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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The diurnal stratocumulus-to-cumulus transition over land" *by* Xabier Pedruzo-Bagazgoitia et al.

Anonymous Referee #2

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Suggestion: Minor Revision

Summary: This manuscript presents a set of large-eddy simulations (LES) of diurnal stratocumulus-to-cumulus (Sc-to-Cu) transition over southern West Africa during its monsoon season. It complements well the existing literature on the Sc-to-Cu transition over other continental regions and subtropical oceans. Specifically, it highlights the important roles of (1) the strong daytime land surface heat and moisture fluxes and (2) the wind shear by monsoonal lower-level jet on the boundary layer growth and decoupling that lead to Sc break-up. I think this manuscript is generally well-written and the analyses are comprehensive. Thus, I have only some specific comments on further clarifying the interpretation of the results. My detailed comments are as follows.

Specific Comments:

Title: This title is too broad. I would suggest making it more specific, e.g., 'the diurnal stratocumulus-to-cumulus transition over land during the southern West Africa monsoon season'.

Page 2, L18: Although I agree that the Sc-to-Cu transition is important for quantifying the Sc radiative effect and its bias in climate models, it would also be nice to discuss briefly why the formation of Sc after sunset is less important. Do all climate models simulate the Sc formation correctly? The Sc-to-Cu transition would be irrelevant to climate models if they cannot even produce Sc in the first place.

Page 3, L9: Could the authors please briefly summarize the mechanisms and processes by Ghonima et al. (2016), since the readers may not be familiar with them?

Page 3, L13: It would be better to clarify that the longwave and shortwave radiation has different effects on the maintenance of Sc.

Page 3, L22: It seems to me that the entrainment velocity should increase, not decrease when cloud-top wind shear is present.

Page 4, L11: The land model is not sufficiently justified. Is the surface homogeneous, and is it free of topography? Why can these assumptions be made?

Page 4, L14: What vegetation is present in SWA? Are the 2 big-leaf scheme parameters tuned specifically for the regional vegetation, or for the mix of grass and bushes (or corn) at the surface flux observation sites (see Page 5, L14 and 19)? How sensitive is the model to the choice of land/vegetation scheme?

Page 4, L20: By 'other chemical compounds', did the authors mean only the radiatively active gases, or are aerosols also included? If so, how are the aerosol concentrations prescribed?

Page 5, L5: How should I interpret the observed cloud base height: is it a local value, the domain average, or the minimum? This is especially important after the continuous Sc-deck breaks up (e.g., the sudden jump of the red circle from 1000m to 500m at 12

00 UTC in Figure 2b requires further clarification).

Page 5, L16: What does 'sonic temperature and humidity measurements from fast infrared hygrometer' mean: is it a sonic or optical equipment?

Page 5, L19: What is the motivation of using an additional site over corn for TKE measurements but not for surface fluxes?

Page 6, L5: The word 'coupled' is confusing. Did the authors mean the atmosphere and land surface are coupled, or the cloud layer and the surface air layer are coupled?

Page 6, L15: The lower level divergence is about 8x10ˆ-6 sˆ-1, which is even stronger than the typical conditions for marine Sc (e.g., DYCOMS). Also, the subsidence profile is very shallow with a scale height of only 300m. How are these values chosen? Are they selected to keep a steady Sc-deck during nighttime? Although the authors stated that the COSMO and ERA-I both show a large spread of subsidence, it would still be necessary to demonstrate that the prescribed subsidence falls within the ranges of COSMO and ERA-I, and that the subsidence profile does not change during the diurnal cycle. It would also be necessary to discuss briefly the model sensitivity to the prescribed subsidence.

Page 6, L22: Is radiation calculated column-by-column, accounting for the spatial inhomogeneity in cloudiness?

Page 6, L31: 'Drying' is a process that makes something drier (e.g., entrainment drying), but I think the authors may instead mean that the air mass above cloud top is drier than below.

Page 7, L7: Do the free-tropospheric temperature and moisture profiles drift?

Page 7, L27: With no large-scale wind, does the surface flux rely entirely on the surface wind produced by turbulent motion in the LES (without additional gust)? How much is the LES surface wind changed by imposing the 3m/s horizontal wind in the MEAN case?

Figure 2: On L5 of the caption, the word 'red circles' should be changed to 'red triangles' based on panel (c).

Page 8, L15: It should be clarified that the sudden jump in surface fluxes is only shown in observations, whereas the change in surface fluxes is very smooth. For LES, most of the surface flux increase occurs well before the cloud break up, and the correlation between surface flux and cloud fraction seems very weak in the LES. However, there is a clear negative correlation between LWP and surface fluxes, which is not discussed.

Page 9, L1: Following the comment above, the statement that 'the surface fluxes are radiation-driven' is not well supported by the presented data: (1) the sudden change does not occur in LES, and (2) the coincidence between the sudden jumps in surface fluxes and cloud cover does not imply causality (or which one drives the other), and (3) cloud cover is not a good proxy for the cloud radiative effect, as the clouds can thin significantly while maintaining 100% cover. I would suggest the authors add a panel in Figure 2 to show the surface insolation, or fraction of insolation reaching the surface (both LES and observation if available). The statement would be better supported if the insolation jump occurs earlier than the surface flux jump.

Page 9, L5: In LES, the cloud cover decreases quasi-linearly only after 13 00 UTC, i.e., about 1.5 hours later than the initial break-up.

Figure 3: I suggest adding a panel showing the vertical profiles of domain-mean cloud fraction and liquid water content for completeness.

Page 10, L2: I suggest using 'thin' instead of 'narrow' because the inversion layer extends vertically, not laterally.

Page 10, L4: It may be worth clarifying that the 'inversion layer' after decoupling includes the entire conditionally unstable Cu-layer, and is much thicker than commonly known sharper inversion layer that tops the Cu-layer (at around 1200 m in Figure 3e). Also, how sensitive is the definition of zi+ and zi- to the threshold of 5%? It seems that a threshold of 15% to 20% would identify the aforementioned sharper inversion layer.

Page 11, L13: It was unclear to me initially that the authors are already talking about the conditions at 14 30 UTC ('convective clouds above 950m'), so I suggest adding some reference to the time. I would also suggest moving the next sentence (about 11 00 UTC conditions) in front of this sentence based on the timeline.

Page 15, L3: Is there a reference for the statement that enhanced buoyancy within the cloud layer (instead of near the cloud top) increases entrainment?

Figure 7: Since the plotted time-series represent the time-accumulated differences, I would suggest removing the 1/dt from the y-label to avoid confusion.

Page 16, L7: I suggest clarifying that the BASE contribution is from the increased moisture flux (rather than sensible heat flux, which is almost zero at nighttime).

Figure 8: To better support the discussion (Page 17, L9), I suggest adding horizontal lines that indicate cloud top heights in all panels.

Page 17, L15: I suggest adding the reference to Figure 9a for better clarity.

Figure 9(d): Should the legend 'cbase_max' be 'ctop_max' instead?

Page 18, L3: The statement that SHEAR 'hampers the cloud growth' is not well supported by the figures: the differences in cloud top and base heights are insignificant in Figure 9(c), and the max cloud top height is even higher in SHEAR than the other cases in Figure 9(d). Could the authors provide further clarifications?

Page 18, L4: The statement that MEAN and REF differ little seems inconsistent with Figure 7, where dLWP seems similar for MEAN and SHEAR, and they both produce larger LWP than REF. Could the authors provide further clarifications?

Page 18, L9: As this subsection is focused on nighttime effects, the statement that 'SHEAR has larger effects in the Sc-Cu transition' seems a bit irrelevant, because the transition occurs several hours after sunrise. This statement may be more appropriate

for the next subsection (daytime effects).

Page 18, L12: The first paragraph of Section 3.4.2 discusses various different effects and is a bit too long to read. I suggest breaking it up into shorter paragraphs.

Page 18, L19: Based on Figure 9(a), the daytime inversion layer seems to be thickening, not thinning.

Page 20, L13: The latent heat flux at the surface appears as the denominator of the r_qt formula (Page 20, L1). Why does its increase imply a larger, not smaller r_qt?

Page 21, L10: I would suggest adding a brief summary of the distinct features of the SWA Sc clouds from the typical marine Sc (e.g., lower cloud top but higher LWP).

Typographical Comments:

Page 3, L11: No hyphen is needed for 'subtropical'. Also, the word 'role' is misspelled as 'rol'.

Page 3, L17: There is a redundant space.

Page 3, L19: The letter 'd' at the end of the sentence seems redundant.

Page 5, L15: 'Ultrasonic' should be one word.

Page 16, L13: The author 'Kazil et al.' should be placed outside the bracket.

Page 19, L3: The reference to Fig 7(d) should be Fig 7(e) instead.

Page 24: Please be consistent on the capitalization of names.

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