Responses to Referee 2

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 will be made in the manuscript are shown in italics.

This paper presents the analysis of the daytime low level clouds (LLC) over West Africa using ERA5 dataset for the period 2006-2015 to investigate their occurrence frequency, seasonal and diurnal cycles, as well as the associated atmospheric conditions for the two contrasting regions, i.e. Sahelian and Guinean region. Based on the cloud fraction, three classes were formed (no LLC, LLC class-1, and LLC class-2) and the results indicate that during the summer months LLC class-1 has the peak occurrence frequency in the Sahel, while LLC class-2 is dominant in the Gulf of Guinean. Finally, this study also addressed the attenuation of the shortwave downwelling radiation due to the LLC and found that during the summer months it is on average between 44 % and 49 % for the Sahel and Guinean region.

Considering the importance of the LLC for the regional climate of West Africa and the general lack of studies addressing these clouds, I find the topic of this study to be relevant and suitable for publication in ESD. This study provides new results which in my opinion will be useful, especially considering the ongoing and future solar energy project. However, before this manuscript can be accepted for the publication, it should be carefully revised, since there are some shortcomings in the analysis and the presentation of the results needs to be more clear. My comments and suggestions are listed below.

We thank the reviewer for the overall positive and constructive comments provided to improve our study.

Major comments:

1. I do not understand why authors extract reanalysis data of a higher resolution of 0.25o to a very coarse 1o resolution. What do you mean by "directly extracted" (Pg. 3, line 87)? Although the focus is on the analysis of large scale conditions, it is well known that some mesoscale processes are responsible for the formation and maintenance of LLC. Have you performed some sort of validation test to make sure that the conditions are properly presented with the data of coarser resolution? Additionally, sensible and latent heat flux analyzed here are not large scale phenomena.

   During the retrieval of ERA5 data, there are options to select the resolution of the grid (finer or coarser than the native grid) onto which the data will be given. In our case, we chose to retrieve the data on a 1°x1° grid. This is what we meant by 'directly extracted'. Retrievals and downloads at the finer resolutions could be very slow (especially for the data over several pressure levels). And since most of our analyses are based on large-scale features (moisture and winds), we have decided to use the 1° grid. However, we performed some comparison tests before going ahead to use the 1° resolution data.

We made a comparison of the moisture flux and winds during LLC occurrence for the 1° resolution (Figure 1 upper row) and native ERA5 resolution = 0.25° (Figure 2 lower row). This comparison was made for only a short sub-period of 2006-2015. This comparison does not present any major discrepancy. Synoptic conditions seen by using the fine resolution are also presented very well by using the coarse resolution. Here we show only one month (August 2006) and for LLC occurrence in the Guinean region.

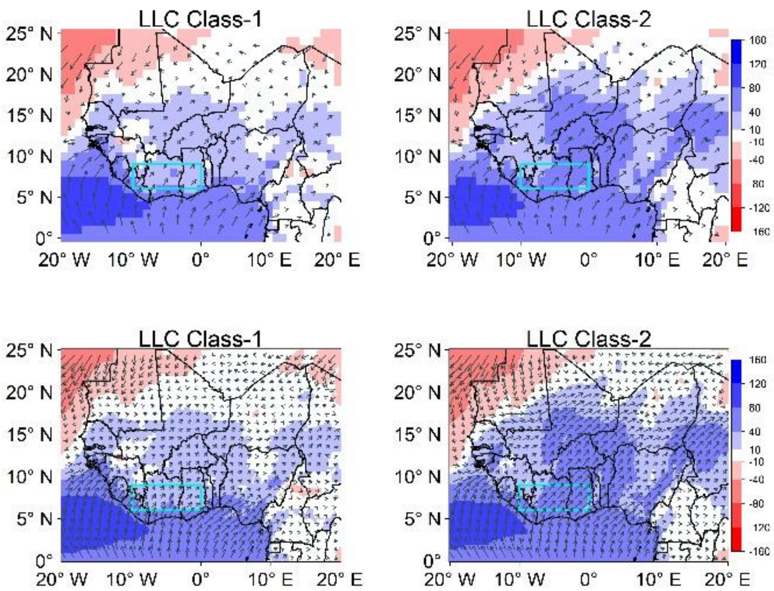

Figure 1: Comparison of moisture flux and winds for LLC occurrence using ERA5 data at 1° (top) and 0.25° (bottom) resolutions.

With regards to latent and sensible heat fluxes, we agree that these are not large-scale features, and this should at least be mentioned in the manuscript. In the revised manuscript, we will modify the sentence that introduces the latent and sensible heat fluxes. It will read as:

"The atmospheric variables include specific humidity, zonal and meridional wind components, and the vertical velocity. The surface fluxes to be analyzed are the sensible and latent heat fluxes, although it should be noted that these are not large-scale phenomena."

2. In my view, the analysis of the atmospheric conditions should be presented more consistently and more clearly.

The definition of the "horizontal moisture flux advection" is not correct (Pg. 5, line 141). Namely, eq. (1) presents the average moisture flux and not the advection of the moisture! Therefore, the presentation and discussion of the results regarding the advection of the moisture need to be carefully revised. If Figs. 5 and 6. show the quantity calculated according to the eq. (1), then they do not show moisture advection. The large majority of the paper discusses the role of moisture advection, however, this quantity is not calculated.

We thank the reviewer for this important comment. We acknowledge that the moisture flux advection was not computed. We will revise our manuscript, so that the discussion of our analysis of the atmospheric conditions will be based on the moisture flux, rather than the advection.

On Pg. 6 Equation (2) is inconsistent with the text on lines 160-161: shouldn't the equation read as: CREswt=SWt-SWcs(t)?

You are absolutely right. We only presented eq2 as CREswt= SWcs(t)-SW(t) because we did not want to present CRA with negative values. To rectify this, we will define CRA in the revised manuscript, as the absolute value of SW(t)-SWcs(t). Please also note that based on the reviewer RC1 comment #10, we will remove equation 2 and just keep eqn 3. The new equation 2 will now be:

$$CRE_{SW\downarrow}(t) = \frac{\left|SW^{\downarrow}(t) - SW_{CS}^{\downarrow}(t)\right|}{SW_{CS}^{\downarrow}(t)} \times 100$$

3. Due to the definition of the LLC used in this study, i.e. all clouds with cloud base below 2 km, different cloud types are considered as LLC. This causes some confusion in understanding atmospheric forcing related to different LLC classes in the two regions. For example, the dominant LLC class-2 during the JAS period in the Guinean region is most likely related to the stratiform clouds and shallow cumulus, while the LLC class-2 peak in the same season most likely corresponds to deep convective clouds (as it corresponds to rain events). These different cloud types have different forcings and the authors need to make a more clear distinction between these throughout the manuscript. This is especially confusing in the Abstract.

   The authors should refer to the recent finding from the DACCIWA regarding the physical processes responsible for the formation and maintenance of the LLC over the Gulf of Guinea during the WAM season.

   We thank the reviewer for this comment. In the revised manuscript, we will make the discussion clearer regarding which cloud types are likely to be associated with each LLC class. We will also discuss the effects of having multiple cloud types in our definition on the results obtained. We will use especially the findings of Lohou et el., (2020) and Zouzoua et al., (2020) when discussing the kinds of LLCs included in our work. For example they showed that some LLC can decouple from the boundary layer into higher altitudes and are therefore not influenced by surface fluxes. Babic et al. (2019) and Adler et al. (2019) provides detailed discussions on the processes that trigger LLCs. We will support our discussions with their findings.

4. In the recent DACCIWA papers (ACP special issue available at https://www.atmoschem-phys.net/special_issue914.html), the advection of the cold maritime air mass, related to the low level jet and the SW monsoon flow, is found to be the key process responsible for the LLC formation and not the advection of moist air. On the other hand, the advection of the relatively moist air could be important for the LLC formation in the Sahel. However, the authors do not assess the role of temperature advection. What is the reason for this?

As you mentioned in comment #3, the different cloud types have different forcings, and for low-level stratiform clouds, cold air advection is very important as shown in the DACCIWA papers (e.g., Adler et al. 2019). We actually computed the horizontal temperature advection (H_{adv_T}) but did not introduce the figure in the manuscript. H_{adv_T} was computed as follows:

$$H_{adv\_T} = -\left(u\frac{\partial\theta}{\partial x} + v\frac{\partial\theta}{\partial y}\right),$$

where u and v are the zonal and meridional wind components respectively, and θ is the potential temperature.

The vertical profile of the H_{adv_T} for the first 2km is shown below in Figure 2 for LLC occurrence (Class 1 and 2) for the JAS season. The profile shown here (for the first 1km) is somehow similar to some of the results from the DACCIWA papers (*HADV in Adler et al., 2019 Figure 6a*), however the values here are rather very low compared to what was is shown in Adler et al., (2019). This difference is probably due to the fact that our LLC definition combines different low clouds (all types of clouds below 2km) – stratiform clouds probably dominate during JAS and explains the cold air advection as shown in Figure 2. Again, the events analyzed during the DACCIWA campaign are much lower than those we analyzed – averaging over all these events could have led to these low values.

On the other hand, we do not expect moisture flux to differ significantly for the different low clouds. As we know, saturation can be reached in two main ways: by evaporation (evaporation may be higher when the available moisture is high – we showed high moisture for the cloudy events) and condensation (cooling – cold air advection as shown in the DACCIWA papers). For instance, in one of the DACCIWA papers (Babic et al. 2019) the most distinct difference between stratus and stratus free events was evident in the specific humidity (please see their Figure 6c) – which shows the importance of the moisture flux for the occurrence and maintenance of these low clouds. Therefore, we decided to focus our analysis on the moisture flux in the atmosphere during the occurrence of LLCs. In the revised manuscript, we will discuss the effect of including all low clouds in our LLC definition on the results obtained and we will mention that the role of forcings such as the temperature advection as shown in the DACCIWA papers, may not be clearly noticed. We will support our discussion with some of the findings of the DACCIWA studies (especially with regards to cold air advection) where applicable.

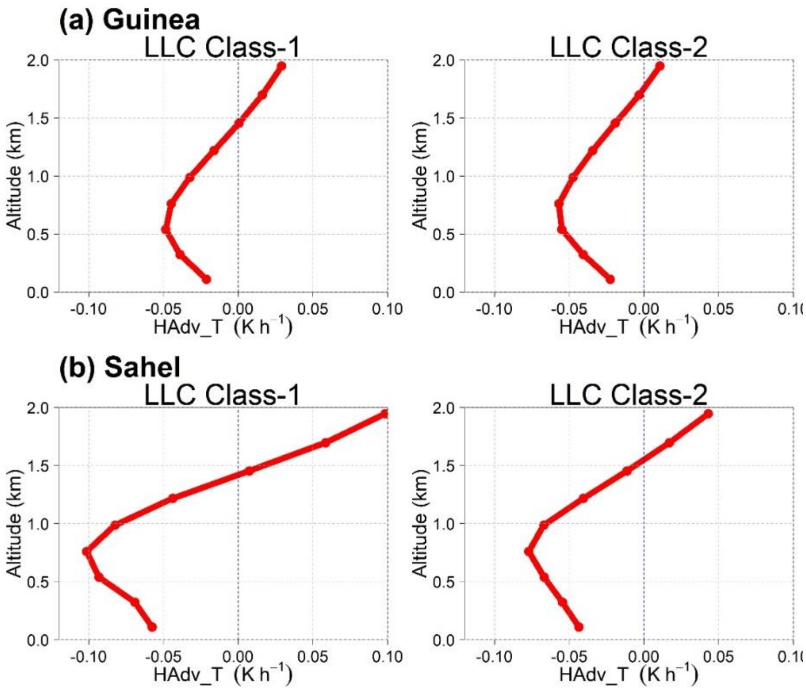

Figure 2: Vertical profiles of horizontal advection of temperature for LLC occurrence in Guinea and Sahel

Minor comments:

- Pg. 2, line 59-60: This statement is not correct. In the Guinean region the LLC form during the night and persist long into the morning and early afternoon hours, therefore have a direct impact on the surface solar irradiance. Please see the study of Lohou et al. (2020) and Zouzoua et al. (2020) regarding this.

  Thanks. The statement will be revised. It will now read as:

  In the night, the nocturnal LCCs have no influence on surface solar irradiance due to the absence of sunlight. However, they persist long into the morning and early afternoon hours, thus, directly influencing the amount of incoming solar irradiance.

- Pg. 7, lines 198-200: Here it would be better to refer to recent DACCIWA publications which show that the advection of cold air, and not the advection of moist air, is the key process responsible for the formation of LLC in the Guinean region during the monsoon season.

  We thank the reviewer for this comment. We will revise the sentences and include the DACCIWA references here.

- Pg. 8, line 234-236: The reference here should be Adler et al. (2019) not (2019b). Additionally, all the studies referenced here find that the advection of cold air is the major factor in leading to saturation, not moist air! The authors should carefully revise the paper when discussing the processes leading to the formation of LLC in the two regions and when referring to previous studies to avoid making mistakes like these. Namely, based on the DACCIWA observational data it became clear that the advection of cold maritime air is the dominant process for the LLC formation in the Gulf of Guinea, while the advection of moist air into the Sahelian region is the most dominant process for LLC formation.

Noted. The manuscript will be revised so that the appropriate terms (e.g., cold air advection instead of moist air for the Guinean region) will be used in the discussion when making references to the DACCIWA observational studies.

- Pg. 8, line 242: It should be Saharan Heat Low.

  Thanks. This will be corrected in the revised manuscript.

- Pg . 8, line 243: What is the role of the cold air advection?

  In this sentence, we believe *"moisture advection"* and not *"cold air advection"* was mentioned. However, the appropriate term here should be *"moisture flux"* and not "moisture advection". The sentence will be revised.

- Pg. 8, line 245: Consider here replacing "moist air" with cold air.

  Noted. 'moist air' will be changed to 'cold air'.

- Pg. 9, line 287: How can water vapor be cooled by moist air advection?

  This was wrong. In the revised manuscript, the sentence will read now as "…further cooled by cold air advected from the Guinean Coast, leading to saturation and then LLC formation."

- Pg. 10, line 304 and 312: Which region are you referring to? Is it entire West Africa?

  In line 304, the discussion was on the Sahel region. In line 312, region being referred to was the whole of West Africa. We will make those sentences clear to indicate which regions were being referred to in those two statements.

- Pg. 10, line 310: Figs. 10 and 11 also show the downwelling shortwave radiation attenuation for no LLC class.

  Yes, this is because there could be higher level clouds in those cases with no LLC. We mentioned this situation in the method section (line 127 to 129 in the original manuscript).

- Pg. 11, line 329-330: The sentence "In Sahel, the mean attenuation during the occurrence of LLC Class-1 events is 16.3% *though* areas around the southern coast of WA can experience higher losses." should be rephrased since it is not clear what is the connection between the attenuation in the Sahel and the southern coast of WA.

  This will be rephrased. It will now read simply as:

  In Sahel, the mean attenuation during the occurrence of LLC Class-1 events is 16.3%.

References:

Adler, B., Babia, K., Kalthoff, N., Lohou, F., Lothon, M., Dione, C., Pedruzo-Bagazgoitia, X., and Andersen, H.: Nocturnal low-level clouds in the atmospheric boundary layer over southern West Africa: An observation-based analysis of conditions and processes. Atmos Chem Phys, 19, 663–681, https://doi.org/10.5194/acp-19-663-2019, 2019

Babić, K., Kalthoff, N., Adler, B., Quinting, J. F., Lohou, F., Dione, C., and Lothon, M.: What controls the formation of nocturnal low-level stratus clouds over southern West Africa during the monsoon season? Atmos Chem Phys, 19, 13489–13506, https://doi.org/10.5194/acp-19-13489-2019, 2019