Review of the manuscript titled "Daytime low-level clouds in West Africa – occurrence, associated drivers and shortwave radiation attenuation" by Danso et al.

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.

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).
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).

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.
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.

Minor comments:

l. 29: "efficiently represent convection AND CLOUDINESS"

l. 35: "escalation" is not the right wording

l. 37: "remains"

l. 40: prefer references in a chronological order.

l. 40: "van deR Linden", please correct.

l. 40: "Farther north"

l. 43: "persisting into the early afternoon"

l. 43: "as shallow convective clouds"?????

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

l 50: "The majority if these studies"

l. 63: "limited TO the WAM season"

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

l. 101 "their surface heat fluxes explore", awkward sentence

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

l. 182 Reword "convected air"

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

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

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

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

l. 234 “...cold moist”. Sentence terminates awkward.

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.

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.

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).

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

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