Responses to Referee 3

Daytime low-level clouds in West Africa – occurrence, associated drivers and shortwave radiation attenuation *by Danso et al.*

On behalf of all the authors, I wish to thank the reviewer for the thorough assessment of our study and for providing us with comments to improve the manuscript during the revision. Please see below the detailed responses to each of the reviewer's comments/questions. The reviewer's comments are shown in black font while our responses are shown in blue font. Where applicable, the changes that would be made in the manuscript are shown in italics.

This manuscript investigates daytime (06-17h) low-level clouds (LLC) over West Africa based on ERA5 (2006–2015) in two regions, the Sahel and Guinea Coastal region. LLC taken from the ERA5 archive describes cloudiness below 800 hPa (~2km). Foci of the study are on diurnal cycles in the dry and wet seasons, the seasonal variation of LLC, the atmospheric conditions related to different classes of LLC and the impact of the latter on the incoming solar radiation.

While the article describes an overall interesting topic and contains some interesting material, it also has weaknesses that shall be addressed in a major revision.

We are grateful to the reviewer for his overall positive comments on our study.

1) There needs to be a proper explanation between ERA-5 cloud fraction that usually depends on the subgrid-scale cloud scheme and the liquid and ice water paths that are relevant for the radiation scheme. Cloud fraction can be larger than zero for relative humidities below 100% and zero liquid or ice water content. There is an interesting discussion on this in Hannak et al. (2017, JCLIM). Thus cloud occurrence frequency at a gridpoint can be defined by a sub-grid scale cloud fraction or a hydrometeor content > 0. There needs to be explanations/discussion of this issue (see below).

   Thank you for this comment. We will discuss this in the data presentation in our revised manuscript. Please also see the last paragraph of our response to your major comment #3 and Figure 4 in this document which confirms your comment regarding the definition cloud occurrence frequency.

2) My major concern that also needs some time in the revision relates to the degree of realism that hourly ERA5 data have in the representation of LLC. There was no evaluation of ERA5 in Danso et al. (2019), only a comparison of total cloud cover (all clouds, all levels) based on a very coarse cloud fraction partitioning of METAR reports for three stations in West Africa. From this one figure in the Suppl. Mat. it is not obvious that the CERES data set would be inferior to ERA5 – which it likely is according to the findings in Danso et al. I am very worried about that this study may show physically consistent errors of the underlying ERA5 model. For example high LLC fractions/frequencies may be related to errors in the Bown ratio in the ERA5 model etc (with causality being another point of concern). The Reviewer proposes two ways out of it: One is to use available ground and satellite observational evidence that exists, the other is to use two other re-analyses (e.g. MERRA2, JRA-55, NCEP). In terms of the former, van der Linden et al. (2015) have shown the usefulness of the 2B-GEOPROF-

LIDAR tracks for cloud occurrence frequency in mean, 250m vertically resolved profiles, including the layer below 2 km (their Figure 6). The sampling argument given in the manuscript is not robust, as is the argument of the 1x1 grid resolution needed for PV application – the purpose is to validate the usefulness of ERA5 cloudiness and for this this the combined Cloudsat-Calipso at 01:30 LT would serve the purpose. Moreover, multi-year measurements of solar incoming radiation are available from AMMA-CATCH (http://www.amma-catch.org/?lang=fr), for the Upper Ouémé site even multiyear measurements of sensible and latent heat fluxes can be obtained. Radiation measurements for Parakou and Cotonou are also available from doi: 10.6096/baobab-dacciwa.1785. Kniffka et al. (2019, ACP) have shown large errors in surface solar radiation in ERA-I and it is questionable that this has been improved a lot in ERA5. Kniffka et al. (2020, QJRMS) have also shown that short-term forecasts of weather forecasting models, among which is the ECWMF IFS model have large errors in precipitation, radiation, and cloud cover. So there are strong arguments to validate ERA5 before drawing far reaching conclusions. I prefer to use the few, but available observational evidence, but using two other reanalyses also allows inferences about the fidelity of the results. Clearly, I do not want the author to go to deep into validation, but some more validation is necessary (for some observational points and subperiods of 2006–2015).

We thank the reviewer this comment and the suggestions given.

In the revised manuscript, we will show some validations of the ERA5 data and will discuss them. We intend to show this in an appendix that will be added to our manuscript.

Cloud cover evaluation:

We will use the 2B-GEOPROF-LIDAR product to perform an evaluation of the ERA5 clouds in our study region. We have already started with the evaluation of ERA5 LLC data. This is explained below:

The ERA5 cloud fractions are instantaneously generated at every hour of the day. However, the instantaneous 2B-GEOPROF-LIDAR observations over the study area are not recorded exactly on the hour (e.g., in Figure 1 below, the 2B-GEOPROF-LIDAR passes over whole WA in ≈ 7 minutes from 14:08 to 14:15). In order to compare ERA5 with the satellite observations, we compute the mean cloud fraction of the two hours bounding the 2B-GEOPROF-LIDAR observation time (i.e., in this case 14 and 15UTC, figure 1c and 1d below). The closest grid points to the 2B-GEOPROF-LIDAR observation track are selected from the ERA5 dataset for this evaluation.

As already indicated, the satellite moves along its track over WA in approximately 7 minutes. This means that the time taken for the satellite to pass along its track within the Guinea or Sahel window will be much smaller. Therefore to make the evaluation of ERA5 with 2B-GEOPROF-LIDAR in each of the windows, the mean cloud fraction along the part of the satellite track passing through that window is used. The difference between the maximum and minimum longitude (latitude) along the track passing through each window is approximately 0.5° (3°).

We then compute the mean bias error (bias) and root mean square error (RMSE) between the 2B-GEOPROF-LIDAR observations and ERA5. Please note that this evaluation is based on only data from June to December 2006. In the proposed appendix, we will perform this evaluation with a longer time series of the 2B-GEOPROF-LIDAR observations.

Please note that the overall LLC from 2B-GEOPROF-LIDAR vertical layers (from surface to 2km) was computed using the maximum-random overlap rule (Geleyn and Hollingsworth 1979) which is used by the ECMWF for computing cloud fractions.

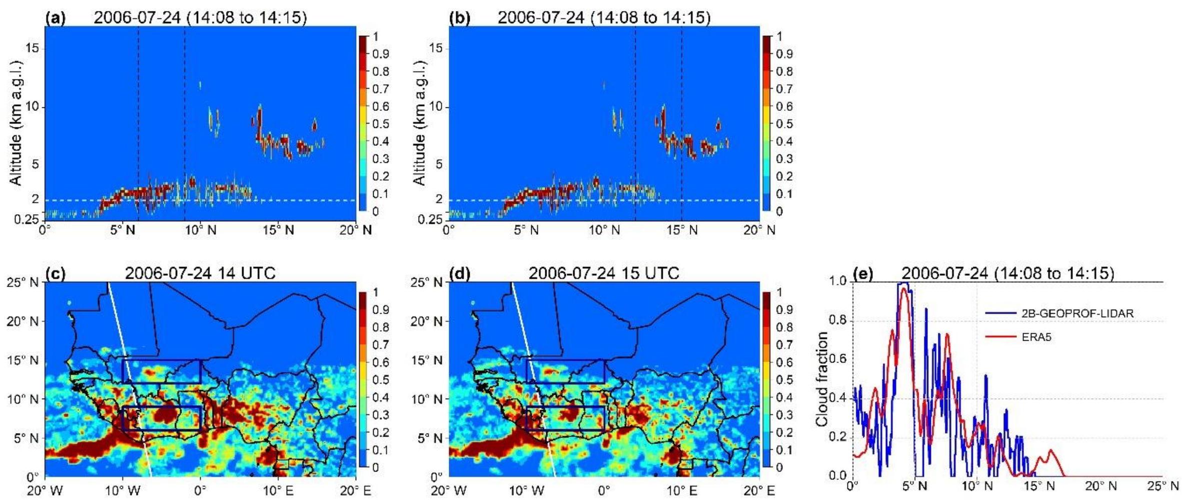

Figure 1: Top: 2B-GEOPROF-LIDAR vertical profile of cloud fraction (a, b) for a given daytime track. (a) and (b) are the same but black dashed lines delineates the locations of the Guinea and Sahel windows respectively. All cloud structures below the white dashed horizontal line, are considered as LLC in our study. Bottom: Corresponding ERA5 2D plots of LLC for the hours (c) preceding and (d) following the satellite observation time. The white line represents the satellite track for this particular observation time. (e) A comparison the latitudinal cloud fraction seen along the 2B-GEOPROF-LIDAR track and the corresponding ERA5 closest grid point.

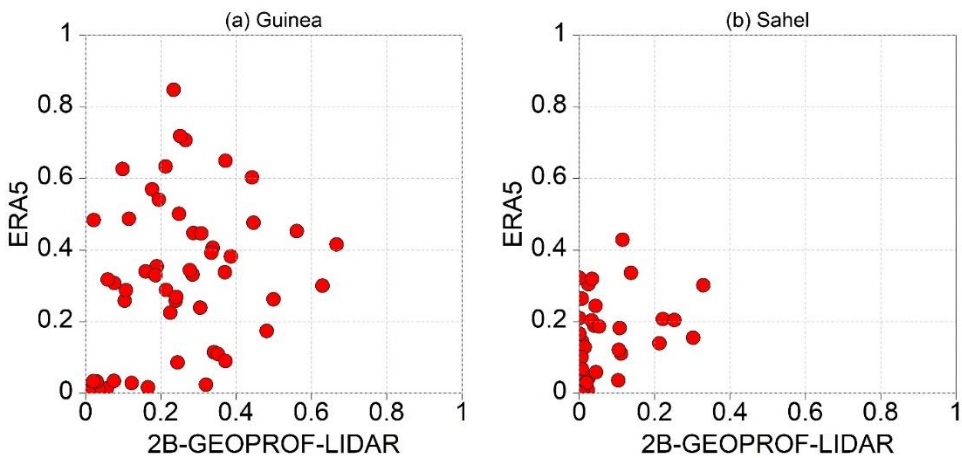

Figure 2: ERA5 vs 2B-GEOPROF-LIDAR cloud fraction for all satellite observations from June to December 2006 in the (a) Guinea and (b) Sahel windows.

Table 1: Bias and RMSE of ERA5 LLC against 2B-GEOPROF-LIDAR observations. Evaluation is done for only the daytime passes of the satellite from June to December 2006

	GUINEA	SAHEL
BIAS	0.072	0.056
RMSE	0.237	0.118

Although the ERA5 hourly low-level clouds data present some biases and deviations from the satellite observations as shown above, it performs reasonably well in reproducing the LLC observations.

Radiation evaluation:

We will also use the surface observations from the AMMA-CATCH DB to perform an evaluation of the ERA5 surface incoming solar radiation. We will also include other datasets such as SARAH2, CERES and MERRA2 in order to compare the relative performance of ERA5. Figure 3 shows the evaluation of ERA5 against the surface observation performed for one station. Details on this evaluation will be provided in the proposed appendix to be included in the revised manuscript, as well as evaluation results from other stations with data available. We will also include a Taylor's diagram for the validation of ERA5 surface radiation data.

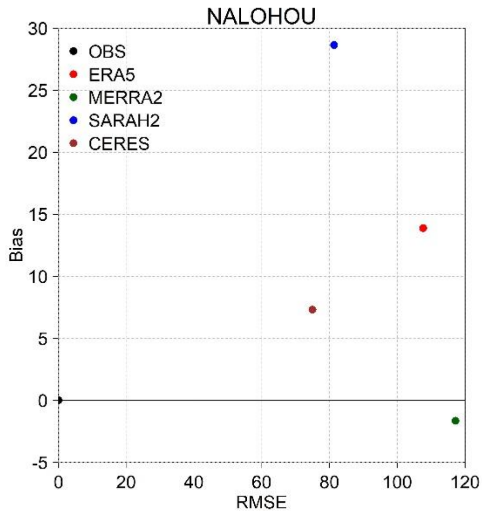

Figure 3: A plot of bias against RMSE for ERA5 (and other datasets) against surface observations at Nalohou in Benin. The period for this evaluation is 2006 to 2015.

With regards to the latent and sensible heat fluxes, the available time series from the AMMA-CATCH stations have a lot of missing dates (percentage of missing data ranges from 45% to 100%). We feel this will make any evaluation of the ERA5 data with these observations will be unreliable.

3) LLCs and MCSs
Having been in the region many times, I can't understand why MCSs should explain a large fraction of LLCs in the Sahel, for example. There are LLCs in the "small" leading edge/in the convective part of the MCSs, but not in the stratiform part. And MCSs are relatively infrequent. LLCs in the rainy season over the Sahel occur in the morning, but dissipate in the afternoon when isolated Cu cong or CB develop. Please comment on this.

We thank the reviewer for this comment. In our manuscript, we did not generalize that all LLCs in the Sahel are likely explained by MCSs. When we said "*It is, therefore, reasonable to assume that most of these LLC Class-2 events in the Sahel are the well documented deep MCSs which are responsible for around 90 % of total rainfall in the region*" we were referring not to the Class 1 but the Class 2. The total number of the Class 2 events are very low compared to the Class 1 (with an occurrence ratio of about

13:1). In other words, the Class 2 LLCs are infrequent in the Sahel region just like the MCSs, as you mentioned.

We however, agree with your point on LLCs and MCSs and we also acknowledge that the link we made between LLC Class 2 and MCSs is perhaps not entirely “accurate”. We believe this link could be best explained by the following statement which will be introduced in the revised manuscript:

“*The probability of an MCS event is therefore higher during the occurrence of LLC Class 2 events than Class 1 events.*”

We have made some plots to illustrate this point. Figures 4 and 5 below show the vertical profile of the cloud water (liquid + ice) of 12 random events of LLC Class 1 and Class 2 respectively. Based on the work of Storer et al. (2013), the definition of an MCS is a column containing cloud water ≥ 0.01 $g \cdot kg^{-1}$ (red dashed line) through a continuous depth of at least 8km. These plots suggest that the likelihood of having a deep system is higher in the Class 2 events than in the Class 1 events. From the 12 sub-plots, we count around 5 cases where the cloud water is equal or higher than the threshold value starting from the lower levels and extending for at least 8km upwards. Again, we can also see the point you made about having LLCs in the convective part of an MCS in the plots.

In the revised manuscript, we will clearly highlight these points (especially the higher likelihood of an MCS occurrence during Class 2 events) and add these two figures in an appendix.

We also believe Figure 4 confirms your major comment #1 in a way. For example, we see in many of the sub-plots that water content is zero in the first 2km, however, these are all cases when cloud fraction is not zero. We will also use this figure to define the cloud occurrence frequency based on the cloud fraction rather than the hydrometeor content.

We thank you for these comments.

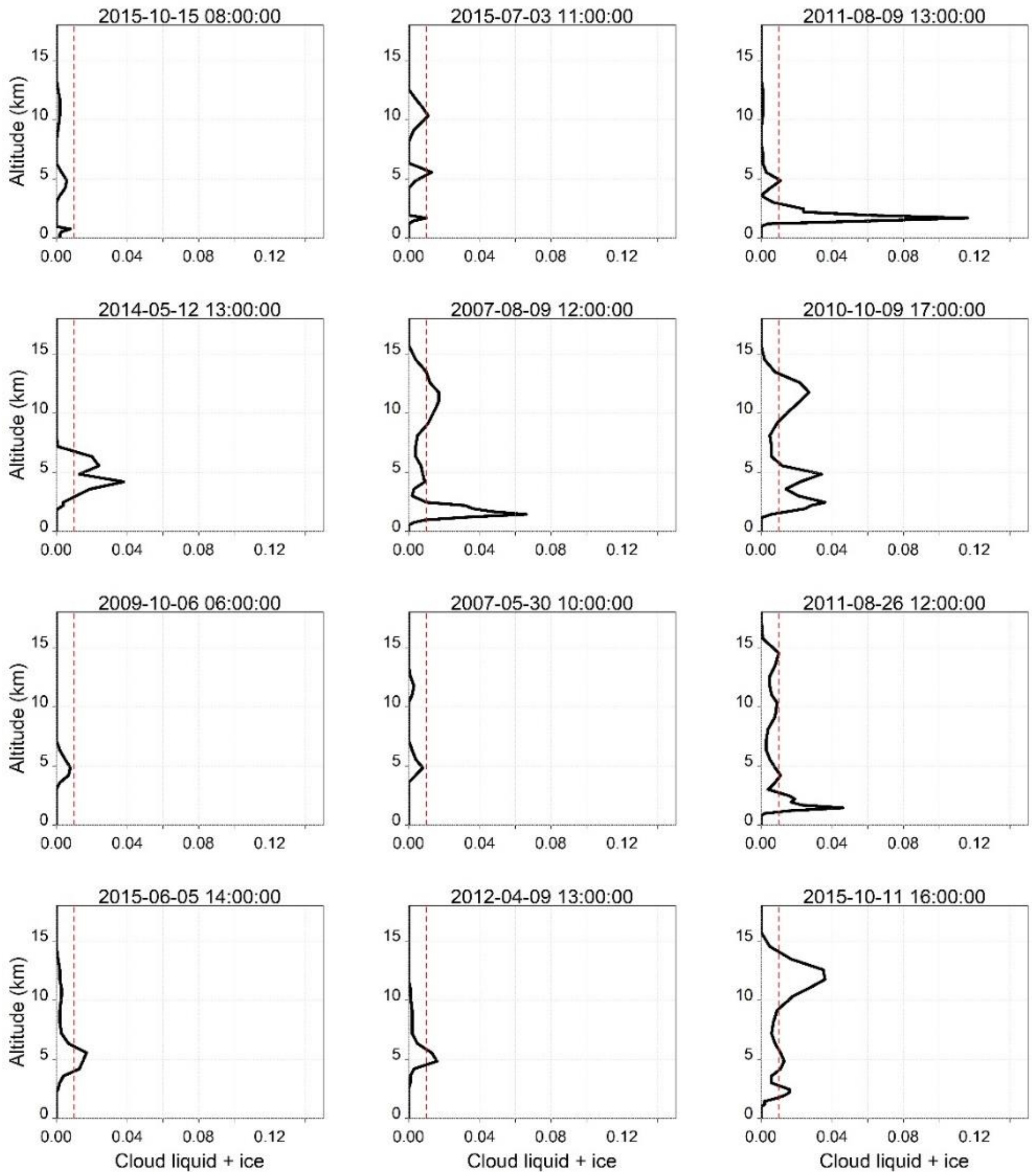

Figure 4: Vertical profiles of cloud water content (liquid + ice) during the occurrence of LLC Class-1 events in the Sahel region. The 12 subplots are randomly extracted from the Class 1 events. The red dashed line shows the threshold for deep convective clouds as defined by Storer et al., (2013).

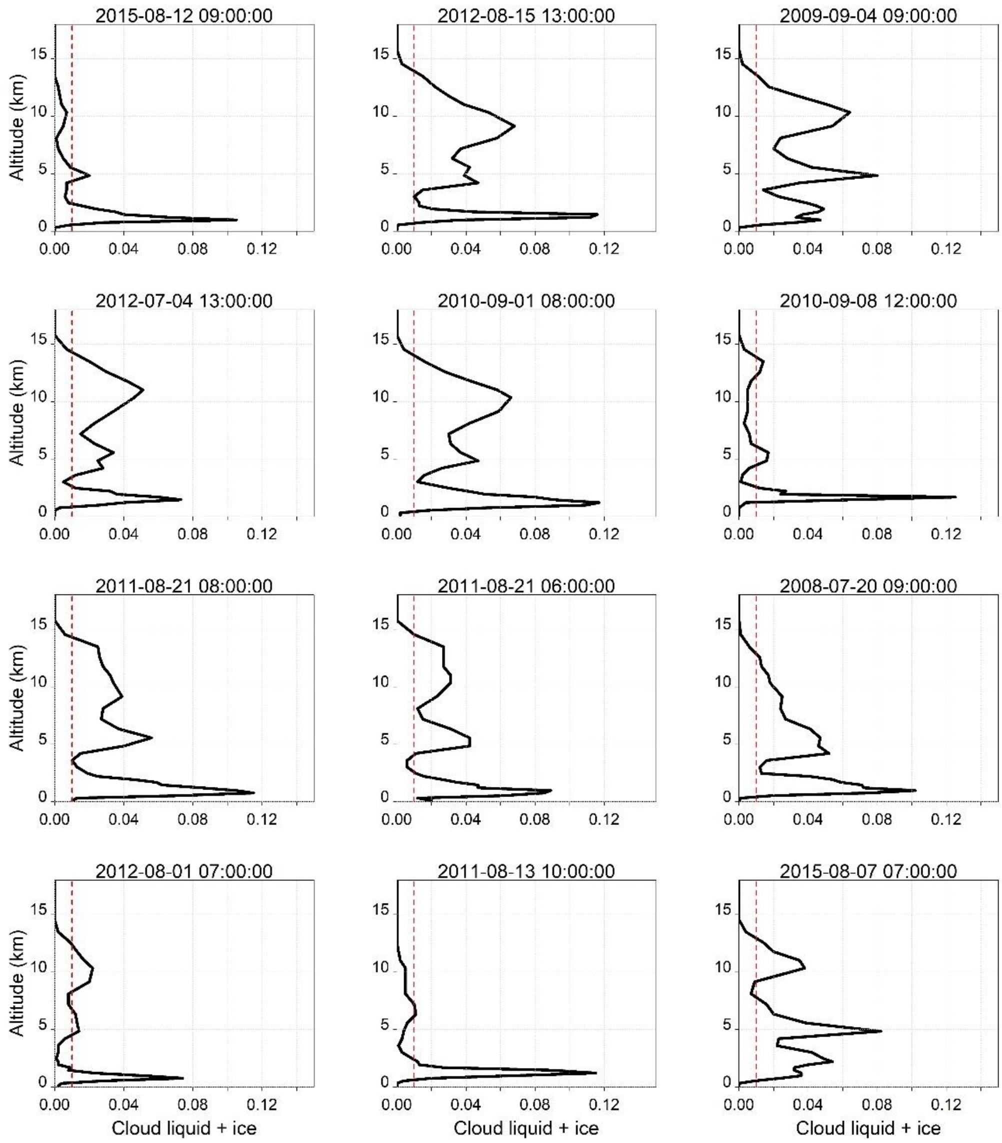

Figure 5: Vertical profiles of cloud water content (liquid + ice) during the occurrence of LLC Class-2 events in the Sahel region. The 12 subplots are randomly extracted from the Class 2 events. The red dashed line shows the threshold for deep convective clouds as defined by Storer et al., (2013).

4) I have not corrected all language errors and some statements are not very clear. The author should go over the manuscript meticulously in the revision to account for this deficiency.

Thank you. In the revised version, we will check for English language errors and correct them. We will also try to rephrase some sentences when possible, to make the discussion clearer.

Minor comments:

l. 29: “efficiently represent convection AND CLOUDINESS”

Thanks. The sentence will be revised to include “cloudiness”.

l. 35: “escalation” is not the right wording

The sentence will be revised. It will now read as:

“In the light of the recent increased interest in solar energy projects in WA…”

l. 37: “remains”

Noted and corrected. Thank you.

l. 40: prefer references in a chronological order.

Well noted. This will be revised.

l. 40: “van deR Linden”, please correct.

Thank you for this correction. It will be revised.

l. 40: “Farther north”

This will be corrected. Thanks.

l. 43: “persisting into the early afternoon”

This will be corrected. Thanks.

l. 43: “as shallow convective clouds”?????

This sentence will be re-written as:

“These LLCs consist of stratified clouds, most of which are nocturnal low stratus clouds covering wide areas and persisting into the early afternoons (Babić et al., 2019a; Schuster et al., 2013), as well as shallow convective clouds.”

l. 45-47: Here a reference to Kniffka et al. (2019, ACP) is appropriate.

Kniffka et al. (2019) will be referenced in this sentence in the revised version.

Thanks.

l 50: “The majority if these studies”

Thanks. The sentence will be revised.

l. 63: “limited TO the WAM season”

The sentence will be corrected. Thank you.

l. 84: “A better reference to ERA5 is now Hersbach et al. (2020, doi: 10.1002/qj.3803)

Thank you. We will use this reference in the revised version.

l. 101 “their surface heat fluxes explore”, awkward sentence

This sentence will be rephrased. In the revised manuscript, it will read as:

"*Other ERA5 variables are analyzed to show some of the atmospheric and surface conditions during the occurrence of LLCs in order to understand the possible interactions between the surface and the lower levels of the atmosphere.*"

l. 147 "horizontal air divergence", omit "air", perhaps add "wind".

Thanks. This will be revised. Then sentence will now read as:

"In addition, the horizontal divergence of wind, and anomalies of q are analyzed together with Q_{flux}*."*

Please note that we will change the symbol of moisture flux in the previous manuscript from Q_{adv} to Q_{flux} because the former may appear to suggest that the moisture advection was computed whereas the computed quantity was actually the moisture flux.

l. 182 Reword "convected air"

Please the sentence will now read as:

"The proximity of the Guinean region to the ocean means that cold air from the Gulf of Guinea could be advected inland which will, in turn, enhance LLC formation."

l. 188-192: Doubt that MCS contribute to LLC in reality (see major comment 3)

Please see our response to major comment #3.

l. 202-204: What about the contribution of morning LLC?

Most of the morning LLC may probably be non-precipitating during that period.

l. 212: Did Mathon et al. (2002b) really refer to LLC below 2km?

Mathon et al. (2002b) did not explicitly refer to LLC below 2km. They studied organized convective systems, some of which may have their bases below 2km.

l. 232: the adverb "predictably" seems inappropriate here. Please rephrase.

Predictably will be removed. The sentence will now read simply as:

"The horizontal advection of moist air is stronger during the occurrence of LLC Class-2 events than LLC Class-1 events."

l. 234 "...cold moist". Sentence terminates awkward.

Please the whole sentence will be revised based on comments from the other reviewers. The sentence will now focus on the moisture flux rather than moisture advection.

l. 240-242: q anomalies transported from the Atlantic in DJF and modulation by the WA heat low? The DJF heat low is somewhere over the Central African Republic/South Sudan at this time of the year. Please clarify.

The WAHL bit of the sentence was not correct. The sentence will be revised. It will read as:

"Nevertheless, the LLC Class-1 events during DJF (in Sahel) are related to positive q anomalies that seem to have been transported onto the continent from the North Atlantic (see

Fig. A1a in Appendix) by anomalous north westerly winds but this needs to be investigated further."

Thank you.

l. 248: Very awkward explanation of divergence. Usually, the divergence of the 2-d wind field is a good approximation of mass divergence since horizontal gradients of density are small. Please rephrase.

This is actually the definition of horizontal divergence as provided by the ECMWF. Please see https://apps.ecmwf.int/codes/grib/param-db?id=155

Section 5: The liquid and ice water content is relevant for attenuation. I am pretty sure it is not the subgrid scale cloud fraction (please clarify this point here or in the data section, see major comment 1).

Thank you. As noted in major comments #1 and #2, we will mention this point in the revised manuscript.

l. 363-365: "Other..." these processes or better features are only relevant in the wet season, not the dry season. Please mention this here.

Thank you. This will be added in the revised manuscript. It will now read as:

"Other processes such as the atmospheric waves and jets in the region (African Easterly wave, African Easterly Jet, etc.) may also contribute to the occurrence of LLCs but these not analyzed in this study. These processes are, however, only relevant in the wet season."

l. 393 "redaction" I think this is not the right word.

Thanks. This will be changed to *"…editing of the first draft".*

References

Geleyn, J.-F. and Hollingsworth, A. An economical analytical method for the computation of the interaction between scattering and line absorption of radiation. *Beitr. Phys. Atmos.*, 1979, **52,** 1–16

Storer, R. L.; Van den heever, S. C. Microphysical Processes Evident in Aerosol Forcing of Tropical Deep Convective Clouds. *J. Atmos. Sci.*, **2013**, *70* (2), 430–446. https://doi.org/10.1175/JAS-D-12-076.1