical dashed lines indicate periods of 6 hours starting at 00:00 UTC. (b) Means of each hour for the period 1–7 July 2016 are presented by lines, and the upper and lower shading limits correspond to the hourly standard deviation. Daily averages of the Moderate Resolution Imaging Spectroradiometer (MODIS) data retrieved by the instruments on the Terra and the Aqua satellites (datasets: MCD06COSP-D3) are black dots with the error bars representing the standard deviation in the area.*

Page 20: did Menut et al 2019 give reasons for the low bias in precipitation? Do you have insights from this study?

The quantification of this precipitation bias is mainly a finding of the comparison between the observations and the results of the model. It was first quantified in Menut et al., (2015) (https://acp.copernicus.org/articles/15/6159/2015/acp-15-6159-2015.pdf) and found again in Menut et al. (2019). The results showed that the model produces precipitation more often but with a lower intensity than what is observed. It is difficult to identify the main reason: it is probably the mixture of several reasons: possible biases of other variables (wind, temperature, humidity), impact of the biases of parameterization of the convection and, possibly, the horizontal resolution smoothing the orography. Since the precipitation process is at thresholds, small biases in the input parameters can induce this type of change in the frequency of the phenomenon.

The sentence of the previous version: “Even if both state-of-the-art models and satellite products have difficulties in retrieving the precipitation in SWA, this comparison suggests that the model predicts too many days of low precipitation rate, as it has already been shown for the WRF model (Menut et al., 2019).” has been modified to (Page 20, Lines 27-29): “*Even if both state-of-the-art models and satellite products have difficulties in retrieving the precipitation in SWA, this comparison suggests that the model predicts too many days of low precipitation rate,* *as it has already been shown from comparisons between the observations and the results of the WRF model (Menut et al., 2015, 2019).*"

Page 22 line 16 and page 23 line 6: I think it is necessary to add a caveat "aerosols emitted from anthropogenic activities have a regional scale influence on LLC and precipitation IN OUR SIMULATIONS, both…." Modeling these aerosol-cloud interactions is not so easy, and there is no guarantee the model is right!

It has been done for both sentences.

For page 22 line 16, it has been changed as follows (Page 23, Lines 9-11 of the new version): *"To conclude, aerosols emitted by anthropogenic activities have a regional scale influence on the LLC and precipitation in our experiments by comparing AE1 to AE10, both inland and over the ocean. The differences for LLC between the two experiments are small on average, however there are contrasting periods during the day."*

For page 23 line 6, it has been changed as follows (Page 23, Lines 32-33 of the new version): *"We conclude that there is a moderate effect of anthropogenic aerosol emissions on low-level clouds and precipitation in SWA from the analysis of our experiments."*

Figure A3, A4 can you add horizontal snapshots of the cloud cover in the simulations at the same times over this area, or a subset of it? Ideally for both AE1 and AE10?

We added the hourly modeled LWP over the same area for both experiments in two new figures in the Supplement. The new figures are below (Fig. A5 and Fig. A7):

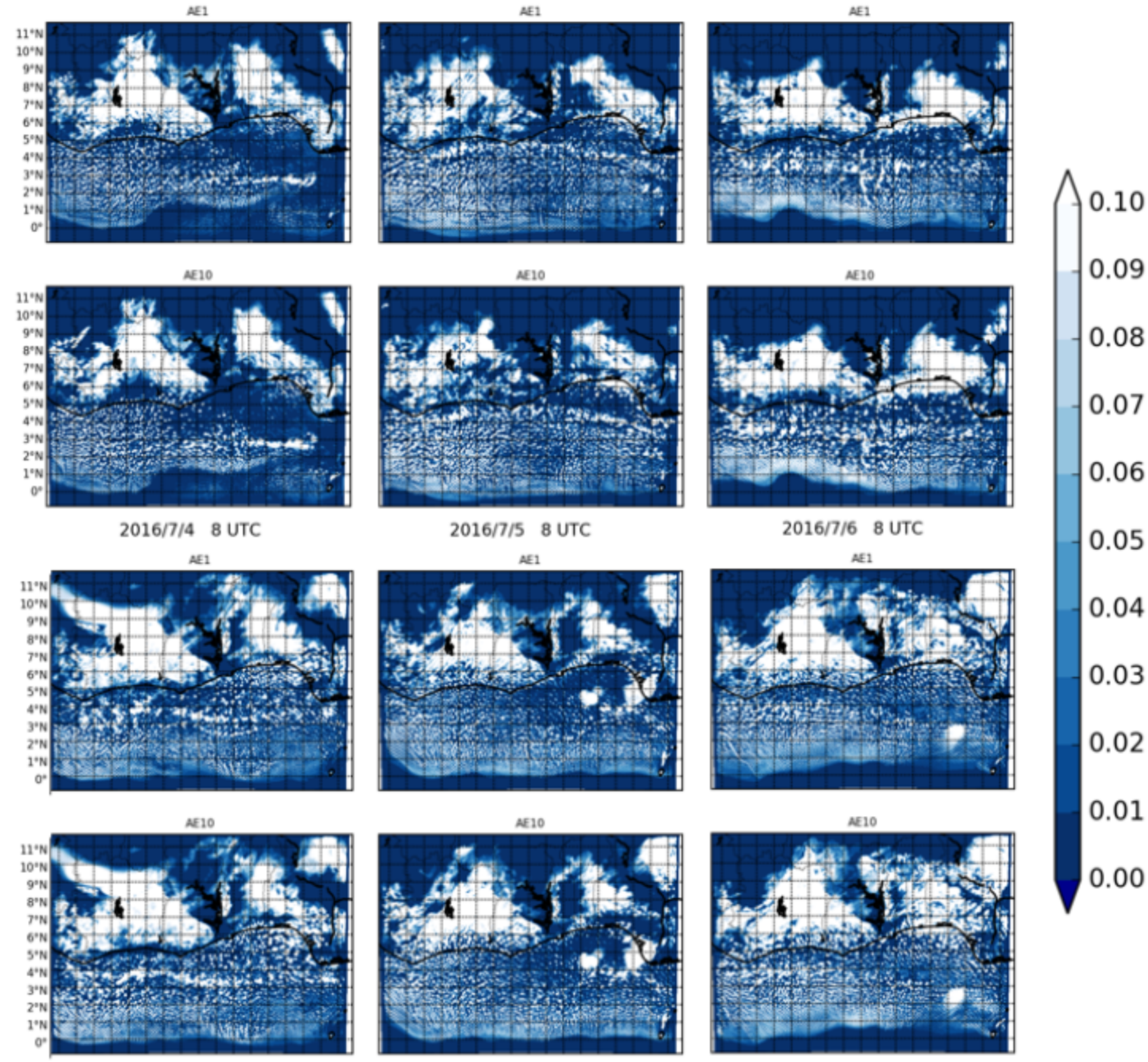

Figure A5. *Modeled Liquid Water Path over the studied domain at 08:00 UTC for the period 1–6 July 2016.*

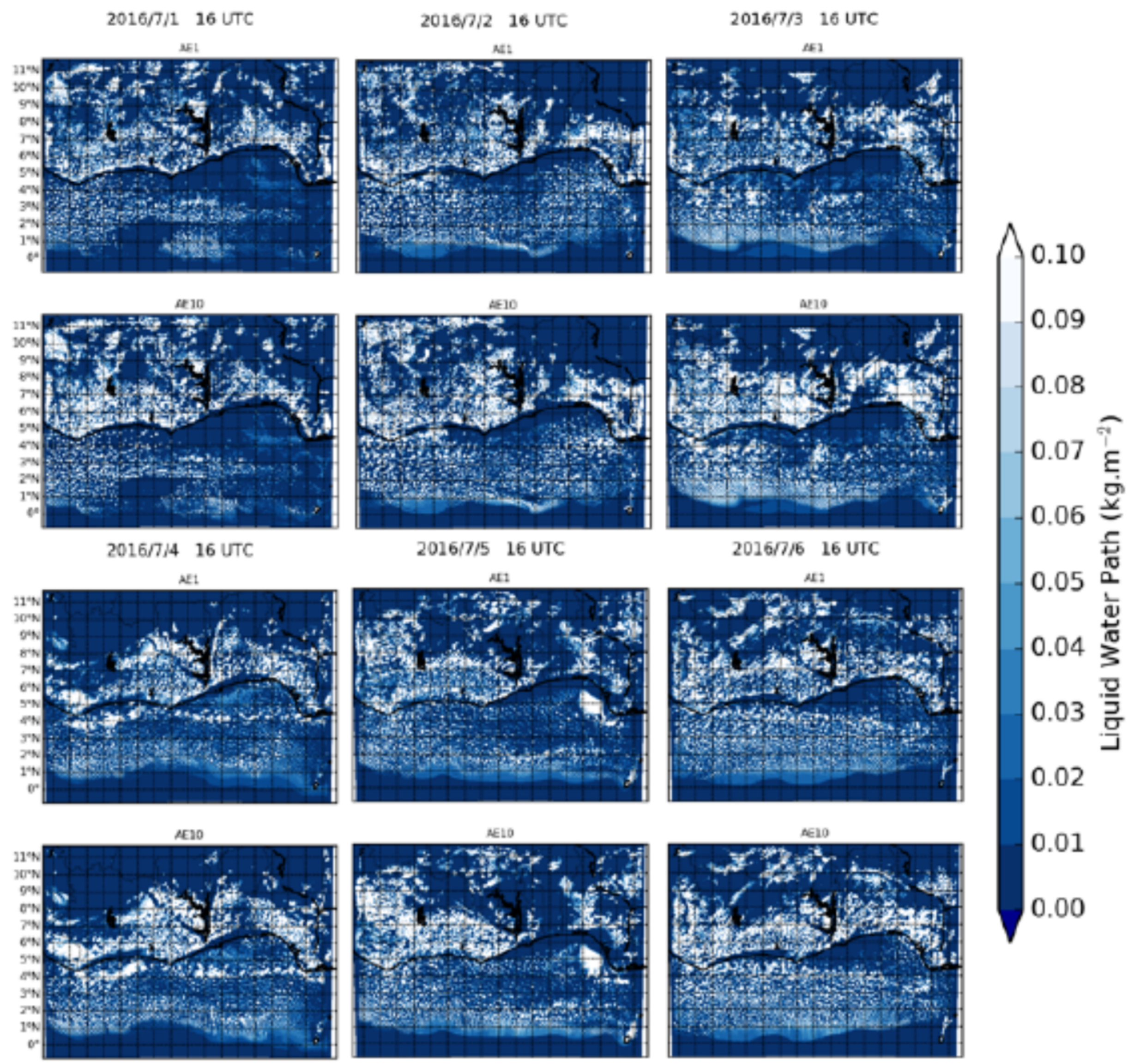

Figure A7. *Modeled Liquid Water Path over the studied domain at 16:00 UTC for the period 1–6 July 2016.*

Review #2

I have reviewed "Sensitivity of low-level clouds and precipitation to anthropogenic aerosol emission in southern West Africa: a DACCIWA case study" by Deroubaix et al. The title succinctly summarizes the study. The study suffers from the problem inherent in case studies, namely generalizability. But it is solid work, and I recommend publication after minor revisions to address my concerns.

My first concern is that the conclusions (precipitation suppression by anthropogenic aerosols delays the breakup of clouds) are mainly a reflection of the cloud physics included in the model. But models are by necessity incomplete. While this model includes a precipitation suppression mechanism via its precipitation microphysics, there are other aerosol effects that could lead to an enhanced loss of cloud cover through evaporation (e.g., Ackerman et al., 2004). These effects are unlikely to be correctly represented in a 5 km resolution model without convection parameterization because the relevant scales are much smaller for shallow clouds. Over all, the enhanced evaporation effect is as strong (Toll et al., 2019) or stronger (Gryspeerdt et al., 2019) than the precipitation suppression effect, but there is likely to be a great amount of diversity depending on cloud regime, aerosol loading, etc.

We thank the reviewer for their nice comments about the manuscript.

We agree that the representation of clouds is simplified with a 5-km resolution grid even with a scale-aware scheme (Grell and Freitas, 2014).

We also agree with the reviewer that the role of evaporation and precipitation on clouds is an important and interesting aspect that was not addressed in the previous version of manuscript. We think we would need more simulations and evaluations of the models in order to answer these questions accurately. That is why we have followed the reviewer's suggestion and we have conducted the analysis of our experiments more in depth.

Firstly, in Section 2, we have added a paragraph to explain how aerosol could affect the cloud lifetime and precipitation in the model (Page 5, Lines 5-9) : *"In WRF-CHIMERE, the aerosol load affects the cloud lifetime and the precipitation because it modifies the autoconversion from cloud water droplet to rain particles, which is based on the scheme of Berry and Reinhardt (1974). Moreover, it is also affected by the radiation absorption from dust or black carbon via the semi-direct effect (Briant et al., 2017). Over West Africa, the high aerosol load makes the integration of aerosol effects an important step towards understanding meteorological feedbacks during the monsoon (Menut et al., 2019)."*

Secondly, in order to address this important aspect, we have analyzed the differences between the two experiments for temperature, specific humidity, liquid water mixing ratio and rain mixing ratio. We show that the modifications of the temperature and specific humidity are small, while there are important modifications of the liquid water and rain mixing ratios.

In Section 5, we added a new paragraph (Page 18, Lines 6-14) and a figure (in the Supplement), which are:

"In our simulations, both the semi-direct and the indirect effects are taken into account, which act simultaneously on evaporation and on precipitation. An increase in the aerosol load may not result in an increase in LWMR, depending on the change in the entrainment in and around clouds due to the evaporation and precipitation of cloud liquid water (e.g. Hill et al., 2009; Toll et

al., 2019). The differences between the two experiments (AE10 - AE1) are small for temperature and specific humidity, less than 1 % (with maximum hourly differences reaching 0.24°C and 0.0003 kg/kg respectively), and high for liquid water mixing ratio and rain mixing ratio, around -20 % (with maximum hourly differences reaching 10^5 and 10^7 kg/kg respectively), which suggests that the reduction in precipitation is mainly related to wetter clouds (Fig. A3). Nevertheless, with our experiments, it is not possible to distinguish the influence of evaporation by the semi-direct effect and of precipitation by the indirect effect on liquid water because we cannot exclude possible opposing effects on temperature and humidity."

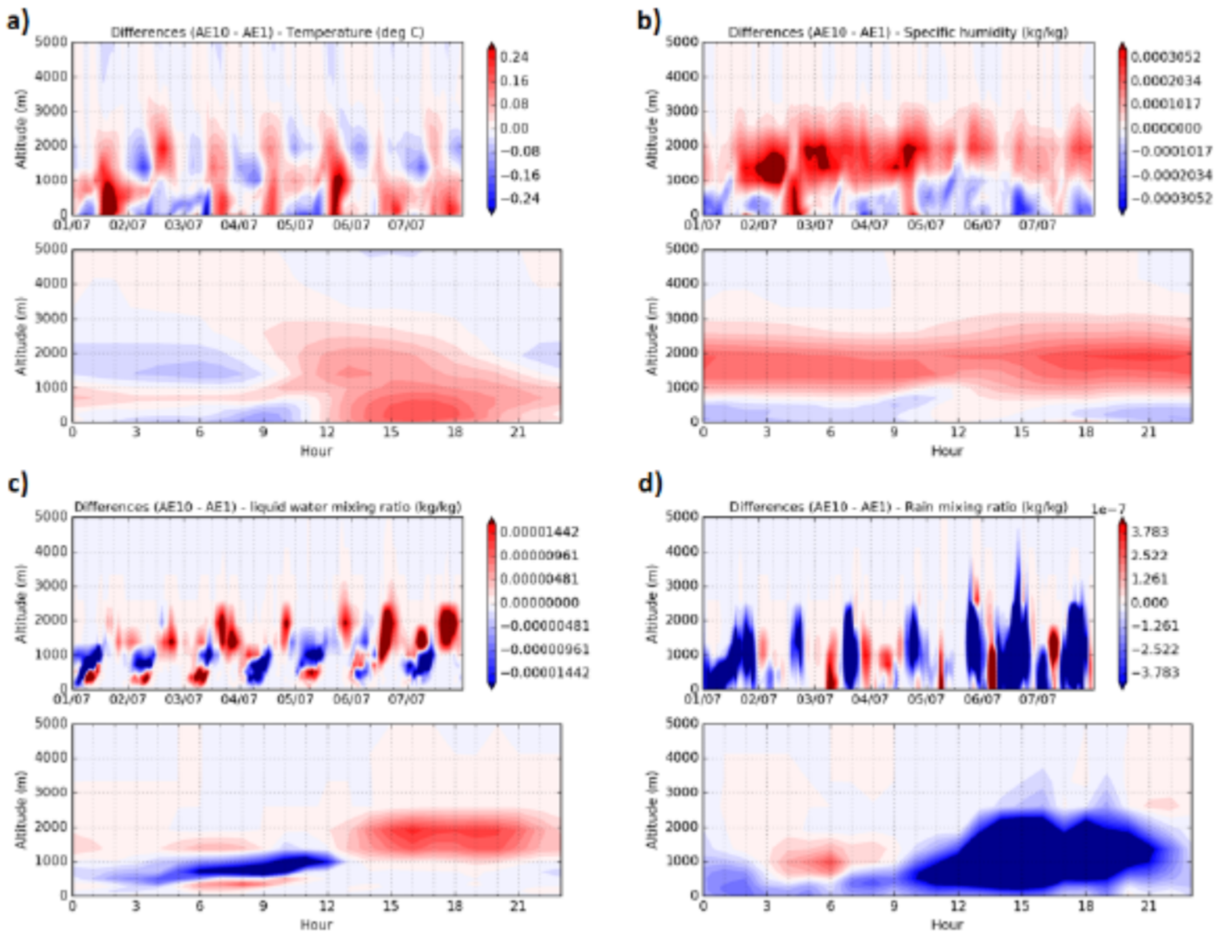

Figure A3. *Time series of vertical profiles of the differences between the two experiments (AE10-AE1) for (a) temperature, (b) specific humidity, (c) liquid water mixing ratio and (d) rain mixing ratio, averaged over an inland area ranging from 7°N to 9°N, and from 1°E to 4°E for (top) the period 1–7 July 2016, and (bottom) means of each hour for this period.*

To gauge how much to trust a model that only parameterizes the precipitation mechanism, it would be extremely helpful to know whether the breakup of the clouds discussed in this case study is mainly evaporation-driven or precipitation-driven to begin with. This is of course easier said than done, because we don't have observations of evaporation flux. But a good starting point would be to ask the model: what fraction of the LWP tendency can be explained by evaporation and what fraction by precipitation? If precipitation plays a sizable role in the cloud dissipation in the model, then the next question is whether the precipitation timeseries shown in Fig. 10 agrees with observations. Therefore, I was disappointed that Fig. 10 does not include any observations at all. It would also be useful to include more description of these clouds; I assume they are fairly deep (but still warm) cumulus clouds for which precipitation dissipation is a reasonable assumption, but the onus is on the authors to make this argument.

It is not easy to add observational data to validate the modeled hourly precipitation rate because there is no hourly retrieval available by satellite. It might be possible to use radar data or the data measured at a station.

For radar data, the measure of precipitation is not very accurate (from GPM IMERG and CMORPH, hourly precipitation retrievals could be calculated but in Maranan et al. (2018) have shown that the accuracy is low). For station data, the network of Benin and Togo provide only daily precipitation rates. However, in the framework of the DACCIWA project, three sites have been installed with measured hourly precipitation rates. Over the entire campaign period, it has been shown that the precipitation mostly occurs from 18:00 to 21:00 (Kalthoff et al., 2018), which is in agreement with our simulations. Moreover, in Figure 11, the modeled daily precipitation rates are compared to TRMM satellite data.

Therefore, since it is difficult to validate the modeled hourly precipitation rates, we consider that the analysis of the role of evaporation and precipitation on clouds would be too speculative using our experiments. We would also need more constraints on evaporation, which is not available.

My second concern is representativeness. Recognizing that aerosol effects on LWP and cloud cover tend to be subtle and can have either sign, it is hard to draw a general conclusion from this study, even setting the model correctness concern aside for the moment, and I am struggling to identify anything new that I have learned from reading the manuscript. This is of course a general problem of case studies with no easy solution. Ideally, the manuscript would make connections to other work, e.g., longer time period regional modeling, and discuss how this analysis corroborates or modifies conclusions of those longer-term studies. Another approach would be to perform an ensemble of model runs for this case study to explore how robust the conclusions are to meteorological variability or model physics (depending on how the ensemble is designed). Model runs (especially ensembles) are not free, so I do not expect the authors to come up with additional analysis. However, I think it is important for the authors to at least discuss representativeness in the final paper.

The goal of the manuscript is to quantify the general performances of the model in SWA where few observational data are available. The second objective is to evaluate the sensitivity of an increase of aerosol, thus CCN, on LWP and precipitation in an area where the aerosol load is high and a complex mixture.

We agree with the reviewer, if we leave aside the evaluation of a coupled state-of-the-art model (in a complex region with high aerosol load and coastal meteorology during the monsoon period), the main result is quite specific, as it concerns the moderate but significant impact of anthropogenic emission on the diurnal cycles of cloud and precipitation in SWA.

However the study corroborates other studies more theoretical, which have included less evaluations (e.g. Deetz et al., 2018; Pante et al. 2020), and which are mentioned in the discussion section. Moreover this article also proposes a framework to compare a coupled model and observation (using data from aircraft, stations, radiosondes and satellites) focused on clouds which is, to our knowledge, not common.

The tendencies explained by evaporation and precipitation on LWP would need microphysical validation to be reliable (which is possible with DACCIWA data but could almost be a new study). In addition, given the vertical resolution of regional models, the role of entrainment in and around clouds may not be well represented. Therefore, we consider the quantification of LWP tendencies beyond the scope of this study, although this is a very valuable question that will need to be answered in the future.

We agree that conducting an ensemble of coupled models would be the best approach, but we consider this is beyond the scope of this particular study.

Review #3

Review of “Sensitivity of low-level clouds and precipitation to anthropogenic aerosol emission in southern West Africa: a DACCIWA case study, by Deroubaix et al.

This manuscript presents a nice study on aerosol impact on cloud cover and precipitation in the southern West Africa. To evaluate the effects, the authors use the combined Chimere-WRF model, and first compare the model results against observations. They find that increased aerosol loading has moderate effect on precipitation and cloud cover. The main findings are changes in cloud breakup time and precipitation timing, and with increased aerosol loading leading to slightly reduced precipitation. The paper is well written, clear to understand and I recommend publishing after addressing some minor issues.

We thank the reviewer for his kind comment concerning the manuscript.

Minor comments:

1) Page 4, line 8: By adding spectral nudging, is it possible that some of the aerosol effect on dynamics is lost? This comes up again on page 13, lines 3-5. I would not expect the aerosols to have a large effect on the rh and wind, but could spectral nudging also reduce any impact (specifically on the wind)? Perhaps you could add a small discussion regarding how spectral nudging impact aerosol indirect effect evaluation.

We are grateful that the reviewer points out the problems linked to nudging in simulations using coupled models. Indeed we made a preliminary test and the nudging needs to be turned off below the cloud top height (which reaches 2 km altitude during the day), otherwise the diurnal cycle of low level clouds was inaccurate. So the nudging has an important impact, and this information was missing.

The spectral nudging is used only above the eleventh level above the surface, which corresponds to an altitude of about 2 km amsl. This information has been added in a sentence Section 2.1 (Page 5, Lines 12-15): *"The 32 vertical levels of WRF from the surface to 50 hPa are projected onto the 20 levels of CHIMERE from the surface up to 200 hPa, which includes 12 levels below 2 km where the LLC are located, this is why spectral nudging is only applied for vertical levels above the twelfth level."*

2) Page 20, line 9: Could the relative difference between AE1 and AE10 be larger over the ocean because the aerosol loading there is lower (cleaner)? Large changes in aerosols over a clean area is likely to impose a larger effect compared to increase aerosol loading in an already polluted area.

We thank the reviewer for this comment. We have modified this sentence such as (Pages 20-21, Lines 31-2):*"The relative difference between the two experiments is greater over the ocean in the Area-5-7N for LWP reaching 13.2%, and for precipitation rate reaching -10.0%, which shows the importance of anthropogenic aerosol transported over the ocean where the aerosol load is lower than over the two other areas."*

Technical comments:

Page 2, line 14. I would add Thompson and Eidhammer (2014) here as well.

Yes, it has been added in the new version.

Page 2, line 32-33. This sentence is hard to read. Please rephrase.

We have modified from: *"The interactions between the nocturnal low-level jet and the LLC have been studied with an observational campaign deployed in SWA, including aircraft (Flamant et al., 2018b), radiosondes and three super-sites that had been instrumented to monitor the LLC diurnal evolution (Kalthoff et al., 2018)."* To (Page 2, Lines 29-31): *"The interactions between the nocturnal low-level jet and the LLC have been studied during the DACCIWA campaign deployed in SWA, using data from radiosondes, aircraft (Flamant et al., 2018b), and three super-sites (Kalthoff et al., 2018)."*

Page 3, line 9. Change Inflow to inflow.

Thanks. It has been done.

Page 4, line 15: Perhaps add Iacono 2008

Thanks. It has been done.

Page 6, line 8. Why is sulfate not being evaluated?

Sulfate concentration has not been presented because of the large bias present in our model as well as in other models. The emission inventories available for West Africa are probably inaccurate for SO2 because of the emission factors (e.g. Elguindi et al., 2020; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020EF001520).

Page 10, line 7. I would suggest to shortly describe how RH affect aerosol optical properties.

This sentence was unclear because we do not analyze the relationship between aerosol optical properties and RH. We want to analyze the effects of aerosols on meteorology. With a higher aerosol load, we expect a modification of temperature, thus the stability of the atmosphere, inducing change in the wind and in the relative humidity among others. We choose these two variables because the shape of the RH and WS vertical profiles are well understood and described (*e.g.* Menut et al., 2019; Deroubaix et al 2019).

The sentence: *"We select two major variables influencing the aerosol optical properties and transport, namely relative humidity (RH) and wind speed (WS)."* has been modified to (Page 10 Lines 23-25): *"We select two meteorological variables, namely relative humidity (RH) and wind speed (WS) for which the vertical profiles have already been described by e.g. Deroubaix et al. (2019)."*

Page 19, line 23. Please refer to Figure 1 here.

We thank the reviewer for this suggestion. We have added a reference to Figure 1.

Page 22, line1. After 12:00 UTC , you could add "the next day"

We agree with the reviewer and it has been added.

References

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