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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Breakup of nocturnal low-level stratiform clouds during southern West African Monsoon Season" *by* Maurin Zouzoua et al.

Anonymous Referee #2

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This study attempts to understand the evolutions of daytime low-lying stratiform clouds during southern West Africa monsoon seasons. The analyses are primarily based on ground-based observations of about two dozens cases collected during the DACCIWA field campaign. Although the size of the samples is not large enough to conduct statistically robust analyses, the authors did an overall good job of taking full advantage of the cases by conducting systematic and all-round analyses. In particular, the budget analyses of liquid water path (LWP) are conducted, which has been considered very challenging for observational studies since many budget terms are difficult to quantify with observations. In that regard, the authors' patience to quantify each LWP budget terms and their potential uncertainties is very impressive (although uncertainties are

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Discussion paper

still considerable). Overall, I think it is a high-quality manuscript with well-organized presentations, mostly solid analyses, and useful conclusions. This makes me believe that the manuscript should be eventually suitable for being published in ACP. However, there are several major issues that must be addressed before I recommend acceptance. I detail them below:

(1) Insufficient treatment of radiative cooling term (RAD) quantification RAD is the dominant term controlling the convective overturning before the early morning, as also recognized by the authors. However, the equations (Eq. 2 and 3) used to quantify RAD in this study are too rough. As shown by Zheng et al. (2019), the RAD is most sensitive to two parameters: cloud optical thickness and moisture loading in the free atmosphere. If high clouds are present, the RAD will weaken significantly (e.g. Christensen et al., 2013). Even though the free-tropospheric moisture loading can be somewhat accounted for in Eq. (2) (the IWP), the cloud optical thickness and higher clouds can also modulate the RAD considerably. The blackbody assumption is only always valid for not-too-thick stratiform clouds (Zheng et al., 2019). The authors show that the RAD varies very little ($\sim$ 5 Wm-2), which could be artificial consequence of the two assumptions behind the equations (i.e. blackbody and no high clouds). Thus, given the significant role of RAD, it should be worthwhile to use a radiative transfer model instead. All inputs for the model are available from the observations: cloud-base and -top heights and soundings. Running it is computationally cheap.

(2) Inappropriate classification of the scenario of DD I am very reluctant to consider the clouds in Fig.10 c as "decoupled throughout the day". There are three possibilities for this case: (1) initially decoupled clouds remain decoupled and surface-heating-driven cumulus clouds start to form underneath it. If they don't interact, the upper-layer clouds are decoupled and the bottom clouds are coupled; (2) if they interact, they form the cumulus-coupled stratocumulus-topped boundary layer such as those in downstream subtropical oceans; (3) If the initially decoupled clouds dissipate rapidly after decoupling, with only the underlying cumulus clouds left, this case is simply regular

continental shallow cumulus that are, by definition, coupled.

All the above-stated cloud regimes are possible. Thus, it is a little bit misleading to call all of them "decoupled throughout". I would suggest either renaming it or adding additional discussions to clarify the definition of the decoupling.

(3) other comments: - Page 5, Line 25: there are earlier literatures form the ASTEX campaign that is the first attempt to study the SC-to-cu transitions. - Figure 2 and other figures: it should be helpful to use local time as well, which makes the readers easier to think of the problem from a diurnal cycle perspective. - Page 10-11: some discussions on what determines the RAD is useful. (check the work by Zheng et al., 2019) - Page 12, Line1: large-scale subsidence is commonly obtained from reanalysis data. Not very accurate, but better than nothing. - Section 4.1 as a whole: this section is centered on the difference between coupling and decoupling, however, what may cause the decoupling/coupling in the first place is not discussed in detail. There are several influential factors: cloud-top cooling itself (Nicholl 1984), precipitation (this is not important in your case), "deepening warming" decoupling (Bretherton and Wyant, 1997), and warm thermal advection (Zheng and Li, 2019). It may be more enlightening to discuss your results in the context of these potential influential controllers. - Page 22, Line 15: again, it could be due to too simple treatment of RAD. - Figure 13: there are too many symbols, making the readers hard to recognize each of them. This defeats the purpose of using a diagram for illustrations. Try to use process-based cartoons (e.g. the one from Wood 2012).

References:

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