

## Response to reviewer 1#

We thank the reviewer 1# for his/her valuable and constructive suggestions, which led to significant improvements of the quality of our manuscript. Below we detailed how his/her comments are addressed in the revised version of the manuscript. The corrections made in the manuscript and cited in this document appear in *italic*.

### 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.

We agree with reviewer #1 that the clouds under study are stratiform clouds. We corrected the title and the text as suggested.

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.

The statement “monsoon flow is observed 100% of the time” is misleading. The monsoon flow is observed every day at Savè/Benin during our study period but not all the time. The sentence is now: “*Monsoon flow is observed every day during our study period*”.

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.

We changed in the new version the gray markers to dark red. We hope this figure is now more legible.

b) Figure 4: The vertical lines are not labeled in the figure, described in the caption, mentioned in the manuscript. My impression is that the solid black line is for the FLFmean 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.

We revised this figure as suggested and added more information in his label.

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.

We have thickened the gray line, and used a darker gray for more clarity.

d) Table 1: It would be helpful to specify in the caption that the FLFmean 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.”

We thank the reviewer for this comment. We added the meaning of DC in the legend of Table 1, and the information about the criteria used for the fuzzy function

### **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.

We thank the reviewer for this suggestion which will clarify the objective of the paper. The new title is “*Low Level Stratiform Clouds and Dynamical Features observed within the Southern West African Monsoon*”. However, as reviewer #1 can see, we did not add the location of the observations because it

makes a too long title. This information is now clearly specified at the very beginning (Line 7) of the summary.

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?

We meant that quantitative and precised description of the low troposphere and clouds from the observations were missing before DACCIIWA, for the models to be able to improve their ability to represent them properly.

We have revised the abstract accordingly:

*“Moreover, numerical climate and weather models need a finer description and knowledge of cloud macrophysical characteristics and of the dynamical and thermodynamical structures occupying the lowest troposphere, in order to be properly evaluated in this region”*

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.

Sorry, we meant “according to synoptic atmospheric condition”. We corrected this in the new version.

We have reworded the sentence to avoid redundancy:

*“The maritime inflow reaches Savè around 1800-1900 UTC on average. This time occurrence is correlated with the strength of the monsoon flow”*

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).

We agree with the reviewer and we corrected the sentence as suggested.

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.

Thank you for the reference website for the definition of the field campaigns.. We reworded the sentence: *“Filling the gap of observations and studying the LLSC life cycle were therefore the primary goals of the Dynamics-Aerosol-Chemistry-Cloud Interactions in West Africa (DACCIIWA) project (Knippertz et al., 2015) with an aircraft and ground-based campaigns (Flamant et al., 2017, Kalthoff et al., 2017) performed during summer 2016.”*

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.

Yes, this has been corrected.

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.

The Savè super site was the most instrumented site. Sodars were implemented at Kumasi and Ile-Ife super sites but they provided wind only up to 300 m, preventing the study of the low level jet.

Furthermore, no cloud radar was available at these two sites, and the cloud summit could not be determined properly.

We added a sentence to explain this in the text:

*“The focus on Savè for this study is motivated by the fact that only this site was instrumented in such way that continuous profiling of the wind up to several kilometers and continuous determination of the cloud summit were accessible. This was not the case at Kumasi or Ile Ife”*

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

We defined COSMO in the revised version of the manuscript: *“COSMO (Consortium for Small-scale Modeling)”*

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.

We agree. The sentence is now as suggested: *“Sections 4 presents the results for the NLLJ and MI and section 5 for the LLC.”*

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.

We agree that the sentence is unclear. . We clarified the sentence and reworded this paragraph in the revised version of the manuscript: *“A systematic comparison of the radiosounding temperature profiles with the HATPRO temperature profiles (not shown) revealed a systematic cold bias of 0.2 K below 550 m, 0.5 K in the 550 - 1000 m layer, and 2 K in the 1000 -2000 m layer.”*

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.

We reworded this sentence in the revised version as:

*“This criterion on wind direction allows to exclude atmospheric conditions associated with vortex circulations, deep convection and Harmattan flow.”*

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?

We followed this suggestion and cited Couvreaux et al. (2010) at the end of the sentence.”

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.

We thank the reviewer for this comment. Instead of certify that there is a “clear diurnal cycle” we quantify the turn in wind direction along the day with an amplitude around 32°. The paragraph is now: *“The median strength of the monsoon flow is roughly 3.5 m s<sup>-1</sup> between noon and 1700 UTC with a 210° direction. The median strength regularly increases between 1700 and 0100 UTC up to 5.5 m s<sup>-1</sup> with a simultaneous slight shift in the median wind*

*direction (amplitude around 30° and standard deviation around 9.46). These same changes are observed in wind surface measurements (Kalthoff et al., 2018)."*

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."

We agree with the reviewer and modified the sentence accordingly:

*"This last criterion 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?

It is true that 4 criteria are used to determine the MI arrival time and that could seem a bit confusing. The 302K isentrope was first used by Deetz et al.. This criteria has no clear and objective justification, except that it works well on one cases simulated by Deetz et al. We wished to propose a criterion built on MI characteristics as observed at surface in Savè and which are an increase of wind and a decrease of temperature. Fuzzy logic function seemed to be the most efficient way to study at the same time each change separately and merge their effect in one criterion. We did not wish to remove totally the 302K criterion since it already has been published and finally gives some quite consistent MI occurrence time.

We added this sentence to explain why we suggest a new criterion: *"Since the 302K criterion relies on one simulated case and because no clear justification for this 302K value exists, another criterion based on MI characteristics observed at surface is proposed in this study. MI arrival at Savè should be detected at surface by a combination of both an increase in horizontal wind and a decrease in temperature."*

16. Page 9, Line 11: It would be helpful to rewrite Equation (1) here plugging in the values for  $y_1$ ,  $y_2$ , and  $(r_x)_2$ :

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

We changed the equation 1 as suggested by the reviewer. However, the numeric values of  $r_{x2}$  depend on the time series of the temperature or wind speed modifications. For each day,  $r_{x2}$  is the value  $r_x$  corresponding to 99 percentile. We reworded this paragraph as: *"where  $r_x$  is the rate of change of the variable  $x$ ,  $r_{x1}$  (resp.  $r_{x2}$ ) is a constant value below (above) which  $FLF_x$  is equal to  $y_1$  ( $y_2$ ).  $r_T$  is multiplied by  $-1$  to obtain positive changes for decreasing temperature. As in Coceal et al. (2018),  $y_1$  and  $y_2$  are set to 0 and 1, respectively and  $r_{x1}$  is set to 0 (i.e., no increase in wind speed or no decrease in temperature). Instead of using the maximum value of  $r_x$  divided by two for  $r_{x2}$  (Coceal et al., 2018), for each day, we use the value corresponding to the 99-percentile of  $r_x$  divided by two to avoid outliers."*

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  $\bar{x}$  as the average of the wind speed and negative temperature tendencies.

The mean FLF function is defined as the mean algebraic average of the two other FLF functions. We rewrote this sentence as:

*"In this study, the mean fuzzy logic function  $FLF_{mean}$  is computed using equal weights for  $FLF_T$  and  $FLF_{ws}$ , and the same threshold of 1 is used to detect combined changes in the dynamic and thermodynamic conditions."*

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?

We intended to say that the use of a fuzzy logic function only on wind speed or temperature changes does not make sense: detection of temporal gradient would have been sufficient. We changed the sentence as follows:

*"The fuzzy logic method only makes sense if the temperature and wind speed changes are combined."*

19. Page 10, Line 8: Is this supposed to be criterion 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.

The misunderstanding comes from an unclear explanation of the criteria for the NLLJ detection. The NLLJ core below 500m is only used at the settlement of the NLLJ, since the core rises in height with time. We clarified the paragraph which details the criteria for the NLLJ detection (page 8): *"The detection of the NLLJ is based, in this study, on the use of dynamical and surface stability criteria: (i) the wind direction in the lowest atmosphere below 1500 m is between the south-east and west-northwest with (ii) a maximum wind speed of at least 5 m s<sup>-1</sup> and at least 2 m s<sup>-1</sup> larger than the minimum above and (iii) a surface sensible heat flux lower than 10 W m<sup>-2</sup>. This last criterion ensures stable to neutral conditions at the surface. The onset of the NLLJ is defined when these criteria are satisfied for at least two hours and the height of the maximum wind speed is below 500 m. The breakup time is defined when one of the three criteria mentioned above has not been satisfied for at least 1 hour. The use of the surface sensible heat flux as a diagnostic of the stability may be a limitation to this method because this measurement is very local and may not represent atmospheric stability on large spatial scales."*

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.

We agree with the reviewer that this part of the sentence is not understandable. We decided to simply remove it. It gives:

*"Based on these three examples, one can note that large differences occur that make it difficult to determine solid criteria for the detection of the MI and NLLJ."*

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.

We agree with the reviewer that ‘all times of days’ is not correct. We modified the sentence accordingly:

*“A large increase in wind speed ( $FLF_{ws} = 1$ ) is observed at several times during the day; the largest ( $\geq 5$ ) occurs between 1700 and 2000 UTC. This large range of times is due to the day-to-day variability of the monsoon strength and the arrival time of the NLLJ during this period.”*

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.

We rewrote this paragraph in the revised version as *“The MI arrival times are shown in Fig. 6a. Four estimates of the MI arrival times are displayed, one using the 302 K potential temperature criterion and three corresponding to the first time when the three fuzzy logic functions,  $FLF_{ws}$ ,  $FLF_T$  and  $FLF_{mean}$ , attain a value of 1. Most observations of the MI arrival time at Savè, considering only the wind speed increase, fell between 1600 and 1800 UTC; it is between 1600 and 2100 UTC when we consider only the cooling. The arrival time of the MI deduced from  $FLF_{mean}$ , which couples with an equal weight cooling and wind speed increase, exhibited a strong variability. There are earlier arrival times at 1600 UTC and later ones at 0630 UTC. As noted above, the different tests performed to select the constants and thresholds for the fuzzy logic method yield different MI detection times for each day but quite similar distributions for the period of study. These results suggest that the MI arrival time is difficult to detect with local measurements. However, the MI arrival time as detected by the fuzzy logic function  $FLF_{mean}$  is clearly linked to the mean monsoon flow in the afternoon (Fig. 7): the stronger the monsoon flow strength in the afternoon between 1200 and 1500 UTC, the earlier the MI arrival time. The two exceptionally early arrivals at 1600 UTC shown in Fig. 7 are associated with unusually strong monsoon flow all day long (e.g. the night 9-10 July illustrated in Fig. 4c). ”*

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.

We agree with the reviewer that this sentence was not clearly rewritten and we reworded it:

*“The NLLJ core in Niger and Benin observed during AMMA campaign (Lothon et al., 2008) was roughly at the same height, but the wind speed of the jet core is in contrast more important in Niger, from 10 to 20  $m s^{-1}$ . ”*

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?

G and B are indeed average value in (i) and we corrected it in the new version, with added over lines.

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.

We thank the reviewer for this comment. We missed to mention that LLCs cannot be determined during rain events with infrared camera. We added the following sentence in the new version of the manuscript to clarify this section:

*“Note that during rain events, droplets are retained on the dome of the infrared camera and impact the color of the image as if there was a cloud. Therefore, LLC cannot be detected during rain events*



*which are thus excluded. As far as we know, it is the first time that such methodology is used for the study of the stratus cloud deck formation and breaking.”*

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

We agree that the word “articulation” may not be appropriate here. We modified this sentence in the revised version as:

*“Figure 12 provides a schematic evolution of the NLLJ and of the low level stratus that we observe during the DACCIWA field experiment at the Savè site for the 25 days 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.

Few words are actually missing and we apologize for this. We reworded the sentence:

*“A key need for observations to compare with numerical weather and climate models motivated the field campaign in the DACCIWA project.”*

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.

It is true that MCS were not our focus because their impacts are difficult to investigate with observations at one point. However, we tried to exclude some evident events when we detected density current or rain fall. We added a paragraph explaining what could be the impacts of MCS that could not be detected at Savè and then possibly included in the statistics analyzed in this paper.

*“MCS, when occurred in the surroundings of Savè site, could be detected (rain fall or density current) and excluded from the analyzed days. However, MCS occurring upstream of Savè were hard to detect and could have some more subtle impacts on the monsoon flow or the MI characteristics or propagation.”*

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 wind is turning to the north. Saying the winds become more southerly would be clearer.

We thank the reviewer for his suggestion. We did mean “southerly”, and the text has been corrected.

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.

We thank the reviewer for his correction. These sentences are now:

*“ Low level stratus is a persistent phenomenon that occurs 65% of nights. It forms more than 3 hours (6 hours in average) after the NLLJ onset at the jet core height and persists until noon on 80% of days with nocturnal stratus formation.”*



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.

We agree with the reviewer and added the following sentences: *“The relative contribution of those processes is addressed in Adler et al. (2018) and Babic et al. (2018) for 15 IOPs and one case study respectively. Our work brings an overall statistical analysis of the key dynamical features of the low troposphere during the WAM. It also exhaustively gives quantified diagnostics for each day of the entire period. Therefore, it is an important basis for any future case study, and to model evaluation.”*

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.

Today, there is no embargo anymore on the DACCIWA dataset. We added the URL to access to the baobab database. Dataset DOI are already listed in the reference:

*Derrien, S., Bezombes, Y., Bret, G., Gabella, O., Jarnot, C., Medina, P., Piques, E., Delon, C., Dione, C., Campistron, B., Durand, P., Jambert, C., Lohou, F., Lothon, M., Pacifico, F., Meyerfeld, Y.: DACCIWA field campaign, Savè super-site, UPS instrumentation; SEDOO OMP. <https://doi.org/10.6096/DACCIWA.1618>, 2016.*

*Kohler, M., Kalthoff, N., Seringer, J., and Kraut, S.: DACCIWA field campaign, Savè super-site, Surface measurements; SEDOO OMP. <https://doi.org/10.6096/dacciwa.1690>, 2016*

*Handwerker, J., Scheer, S., and Gamer, T.: DACCIWA field campaign, Savè super-site, Cloud and precipitation; SEDOO OMP. <https://doi.org/10.6096/dacciwa.1686>, 2016.*

**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.

[This has been corrected.](#)

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

[This has been corrected.](#)

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

[This has been corrected.](#)

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.

[This has been corrected.](#)

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

[This has been corrected.](#)

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

[This has been corrected.](#)

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

[This has been corrected.](#)

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

[This has been corrected.](#)

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

[This has been corrected.](#)

10. Page 13, Line 27: Figure 10?

[This has been corrected.](#)