REVIEW

Daytime low-level clouds in West Africa – occurrence, associated drivers and shortwave radiation attenuation

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This study, based on the analysis of the ERA5 hourly data from 2006 to 2015, shows the occurrence of daytime low level clouds in WA at two different latitudes (Sahelian and Guinean). It also aims at determining the atmospheric conditions for different LLC classes (based on cloud fraction) and their impact on the solar incoming radiation. This article can be improved at several points of view: motivation, methodology explanation, results interpretation.

Main comments:

(1) The LLC events are selected in this study with their cloud base height (< 2 km). This is in accordance with the definition of low cloud. However, the authors apply this definition at two very different places in Africa and at very contrasting seasons. The boundary layer varies from few hundred meters (during monsoon season in the Guinean region) to several kilometres (during winter season in the Sahelian region). Consequently, the LLC as defined by the authors may include or not boundary layer clouds according to the region and the season. This should be at least discussed (see specific comments) since it impacts consequently the different statistics presented in this study.

(2) In addition to the previous comment, LLC with base below 2 km gather stratus, stratocumulus, cumulus and MCS (as mentioned in the paper). These clouds develop in quite different atmospheric conditions (specifically in terms of divergence). What is the interest of highlighting the atmospheric conditions of a mix of different clouds (see specific comments)?

(3) The cloud fraction as a parameter to define different LLC classes in order to check their impact on solar radiation seems quite logical, but not to determine the atmospheric conditions of these classes.

(4) Some explanations are missing to fully understand the analyzed atmospheric conditions, specially the surface flux anomaly.

Specific comments:

(1) P2, line 40: Perhaps the authors could add a reference to the work of Shrage et al. who first quantified the low level cloud frequency and fraction in southern WA.

- Schrage, J. M. and Fink, A. H.: Nocturnal continental low-level stratus over tropical West Africa: observations and possible mechanisms controlling its onset, Mon. Weather Rev., 140, 1794–1809,2012.

(2) P2, line 48-57: Three very recent articles published in ACP (DACCIWA special issue) should be cited here since they focus on daytime phase of low level clouds. The first one estimates, with local measurements, the impact of LLC on the surface net radiation and on the convective surface fluxes which is one objective of the present study.

- Lohou, F., Kalthoff, N., Adler, B., Babic, K., Dione, C., Lothon, M., Pedruzo-Bagazgoitia, X., and Zouzoua, M., 2020 : Conceptual model of diurnal cycle of low-level stratiform clouds over southern West Africa. Atmos. Chem. Phys., 20, 2263–2275, doi: 10.5194/acp-20-2263-2020.
- Maurin Zouzoua, Fabienne Lohou, Paul Assamoi, Marie Lothon, Véronique Yoboue, Cheikh Dione, Norbert Kalthoff, Bianca Adler, Karmen Babić, and

Xabier Pedruzo-Bagazgoitia : Breakup of nocturnal low-level stratiform clouds during southern West African Monsoon Season, https://doi.org/10.5194/acp-2020-602

- Pedruzo-Bagazgoitia, X., de Roode, S. R., Adler, B., Babić, K., Dione, C., Kalthoff, N., Lohou, F., Lothon, M., and Vilà-Guerau de Arellano, J., 2020: The diurnal stratocumulus-to-cumulus transition over land, Atmos. Chem. Phys., https://doi.org/10.5194/acp-2019-659.

(3) P2-P3 : I do not understand the link between the last sentence of page 2 and the first one of page 3. Could the authors be more explicit? Or perhaps the word « consequently » is not appropriate in this sentence?

(4) P3, Line 3 : « ...have been limited <u>to</u> the WAM season... »

(5) P3, line 70 : « Few studies which were done with simulations, reanalysis, and satellite data in the region (e.g., Adler et al., 2017; Knippertz et al., 2011; van de Linden et al., 2015; Schuster et al., 2013) have nevertheless shown results similar to those of ground observational studies to some extent. » Could the authors precise to what observational studies they referred to?

(6) P4, line 100-102: Could the author reword this sentence? Remove this part perhaps “and their surface heat fluxes explore » ?

(7) P5, L127-134: This paragraph about upper clouds is very important at several points of view. Because the threshold on the base height (< 2 km) can mix a lot of cloud types (Stratus, Stratocumulus, Cumulus, MCS) I wonder if it would be interesting to add a statistic on the LLC top heights just to be aware of the cloud types mixed in the so large family of LLC.

(8) P5, equ. 1:
- It seems to me that equation 1 gives the Moisture Flux. The moisture advection would be defined with the horizontal gradient of specific humidity. Qavd is named differently along the article: horizontal moisture flux advection, moisture advection,…
- The sum should be between 1 and N and not between i and N.
- Why the chosen level for the moisture flux estimate is 950 hPa? Could the authors justify this choice? Why the integrated value over the vertical is not used in this study?

(9) P5, L: Ek and Holstag study focuses on ABL clouds.
- The present study does not take into account all ABL clouds since a large part of them are higher than 2 km when the ABL is higher, which is very often the case in Sahelian region.
- Some Stratus or Stratocumulus low cloud are decoupled from the surface and are then not influenced by the surface flux (see previous suggested papers of Zouzoua et al., Pedruzo et al, and Lohou et al.).
  ➔ These two points should be addressed in some way.

(10) P6, Equation 2 & 3:
- CRE has two different definitions through equation 2 & 3. The authors could remove equation 2 and just keep equation 3.

- What are the clear sky events? Are they cases with no liquid water at all in ERA5? If so, how many cases are used for CRE estimate? Or are they theoretically calculated? This is important to understand the CRE for no-LLC cases.

(11) P6, L163: "...$SW_{\downarrow}$ is the downwelling shortwave radiation in all-sky conditions..." If I understand correctly $SW_{\downarrow}$ is rather the downwelling short wave radiation for each LLC classes instead of "all sky conditions". This could be written in first place?

(12) P6, L169: The authors should make clear in the text and the legend that this daytime distribution gathers all the seasons.

(13) P6, L180: "Additionally, the early morning peak in the events of LLC Class-2 could also be partly linked to contributions from tropical oceanic low-level convection which is maximum during the early morning (Yang and Slingo, 2001)." Could the authors check the Yang and Slingo paper? It seems to me that Yang and Slingo mentioned the "Tropical oceanic deep convection" (for example page 798-799 in Yang and Slingo, 2001). If this is so, the paragraph the authors certainly referred to finishes this way : "Some of these convective systems, under optimal environmental conditions, continue to grow and reach their mature stage some time later during the night and early morning". It is rather deep convection and MCS that Yang and Slingo are talking about. How do we know that LLC class 2 are deep convection if all seasons are mixed in fig 3? What is the proportion of deep-convection versus stratocumulus? In what extent the MCS can impact the statistic presented in Fig 3 for Guinean region? Did the authors try a diurnal cycle for each season?

(14) P7, All comments on Figure 3: I don't know what the authors call "well-marked diurnal cycle" or "weak diurnal cycle"? For example, 10% of 2500 cases is a larger variation (LLC class-1 Sahelian region) than 4% of 200 cases (LLC class-2 sahelian region).

(15) P7, L189-192: I agree with this comment on the type of cloud included in the statistic. That helps to understand what really means this composite diurnal cycle.

(16) P7, L193-214:

- I think these two paragraphs should be moved at the beginning of this section. It would be interesting for the reader to have a first insight of the seasonal variation before the diurnal one.
- The threshold of 2 km for the cloud base might impact the Sahelian region distributions. The number of no LLC class would decrease with a higher cloud base height threshold whereas LLC class-2 number would increase. That means that MCS, for example, can be sorted out as no-LLC class when ABL is higher than 2 km. Is that possible? In what extent this 2-km threshold impacts the atmospheric conditions for the different classes?

(17) P8, L228, Table 2:

- The total number of hours in season indicated in Table 2 should be presented and commented in the legend and the text?
- There is zero No-LLC events detected in Guinean region during DJF season according to table 2. But there are atmospheric conditions for this class in figures 6 and 11 for example. I guess the table is wrong.

(18) P8, L231: "...cold moist air from the ocean associated with the strong southwesterly winds (positive Q_{ad})..." Figure 5 and 6 are very nice. I am not used in analysing the moisture flux but

according to the Figures 5 and 6, Q_{adv} seems rather dominated by the wind intensity than the moisture (and even less the temperature of course) from my point of view. I think, and some AMMA and DACCIWA studies show this, that the moisture advection is null in the Guinean region during the WAM (e.g. Adler et al., Babic et al.), whereas it is important in the Sahelian region. It depends on the moisture gradient. So what does the moisture flux show? The monsoon or the Harmattan horizontal expansion according to the season?

(19) P8, L232: "Predictably, the horizontal advection of moist air is stronger during the occurrence of LLC Class-2 events than LLC Class-1 events." Moisture flux?

(20) P8, L234," This inland advection of moist air from the ocean has been found to play a major role in cooling (Adler et al., 2019b) which in turn enhances saturation of water vapour and consequently LLC formation (Adler et al., 2019, 2017; Babić et al., 2019b)." Moist air advection cannot induce a cooling. Adler et al., Babic et al. and Lohou et al. showed that there is no moist air advection but cold air advection. It is the cold air advection by the low level jet and Atlantic inflow which induces the saturation and the cloud formation in the Guinean region. That means that the monsoon flow is not moister than the air in the Guinean region where rain events are frequent. This can be different for the Sahelian Region which is much drier than the Guinean one. Please consider to change this comment and make different comments for the two regions.

(21) P8-9, L245-259: this paragraph deals with the vertical velocity and the divergence of the horizontal wind.

- I would show only one of these parameters since they should be proportional. No need to comment both.
- The authors precise at the beginning that a negative value of these two parameters is favourable to cloud formation. Stratus and stratocumulus are characterized by positive vertical velocity (hPa s^{-1}). So this comment cannot be general and this shows the limit of searching for atmospheric conditions of a class which may mix different clouds.
- "It is also important to note that during JAS, the average vertical profiles of these processes (divergence and vertical velocity) are not similar to DJF (not shown).". If this is important, perhaps the authors should comment a little more on this or show the figures for JAS.
- I am not sure what the reader can conclude from this analysis.

(22) P9-10, section 4.2:

- Could the author precise how the monthly anomaly is computed because I am very surprised by these results?
- The reduction of the net radiation at surface by the clouds induces a reduction of the surface sensible and latent heat flux. So, from my point of view, the figure 8 should rather show the cloud impact on the surface energy budget than the effect of the convective flux on the clouds.
- This is why I do not understand how the author can find a negative anomaly of the sensible heat flux during no-LLC event.
- The anomalies should be discussed in comparison to the flux itself. Do the author think that an anomaly lower than 10 W m^{-2} is significant when the surface flux is around 300 W m^{-2} and considering the error of the surface flux in the model.
- At last, the authors deduce from the surface flux anomaly a surface temperature anomaly. The surface heat fluxes are proportional to the temperature vertical gradient and not directly linked to the surface temperature. Such a discussion is misleading.

(23) P11, L313-316: I fully agree with this statement. The surface convective flux should be also reduced according to the cloud fraction. So how the authors can detangle the effect of the cloud on the surface flux from the effect of the flux on the cloud triggering?