Review of Low Level Cloud and Dynamical Features within the Southern West African Monsoon by Cheikh Dione et al.

Summary of manuscript:

The paper analyzes new observational data from a ground site at Savè, Benin, which was established as part of the DACCIWA campaign. The data analysis focuses on quantifying the diurnal cycle and intra-seasonal variability of factors related to the southern West African Monsoon, associated dynamical features like the maritime inflow (MI) and nocturnal low level jet (NLLJ), and the formation and breakup of low level stratiform clouds.

Monsoon flow was found to occur at some point on every day studied, with the strongest flow occurring at night. Onset of both the MI and NLLJ occurred most frequently between 1600-2100 UTC and breakup of the NLLJ occurred most frequently around sunrise. The distribution of MI arrival times was shifted earlier than expected considering the distance travelled, but strong monsoon flows may explain this result. The paper highlights the difficulty of cleanly separating MI and NLLJ phenomena in observations.

Low level stratiform clouds formed on 65% of the nights studied and usually broke up by 1200 UTC the following day as the planetary boundary layer became more turbulent and deepened. Cloud bases were typically formed near the core of the NLLJ.

The manuscript is well-organized but lacks sufficient detail and clarity in discussing its methods and reporting its results. The data analysis itself seems to be on solid footing but the manuscript requires a substantial amount of editing to provide a clearer accounting of the analysis and its significance.

In addition, a significant amount of further proofreading and editing is necessary for missing units, unlabeled elements on figures and tables, figure legibility, citation format, general typos, and grammar.

Recommendation: I recommend acceptance following adequate revision of the manuscript for clarity and completeness in reporting its methods and results.

Major issues:

1. It would be more accurate in your title and throughout the paper to refer to "low level stratiform clouds" or something similar rather than "low level clouds." The paper explicitly limits its analysis to stratiform boundary layer clouds and does not dwell on the shallow cumulus clouds that form after the stratus breaks up during the day. These low-level cumuliform clouds also may be worthy of future study for radiative and other implications.

2. Page 1, Line 15: The statement "Monsoon flow is observed 100% of the time" seems to contradict Figure 1 and the definitions in the paper. Did you mean that monsoon flow is observed at some point on 100% of days studied? Such a statement would be supported by the data provided.

3. Figures and tables, generally: There are major issues with clarity on several figures and tables.

a) Figure 2: The gray markers are extremely difficult to distinguish against the background shading. A more contrasting color, such as gold, could make the figure more legible.

b) Figure 4: The vertical lines are not labelled in the figure, described in the caption, or mentioned in the manuscript. My impression is that the solid black line is for the FLF_{mean} MI threshold, the grey dotted line for the potential temperature MI threshold, and the black dashed line for the NLLJ threshold, but this should be made explicit within the figure or in the caption. In addition, there is no indication of what the horizontal dashed line signifies, although it appears to be the zero marker on the Height axis and also a separator between the z-t plots and the FLF plots. As before, the grey markers are barely legible against the background shading.

c) Figure 10: As before, the grey color is incredibly hard to discern. Here it's possible that thickening the lines would be sufficient, although choosing a different color that offered more contrast would work as well.

d) Table 1: It would be helpful to specify in the caption that the FLF_{mean} criterion was used in the MI onset column. In addition, "DC" is never defined in the caption or the text, although I'm assuming it stands for "density current."

Specific comments:

1. Title: It would be helpful to add "Observed at Savè, Benin" at the end of the title to better describe the paper. It's not clear from the present title whether the study will focus on model results, satellite observations, site-specific observations, etc., and the most significant portion of the paper is the description of novel observations taken at the site.

2. Page 1, Line 5: What does the term "quantitative documentation" mean in this context? Is it that the clouds are not well-simulated, or that not enough has been published about the simulated cloud properties?

3. Page 1, Line 16: It's not clear what "According to monsoon flow conditions" means in this context. You mention the correlation with monsoon flow strength in the next line, which seems to make this phrase redundant.

4. Page 1, Line 23: Perhaps "and intra-seasonal" should be added in between "day-to-day" and "variability" given the importance of the different monsoon phases and synoptic setups (e.g., vortex circulations).

5. Page 2, Line 13: It's confusing to distinguish between "aircraft" and "field" campaigns — aircraft campaigns are generally considered a subset of field campaign. For example, a NASA data archive defines an atmospheric field campaign as "an observational study planned for a specific location and a defined time period during which measurements are conducted from airborne platforms and/or ground sites to study physical and chemical processes in the atmosphere" (https://eosweb.larc.nasa.gov/field-campaigns). "Ground-based" may be a more appropriate phrase for the supersite data.

6. Page 2, Line 13 (and throughout): The citation year for the Kalthoff et al. paper should be 2018 instead of 2017 to refer to the published version.

7. Page 2, Line 16: Why are only the data from Savè used? It would be helpful to more fully motivate the decision to focus on this site in particular when two others are theoretically available as well.

8. Page 4, Line 1: Please define COSMO before using the acronym.

9. Page 4, Line 18: "On the one hand… on the other hand" generally signifies that two things will be contrasted, but that is not really the case in these sections. A re-write to "Section 4 presents results for the NLLJ and MI and Section 5 for the LLC" or something similar would be better.

10. Page 5, Line 14: It is not clear what "above and below" 1000/2000 m means in this sentence. Is it 0.5 K between 550-1000 m and 2 K between 1000-2000 m? Or something similar? Please clarify.

11. Page 6, Line 29: The vortex circulations, deep convection, and Harmattan flow are filtered out, or excluded, from the analysis, correct? Just saying "filtered" is ambiguous about whether these observed values are excluded or somehow corrected.

12. Page 7, Line 9: The phrase "found situations to be true" is missing some critical information. What did the simulations find to be true? In context it seems that the Couvreaux et al. paper is cited to support the previous assertion about linking synoptic setups to monsoon variability. Perhaps it would be better to just cite the paper at the end of that sentence if you're not making any further points about the study?

13. Page 7, Line 34: It's a bit of stretch to say that Figure 3c indicates a "clear diurnal cycle" in wind direction. Can you in some way quantify that there's a statistically meaningful diurnal difference? It seems likely to me the difference is real, but it's not self-evidently true.

14. Page 8, Line 18: It would be helpful to put "This last criterion" or something similar here to make clear it's only the third criterion that "ensures stable to neutral conditions at the surface."

15. Page 8, Line 30: When introducing the fuzzy logic method, it would be helpful to motivate why this method is necessary/helpful. From the rest of the paper it seems like the 302 K potential temperature threshold works just as well, so the main benefit is the ability to look at wind and temperature components separately?

16. Page 9, Line 11: It would be helpful to rewrite Equation (1) here plugging in the values for y_1, y_2, and $(r_x)_1$:

$$FLF_x(r_x) = \begin{cases} 0, & r_x \leq 0 \\ \dfrac{r_x}{(r_x)_2}, & 0 < r_x < (r_x)_2 \\ 1, & r_x \geq (r_x)_2 \end{cases}$$

In addition, for transparency/reproducibility, you should provide the numeric values used for $(r_x)_2$ for both temperature and wind speed.

17. Page 9, Line 11: The definition for the mean FLF function should specify whether you're averaging the two other FLF functions (my impression) or taking r_x as the average of the wind speed and negative temperature tendencies.

18. Page 9, Line 13: I can't tell what this sentence about the fuzzy logic method being "meaningful" is actually saying. Meaningful in what sense? Is there some evidence that you want to highlight about this being a meaningful metric?

19. Page 10, Line 8: Is this supposed to be criteria ii)? Also, as written on Page 8, criterion ii) does not make clear the maximum wind speed must be below 500 m, just that the maximum wind speed below 500 m must be at least 5 m/s. You should clarify this criterion.

20. Page 10, Line 14: It is not clear what "if the same scenario appears every day" means here. If every day had the same scenario, it seems like it would be quite easy to determine solid criteria. This sentence should either be written to more clearly state its point or be deleted.

21. Page 10, Line 21: There is a notable period of muted wind speed increases in the morning between 0600-1200 UTC. This seems to contradict the "all times of day" phrasing. In addition, there are also spikes above 5 just before 6 UTC that complicates a simple 1700-0000 callout.

22. Page 11, Lines 3 and 23: The phrases "most probable" and "most likely" suggest some kind of statistical analysis, although none is carried out, or at least documented. If these are conclusions just from visual inspection of Figures 6 and 8, it would be better to say something more along the lines of "most observations fell between the values of…". If you have some threshold (interquartile range? two standard deviations?) being used to define "most probable" or "most likely," it should be reported.

23. Page 11, Line 24: It's not clear how you reach the conclusion that the NLLJ cores from AMMA would have higher wind speeds if they were the same height as those observed in this paper, or what the implications of this are.

24. Page 12, Line 14: Should G and B also be the average values in criterion i)? If not, what fraction of pixels must satisfy criterion i) for the scene to be considered cloudy?

25. Page 13, Line 30: I can't find where earlier in the paper it's mentioned that LLCs cannot be determined during rain events. From Figure 10 it appears that the IR camera continued to collect valid data. Please clarify either here or in an earlier section.

26. Page 14, Line 17: What does "articulation" mean in this context? It's also unclear what exactly is "considered in the statistics."

27. Page 14, Line 27: Did you mean to say "a key need for observations to compare with numerical weather and climate models" or something to that effect? The sentence is missing something as currently written.

28. Page 15, Line 1: It would be helpful to discuss a bit how the MCSs impact the variability results in the paper versus simply asserting they're important, given this was not made much of a focus previously in the manuscript.

29. Page 15, Line 8: Does the flow really turn to the south? From Figure 3, it looks like the flow becomes more southerly if anything, meaning the winds are turning to the north. Saying the winds become more southerly would be clearer.

30. Page 15, Line 29: It would be better to say that low level stratus clouds persist until noon on "80% of days with nocturnal stratus formation" or something similar. Otherwise it looks like this was observed on 80% of all days, which is problematic given that only 65% of days had nocturnal stratus cloud formation to begin with.

31. Page 16, Line 4: This seems very abrupt and incomplete for a conclusion to the paper. The paper would be greatly improved with a final paragraph explaining the broader significance of this work and perhaps suggestions for future directions or uses for the data.

32. Page 16, Line 5: You should state explicitly how long the DACCIWA embargo period will be. It would also be useful to provide a DOI or URL to the baobab database if available.

Technical corrections: There are numerous issues of copy-editing (grammar, reference format, etc.) that need further review. Because of the importance of the abstract, I list all the issues that I identified here. I leave the remaining, similar errors to the authors to address in further proofreading unless the mistake impedes understanding or is in an important location (e.g., subhead).

1. Page 1, Line 2: "Boreal" should not be capitalized.

2. Page 1, Line 3: There should not be a comma after "land."

3. Page 1, Line 4: "These" should be used instead of "those."

4. Page 1, Line 9: "Continuous measurements collected" should be changed to "measurements continuously collected" or the "continuous" should be moved to before "in-situ" in the line above.

5. Page 1, Line 11: "Data" should not be capitalized.

6. Page 1, Line 20: "Stratus cloud" should be pluralized.

7. Page 3, Line 15: "Phase 3" should instead be "Phase 4."

8. Page 4, Line 28 & Page 5, Line 8: "Low troposphere" should be "lower troposphere."

9. Page 5, Line 14: There is a missing unit of "K" after 0.5.

10. Page 13, Line 27: Figure 10?