Response to reviewer 3#

We thank the reviewer #3 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.

Interactive comment on “Low Level Cloud and Dynamical Features within the Southern West
African Monsoon” by Cheikh Dione et al.

Anonymous Referee #3
Received and published: 21 January 2019
Review of “Low-level cloud and dynamical features within the southern West African monsoon” by Cheikh Dione et al.
Review summary
The authors describe the occurrence statistics of the nocturnal low-level jet, the maritime inflow, and stratus deck during a 40-day observation period of the DACCIWA campaign at Savè, Benin. The jet and inflow are identified from UHF wind profiler measurements and radiometer-derived temperature profiles. The stratus deck is identified using IR “RGB” measurements. The authors identified the dynamical features on 20-25 days out of 40 (when there was no appreciable precipitation or density current) and the stratus deck on most of those days. Comparing the onset and breakup, the stratus deck was found to initiate approximately 3 hours after the onset of the maritime inflow, while the breakup of the jet was found to occur around sunrise and the breakup of the stratus after sunrise.

The manuscript is generally well-written with a clear structure. The figures present some nice and valuable results, although the figures are sometimes too busy to easily interpret. Some of the statistical findings are reported using vague or incorrect terminology. Overall, the scientific concerns are minor and this paper could be accepted after minor revisions.

Major comments
1. Conclusion / novelty
The authors lean a lot on the work presented in Adler et al. (2018) and Babic et al. (2018). When reading the conclusions, the emphasis appears to be on the findings of those two papers, e.g. the list starting at line 31, page 15. Instead, the authors should highlight in their conclusion how their Figure 12 synthesizes their results. For instance, one could identify three key periods, e.g. 1800-0000, 0000-0700, and 0700-1200 (more or less as done in lines 10-24, page 14). This Figure 12 provides a broader context for the case-study type and process analysis done in the previous studies. The main point here is that from the conclusions, it is unclear what the novelty is of this particular paper (although the introduction does provide this in line 13-19 on page 4).
We agree with the reviewer and reworded some parts of the introduction and conclusions.

2. Busy figures
The amount of information condensed into single figure panels is impressive, but it makes it very difficult to interpret some of these. One might imagine using these figures in a presentation and certain features will be difficult to highlight. Specific issues are:

Figure 2. The grey squares are difficult to see. How important are these for this figure?
The rainy conditions could be presented in separate panels, although that would shrink

We agree with the reviewer the gray squares are difficult to see. However we think the information of rain fall is worth to be indicated for several reasons:
1/ rain fall prevents the detection of LLC with the infra-red camera,
2/ rain fall is sometimes an indication of perturbation of the monsoon flow or MI arrival.
That are the reasons why we decided to keep this information in Figure 2, but we have changed the color of the markers for rain to a more visible color. We hope this makes the figure 2 more legible.

Figure 2. Alternatively, the authors could present the rainfall information in a separate Figure using the same day-hour axes. If the information is not crucial to the paper, it could be provided as a supplementary figure. As it stands, the information is getting lost.
Please see the answer to the previous comment.

Figure 4. The three markers are difficult to discern in this figure. A solution could be to (1) remove the wind barbs to a separate figure and (2) replace the open markers with slightly larger, filled, black markers of different shapes. As a separate point, the barbs are not intuitive to interpret, as they are shown against a height axis. If the barbs were placed in a separate panel, the authors could also colour code them or use a filled contour plot to emphasize different cardinal or intercardinal directions.
We agree with the reviewer that the three markers are difficult to discern in Figure 4. We made a new version of this figure using readable markers.

Figure 10. Although the figure is visually fun, it is difficult to read. The preceding analysis means that the colours are no longer necessary. Without the colors, it would be much easier to interpret the relationship between the onset and breakup of the jets and clouds.
We have thickened the gray line, and used a darker gray for more clarity of this figure. This allows an easier interpretation of the relationship between the onset and the breakup of the jet and cloud.

Minor comments

Page 3, line 4. Please mention the source of “dew point temperature” used in this paper.
The dew point temperature are from ERA_interim data. We added in the new version the following sentence: *“ The ITD mean location in June 2016 is indicated in Fig. 1 and estimated using the 15 ◦ C dew point temperature from ERA interim reanalysis (Buckle, 1996)”*

Page 3, line 18. Please rephrase or clarify in the text what is meant by “convective turbulence”.
We rephrased this sentence as follows: “*A second very important dynamical feature is the NLLJ, which typically forms over land at the end of the day when turbulence in the convective boundary layer has ceased.”*

Page 3, line 20. What are “intertial oscillations”?
We meant frictionless inertial oscillations. We corrected the sentence in the new version of the manuscript. The sentence is now*: “However, due to the low latitude and the low Coriolis force in the DACCIWA region, frictionless inertial oscillations above the nocturnal inversion layer might not be applicable’.*

Page 3, line 23. What does “it” refer to? The NLLJ?
The reviewer is right; “it” does refer to NLLJ. We replaced “it” by “The NLLJ”.

Page 5, line 6. Please give the exact limits of the profiler data, rather than “roughly 150 m”.

The first available wind measurement from the profiler is actually precisely at 150 m a.g.l. So we removed “roughly” in this sentence.

Page 5, line 14. “above and below” this phrasing does not make sense. It suggests that the bias is both 0.5 and 2.0 K between 1000m and 2000m.
We reworded this sentence as the following “*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 between 1000 m and 2000 m.*

Page 5, line 15. “funding” should be “finding”.
The text has been corrected.
Page 5, line 19-24. Given that the UHF data are block-averaged to 15 minutes, and given that it is interpreted alongside the sensible heat flux. Shouldn’t the latter also be block-averaged to 15 minutes? Please specify the averaging performed on these data.
We thank the reviewer for his comment on the different block-averaging periods used for UHF and sensible heat flux. Fifteen minutes averaging period is not long enough to include the large eddy contribution to the turbulent flux with a sufficient statistic. Therefore, 30 minutes sample has been chosen for this study. That means that the temporal resolution for NLLJ arrival time is 30 minutes. We added the following sentence at the end of the paragraph about NLLJ detection: “The NLLJ arrival and breakup times are determined with a 30-min temporal resolution, which corresponds to the sample duration for the sensible heat flux estimation.”

Page 5, line 30. “manufactured” should be “manufacturer”
The text has been corrected.

Page 6, line 7. “most of the time” and “complementary scans”. Please be specific. Did the radar perform a volume scan every 30 minutes? How long did the volume scan take, e.g. 5 minutes? Does that mean that you have five 5-minute estimates of cloud-top height per 30-minute period?
We rephrased this sentence to clarify the acquisition mode: *“It was run with vertical pointing every 5 minutes and horizontal scans every 30 minutes.”*

Page 6, line 14-22. This analysis seems really nice and original. Is it designed in this study? Perhaps the authors could emphasize this more. If not, please provide references.
Yes, this analysis was designed in this study for the first time, so there is no reference to cite for it. We emphasized on this as follows: “This instrument is used here to study the horizontal homogeneity of the cloud deck and to define the onset and breakup times of the stratus deck, with a newly designed method."

We also added a sentence at the end of the description of the method: “As far as we know, it is the first time that such methodology is used for the study of stratus cloud deck formation and breaking.”

And we finally emphasized on this aspect in the conclusion.

Page 6, line 29. Should “height” be “top” (of the monsoon flow).
We meant “top”. This has been corrected.

Page 6, line 33. “depth” should be “layer”.
We meant “height”.

Page 7, line 29. The monsoon depth is less than 1500m in the middle of the night.
We changed the text as follows: " The median of the monsoon depth shows a weak diurnal evolution from a minimum value of 1200 m a.g.l. during the night to 2000 m a.g.l. during convective conditions (Fig. 3a), with a day-to-day variability"

Page 7, line 31-33. The authors use a reference from 2010 to describe the status of the monsoon in their 2016. Please consider rephrasing this sentence.
We agree that the sentence was misleading. It was a comment on what was observed during AMMA in early monsoon season. However, this comment is not essential and has been removed.

Page 8, line 18. It is important here to note the temporal resolution of surface sensible heat flux, if it is different to the other measurements (see previous comment for page 5).
As explained in the response to a previous comment, the flux are estimated over 30 minutes because it allows a correct statistic of the large turbulent eddies which contribute to the vertical transfer of buoyancy. Doing this, we agree that the UHF and the sensible heat flux are not at the same temporal resolution. Consequently the arrival time of the NLLJ is determined with the coarser temporal resolution which is the sensible heat flux one, meaning 30 minutes. We added a comment on the text about this: "The NLLJ arrival and breakup times are determined with a 30-min temporal resolution, which corresponds to the sample duration for the sensible heat flux estimation."

Page 8, line 29. How is the 302 K potential temperature measured? Is it based on the radiometer profiler? Please specify.
The 302 K potential temperature is actually based on the radiometer measurements. We have specified this more clearly in the revised version: "This criterion was applied to the temperature measured locally by the microwave radiometer at the Savè site in order to detect the arrival of the MI."

Page 9, line 15-16. "affected" should be "applied".
This has been corrected.

Page 9, line 18-20. Please enlighten the reader to what range of thresholds are appropriate for r-ws and r-T, and which values were chosen for the subsequent analysis.
We thank the reviewer for this comment. The paragraph presenting r_{ws} and r_T was actually incomplete. 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 rx 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 y1 (y2). r_T is multiplied by −1 to obtain positive changes for decreasing temperature. As in Coceal et al. (2018), y1 and y2 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."*

Page 10, line 1-2. "the wind maximum increases" – it is the "height" of the wind maximum that increases. Please rephrase.
Yes, we indeed meant that 'the height of the wind speed maximum' increased. This is reworded in the revised version.

Page 10, line 14. “if the same scenario appears every day” – surely, the authors mean that it is difficult to determine criteria if “different” scenarios appear each day? (i.e. the opposite)
This paragraph was not clear enough. It is reworded in the revised version as *“Based on these three examples, one can note the large variability that can be observed from one day to the other, which makes it challenging to define solid common criteria for MI and NLLJ detection.”*

Page 10, itemized points. These conclusions cannot be drawn based on Figure 5. An increase in wind speed is not observed “at all times”. It “may” be observed at any time, but certainly not at all times for all days. Similarly, cooling “may” occur between 1700 and 0000 UTC, but it certainly does not occur throughout that period for all cases. FLF_mean=1 does not occur “during the entire night” for all cases. If any of these statements were true, then we should see that the temporal occurrence equals the total number of days for a prolonged period of time in figure 5.
We thank the reviewer for his comment. We reworded this section in the revised version as

- *A large increase in wind speed (FLF_{ws} = 1) may be observed at any time during the day; however the largest occurrence (> 5) is between 1700 and 2000 UTC. This variability is due to the day-to-day variability of the monsoon strength and the arrival time of the NLLJ during this period.*
- *As expected, cooling may occur between 1800 and 0030 UTC the following day. Contrary to the wind speed, whose fuzzy logic function reaches 1 but rarely remains at that value for several hours during the night, while the temperature fuzzy logic function reaches this value many times during the night. This trend implies continuous cooling (Fig. 4). This result is in accordance with the continuous decrease in temperature within the MI from north to the south discussed by Adler et al. (2018).*

Page 11, line 3. “most probable” this means the time with the highest occurrence. Instead, the authors appear to refer to the median.
We reworded this sentence in the revised version as “*Most of the observations of MI arrival time at Savè considering only the wind speed increase fell between 1600 and 1800 UTC; while they fell between 1600 and 2100 UTC when we consider only the cooling.'*

Page 11, line 5-6. “exhibited a nearly symmetric distribution centered at 1800 UTC.”
This distribution does not appear symmetric: it has a long tail towards later times. Also, it has a maximum at 1730 UTC.
We rewrote the paragraph in the new version of the manuscript: “The arrival time deduced from FLFmean, which couples with an equal weight cooling and wind speed increase, is observed between 1600 UTC and 0000 UTC, with nevertheless the most probable arrival time at 1800 and 2000 UTC.”

Page 11, line 11. “clearly linked” – what is the reason for this statement? Visual inspection of the scatter plot? The authors should include a correlation value and its significance here.
We based on the correlation coefficient between the strength of the Monsoon and the arrival time of the MI. We added in the revised version the following sentence to clarify “The absolute value of the correlation coefficient between both is 0.61.”
Page 11, line 16. The “most frequent” onset seems to be at 1745 UTC, not 1900.
This has been corrected. The most frequent onset is actually 1730 UTC.

Page 11, line 22. “to reach 700” – this looks like 500 in Figure 8.

That is true, and this is a mistake. We corrected in the revised version as *'to reach about 500 m at NLLJ breakup time'*

Page 11 line 23. "most likely" this again appears to be the "median", which is a different measure.
We reworded this in the revised version; we changed "the most likely strength" by the "median strength".

Page 11, line 31. Is there a reference for this "precious dataset"?
As far as we know there is no specific reference for this dataset. However, some of these data are available on the DACCIWA (baobab) database.

Page 12, line 14. "are said to occur" – please provide a reference.
There is no reference for this. This is a clumsy wording for our own statement here. We reworded this sentence as "Here, we set the occurrence of the low stratus clouds when..."

Page 12, line 14. Should the G and B also be average values?
This has been corrected.

Page 12, line 33. "when they are large enough" – the clouds? Please specify.
We thank the reviewer for pointing out this unclear sentence. It is now splitted in two sentences as follows: "After 10:30 UTC, the cloud base rises and the fractioning of the cloud base (less steady red-pink color, large σRGB for a long duration of time) increases with the development of the convective boundary layer. It defines the end of the stratus LLC"

Page 13, line 20. This section should be "LLC lifetime statistics". "Macrophysical characteristics" suggests a description of the thickness and liquid water path of these clouds.
We thank the reviewer for this suggestion. The title has been changed.

Page 14, line 7. "LLC always form" this is not true. There are days where the NLLJ forms, but no LLC are observed. Perhaps: "on the days that LLC form, they appear more than 3 hours."
The use of the word 'always' is not appropriated, we removed it in the new version, and followed the reviewer suggestion. The sentence is now: "On the days that LLC form, they appear usually more than 3 hours after the onset of the NLLJ and clear up after the NLLJ breakup time (Figure 10)."

Page 14, line 14-16. It appears as if the authors combined different days to produce their Figure 12. It would be a more reliable result if the authors could ensure that their sample is consistent across the three statistics, i.e. only select those days that have both a NLLJ and a cloud deck.
Several approaches could be followed here, for an increase of statistics in each phase in one hand, or more consistency all along the diurnal cycle in the other hand. We have verified that the result was not altered if we considered only the days that have combined NLLJ and cloud deck.

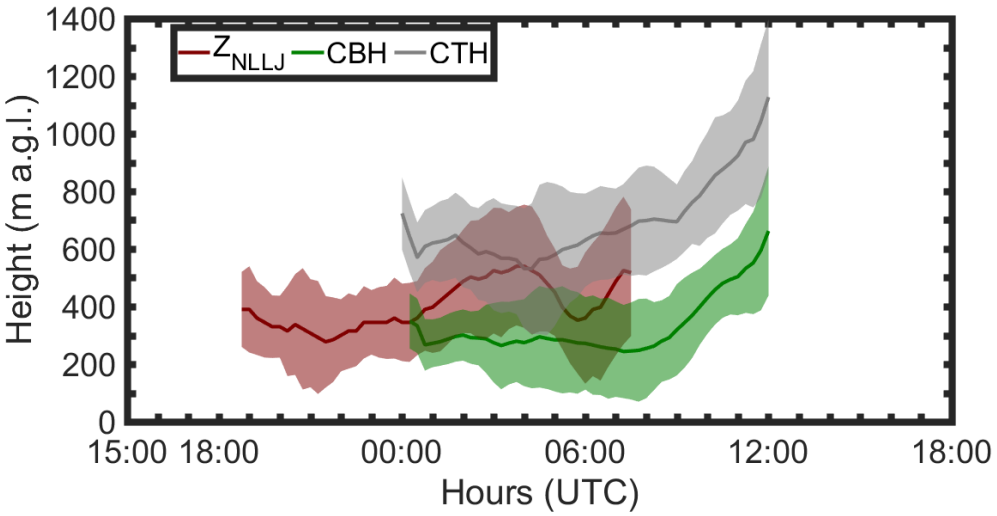

Figure1: as in figure 12 but for only cases that we combined NLLJ and cloud deck

Page 14, line 23. “after sunrise or later” – either say “after sunrise” or be specific about “later”, e.g. “after sunrise or up to X hours later”.
The sentence is corrected in the new version:” It sharply increases after 0800 UTC when the convective boundary layer develops.”

Page 15, line 18. “the most frequent occurrence is at 1800 UTC” – this is true for the FLF-mean measure, but not the others. Is it really “most frequent” that is the useful statistic here? Why not report the median?
We corrected by adding the median of the arrival time of the MI in the revised version. “ *The MI arrival time at the Savè site occurs between 1600 and 2100 UTC; the median occurrence time is at 1900 UTC.”*