RESPONSE TO RC1

Referee #1: We are grateful for the in-depth comments and specific analysis advice the referee provided for our manuscript. This helped significantly improve our results' robustness and sharpen our discussion. Below, we give a point-by-point response to the comments with line references to the new manuscript version. We also provide a tracked changes document highlighting the differences from the previous version.

General comments/questions:

- The authors used 1200 UTC and 1800 UTC, respectively, as the reference times for pre-MCS conditions and the identification of an "MCS day". While this is intuitive in many ways (e.g. highest occurrence frequency of convection in the afternoon/early evening), an entity that is excluded with this definition is nighttime, potentially fast-moving convective systems (e.g. squall lines), which also occur over SWA (Fink and Reiner (2003)) and which can also be important rain contributors in the region (Maranan et al. (2019)). Although I acknowledge that a full analysis exceeds the scope of this study, I believe it is worth including and discussing them (e.g. at 2100 UTC vs 0300 UTC, provided the sample size is high enough), for instance, by extending Fig. 10 by this subset of nighttime convection. I can imagine that lacking CAPE during the night, these convective systems are increasingly shear-driven.

Thank you for the suggestion. We agree that nighttime convective systems are important contributors to rainfall over the entire West African region. Nonetheless, this work focused on the time when the frequency of MCSs reaches a maximum (e.g. Mathon and Laurent 2001).

- Likewise, I do think it is worthwhile to investigate the environmental conditions of "no-MCS" cases. As far as I've understood, the mean-state composites contain all daily timesteps in the 1980-2020 period. Therefore, the anomalies for this case can be integrated in Fig. 10 as well.

The scope of the work looked at investigating environmental conditions favourable for MCSs over the region. Considering "no-MCS" events is out of the scope of this work but can be looked at to improve this work as new research.

- When CAPE is discussed, CIN should be evaluated alongside. This might even be of some relevance for the "no-MCS" cases in the previous bullet point.

Thank you for the suggestion. We agree with the referee on this. Here, we only focussed on variables that contribute to instability in the atmosphere and also lead to a stronger updraft. That is why we did not consider CIN since CIN inhibits the formation of deep convection.

- Can the authors explain why they changed from 925 hPa specific humidity in Fig. 3 to TCWV in Fig. 10? Although TCWV is primarily influenced by the evolution of the humidity field in the lower troposphere, I would stay consistent here by choosing one.

As pointed out by the referee, we considered the TCWV due to its ability to represent the total gaseous water in the vertical column of the atmosphere which is influenced by the evolution of the humidity field. TCWV represents the precipitable water the atmosphere holds better than the humidity. We, therefore, had to show both since in the first instance we were looking at an environment that is suitable for instabilities in the atmosphere, of which humidity forms a part.

- A couple of questions regarding MCS data and definitions which may be clarified in the manuscript as well:
    - 1. Can the authors elaborate why METEOSAT data is limited to 2015?

Actually, the METEOSAT data is up to 2018. The limit was chosen to cover the period over which we could identify match-ups between microwave rainfall estimates from IMERG dataset and MCSs between 1800 and 2100 UTC. The IMERG data only starts in the 2000s, and since we wanted MCSs that produce precipitation of a minimum of 5mm, we had to consider MCS with overlapping maximum rainfall pixels. The period 2004 - 2018, therefore, gives us a good representation of rain-producing MCSs.

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    - 2. Have the authors used 15-minute METEOSAT data?

Yes. The authors used 15-minute METEOSAT data, and this has been added to the manuscript for clarity.

The Meteosat Second Generation (MSG) cloud-top temperature data, which are available every 15 minutes from the Eumetsat archives online (https://navigator.eumetsat.int/product/EO:EUM:DAT:MSG:HRSEVIRI) was used in this study.

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    - 3. What do the authors mean by "5 MCS snapshots"? The detection of at least 5 MCSs or the detection of MCSs at five timesteps between 1600 and 1900 UTC? If the latter, is an MCS day identified irrespective of the overall number of MCSs at 1800 UTC? So the detection of a single MCS is sufficient?

Thank you for the opportunity to clarify. The "5 MCS snapshots" which have now been changed to "5 MCSs" is the detection of at least 5 MCSs between 1600 and 1900 UTC per day and not 5 timesteps. This means we expect to detect at least 5 MCSs within the timeframe to call it an MCS day.

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    - 4. Are only land-based MCSs accepted? What would be the criterion for the position? Center of mass?

We considered only the land-based MCSs because MCSs over land are fundamentally more intense and deep than its counterpart over the ocean (Mohr and Zipser 1996). This has been included in the manuscript to give a clear explanation.

Here, only land-based MCSs because MCSs over land are fundamentally more intense and deep than its counterpart over the ocean (Mohr and Zipser 1996).

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    - 5. How did the authors determine the rainfall amount? What dataset is used for this?

The rainfall amount was determined from rainfall snapshots of the "high-quality precipitation" (HQ) a field within the Integrated Multi-satellite Retrievals for Global Precipitation Measurement (IMERG; Huffman et al. 2019) dataset. This has been included in the manuscript as follows:

This can include the same MCS at several timesteps in a day. Corresponding rainfall snapshots were sampled from the "high-quality precipitation" (HQ) field within the Integrated Multi-satellite Retrievals for Global Precipitation Measurement (IMERG; Huffman et al. 2019) dataset.

- Up until Fig. 6, the extent of the "SWA region" never became clear. The authors may consider including an introductory map of West Africa (e.g. orography) with the SWA region outlined.

Thank you for the notification. The “SWA domain” is now shown earlier in Fig. 2 with coordinates clearly shown in the caption of Fig. 2.

- Can the authors clarify in more detail how nodes 4 and 5 have to be distinguished in the context of the evolution of the WAM? From Fig. 2, the only major difference I can spot is that the background geopotential in node 5 is higher than in node 4. Is that also what the SOM technique identified as the decisive difference to define a dedicated node?

Find the answer to the comment above. Comparing nodes 4 and 5 in Fig. 1, it can be clearly seen that the frequency of node cases in Fig. 1 largely occurs during the months of September, October, and November. It can also be seen in node 4 that the frequency in node cases persists in the pre-monsoon season as well as the post-monsoon, with a similar number of cases for some months in both pre-and post-monsoon seasons, although more prominent in the post-monsoon season.

Yes! From Fig. 2, that is what the SOM technique identified as the decisive difference between nodes 4 and 5. That clearly means the background geopotential during the post-monsoon season itself is high as in Fig. 5. The presence of persisting node cases observed in the pre-monsoon season in node 4 reduces the effect of the background geopotential for events in that node.

- What atmospheric patterns are shown in the remaining three nodes which were dismissed for this study? How many days were then excluded from the overall sample?

Thank you for the opportunity to clarify. In choosing the size of the SOM, we considered the distinctiveness and robustness of the circulation systems. A too-small node size reduces the robustness of capturing the predominant spatial characteristics and a too-large node size introduces redundant nodes. The 9-node SOM was chosen as a compromise on states not being overly generalized while capturing the dominant spatial characteristics over the region. However, three (3) nodes were observed to be similar but low in frequency when compared to the other nodes. These 3 similar nodes were combined with the other nodes showing similar features.

In effect, no nodes were dismissed but rather they were grouped with the ones that are obviously quite similar in atmospheric patterns and seasonal frequency.

<u>Specific comments/questions:</u>

- L42: What reference is “Change 2014”?

Thank you for pointing this out. We have corrected this reference by replacing it with “IPCC 2014”, with its corresponding reference added accordingly as IPCC: Climate Change, 2014: Synthesis Report. Contribution of Working groups I, II, and III to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Core Working Team, R.K. Pachauri and L.A. Meyer (eds)]. IPCC, Geneva, Switzerland, 151, 2014.

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- L76: “In previous studies that evaluated MCS-favouring atmospheric environments, less attention was given to the importance of large-scale WAM modes and their effect on regional MCS frequencies in SWA”. Nonetheless, there are studies that address the largescale settings for WAM-related rainfall throughout the seasons, e.g. the studies by Sultan and Janicot (see reference). Although they do not refer specifically to MCSs, MCSs are part of the WAM rainfall patterns.

Here we absolutely agree and the manuscript highlights that as well that previous studies address the large-scale settings for WAM-related rainfall. The focus of this manuscript was on suitable conditions favourable to the changes in the frequency of MCSs over SWA. Nonetheless, we have rephrased the statement in question to encapsulate the suggestion above.

- L95: “For this purpose, a classification using a self organizing map (SOM; Kohonen 2001) analysis was carried out to characterize large-scale WAM patterns during the 1981-2019 period”. Any reason why the mapping was performed until 2019, but the analysis of atmospheric fields until 2020?

Corrected. The mapping was performed until 2020 and the same was used for the analysis.

- L109: Better “137 vertical model levels”.

Corrected as pointed out

- L111: Can the authors explain what they used the 250 hPa horizontal wind for? Have the authors also investigated the Tropical Easterly Jet? In any case, the 250 hPa wind was never addressed anymore.

We removed the 250 hPa wind level since it was not used anywhere in the manuscript.

- L129-133: Might be better to shift this to the introduction.

We agree with this suggestion and have therefore moved this statement to the introduction.

- L168: Seasonal cycle of what? Monthly rainfall amount?

It is the seasonal cycle of monthly rainfall amounts as suggested.

- L185: Do you mean “low pressure”?

Here, we were talking about the West African heat low (WAHL) which is known to be a region of high surface temperatures and low surface pressures. It is, therefore, a low-pressure region as stipulated and forms part of the West African monsoon system.

- L214: “…show significant changes over the last 4 decades”. In what way exactly?

Thank you for the opportunity to clarify. Based on a mann-kendall trend test conducted along with a test of significance, it is clear that trends observed in Nodes 4 and 5 are significant with p-values of below 0.05 in both nodes.

- Sec. 4.2: Have the anomalies been calculated from the mean state in Fig. 2, i.e. based on 1981-2020? Since the MCS days run from 2004-2015, have the authors account for potential trends between the periods 1981-2003 and 2004-2015?

Thank you. We did not account for potential trends between the two periods. Looking at the period each of them covers (ie. mean state and MCS days), we assume they represent the general behaviour of trends in each state and therefore no need to account for any differences.

- Figure 5: A bit surprising to find zero MCSs in February, but probably a consequence of the high areal MCS criterion chosen in this study.

The focus of MCSs over the study area in this study is during the rainfall season of the SWA domain which mainly starts in March and ends in November. February recorded zero because it wasn't considered in the frame of this work.

- L227: Again, the definition of the SWA domain needs to be outlined earlier.

Done. The SWA domain has been outlined earlier in Fig. 2.

- Figure 6: Again, does "location of the MCS" refer to the center of mass of the cloud area? Does the MCS frequency refer to the amount of MCS days compared to the total number of node days? Does that explain why there are much more MCS dots for node 6 than node 5, the latter of which has a higher frequency?

Here we agree to the view above.

- L248: What do the authors mean by "insignificant behaviour"?

In the manuscript, all anomalies show only regions that are significant at the 5% level. Areas with "insignificant behaviour" are where the two-sided Student's t-test depicts insignificant differences between node climatologies and MCS-day sub-samples.
We have added a phrase to that statement to clarify this point.

- L250: Also seemingly partly northerlies from the Mediterranean region.

The suggestion has been added to the manuscript.

- Figure 8: Can the authors add the maps for the mean-state of vertical wind shear in section 4.1 and discuss them for more clarity?

We have added the maps for the mean-state of zonal wind shear and discussed it accordingly as shown below:

A further investigation was conducted to ascertain the spatial distribution of mean zonal wind shear over SWA (Fig. 4). The patterns demonstrate northward transport during the propagation of the WAM cycle and a wider spread of zonal wind shear as it moves further inland (nodes 1, 2, and 3). These patterns closely follow the southern boundary of weaker geopotential heights representative of high-pressure areas (Fig. 2). During the monsoon season (node 6), zonal wind shear lies clearly to the north of the SWA domain. A southward retreat of zonal wind shear is observed during the post-monsoon season (nodes 4 and 5). Generally, the presence of zonal wind shear can be seen as a necessary condition in the WAM system.

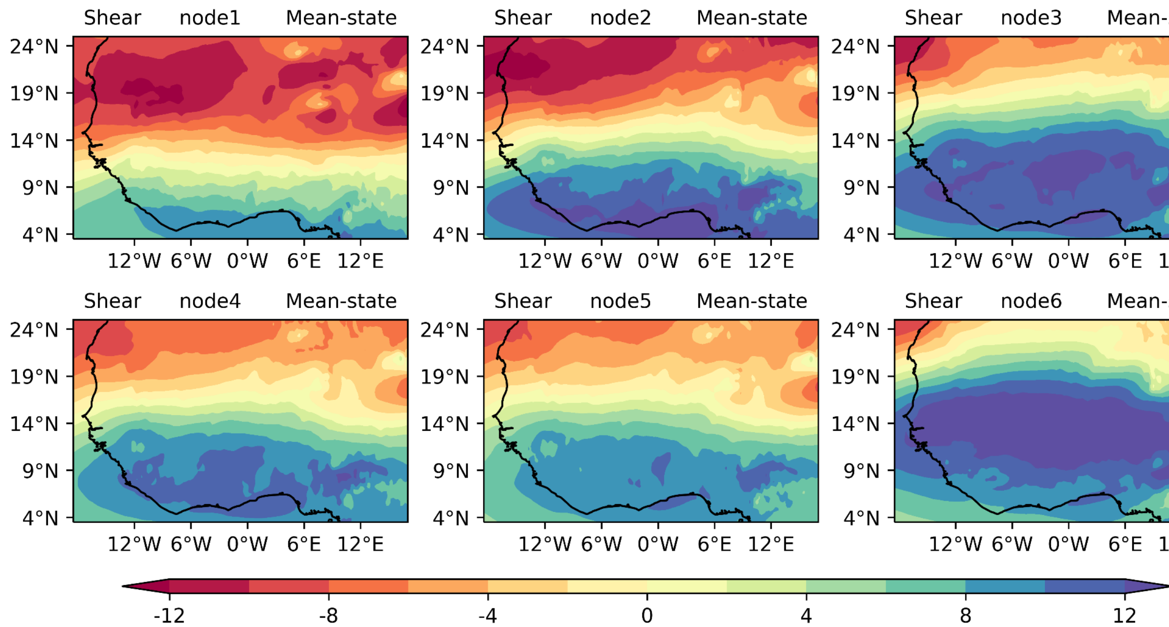


Figure 4. 12 UTC composites of zonal wind shear in six nodes based on SOM analysis.

- Figure 9: As mentioned, CIN should be shown and discussed as well.

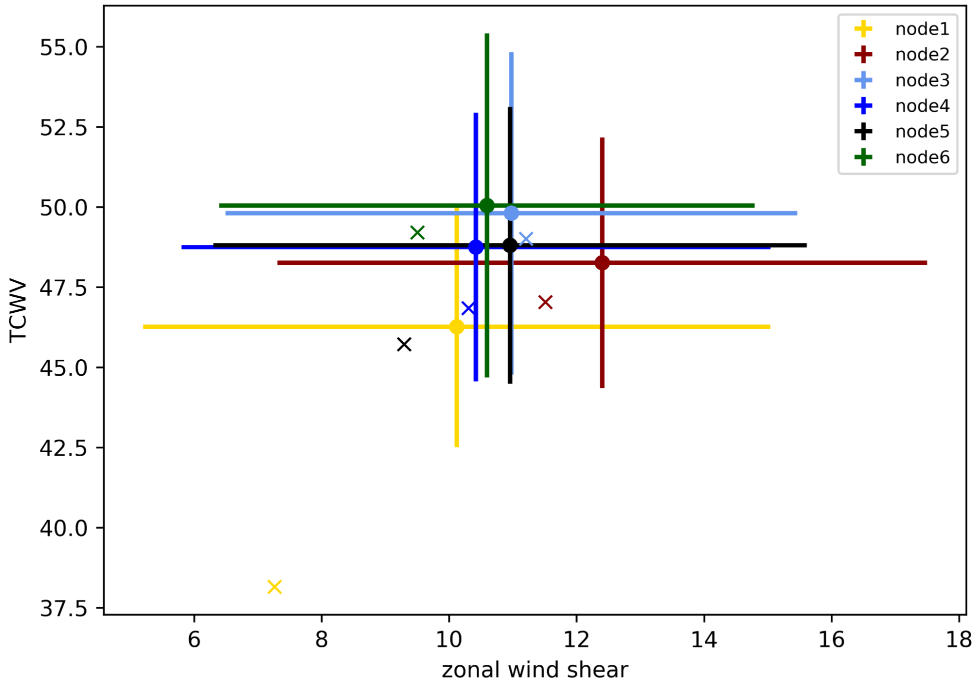
As said earlier, CIN is not considered in the manuscript because we concentrated on parameters that create instability and promote strong updraft.

- L309: "…illustrating the relatively storm-hostile mean conditions…". But doesn't the mean state include all time steps, including MCS days? As outlined in the general comments, the authors may add the specific non-MCS state in Fig. 10 for clarity.

Thank you for pointing this out. We have corrected the statement.

- Figure 10: The reddish colours are hard to distinguish.

We agree with this assertion and have addressed it accordingly.



55.0
52.5
50.0
47.5
45.0
42.5
40.0
37.5
TCWV
6
8
10
12
14
16
18
zonal wind shear
node1
node2
node3
node4
node5
node6