RESPONSE TO REFEREES

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.

    - 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.

    - 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.

    - 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).

    - 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.

Specific comments/questions:

- 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.

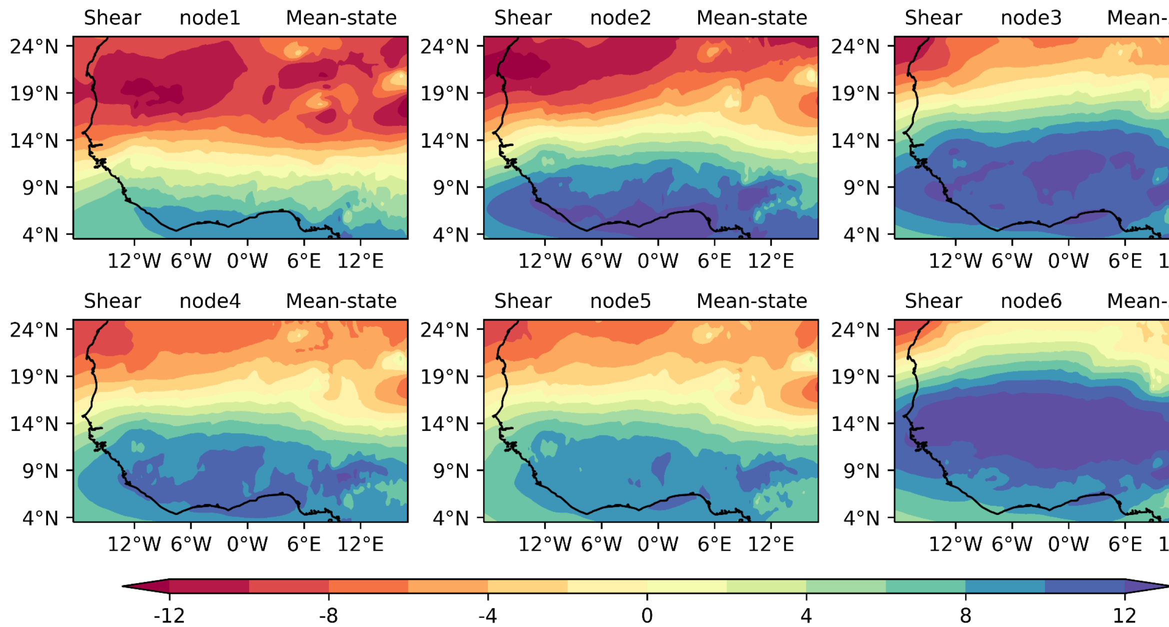


Figure 4. 12 UTC composites of zonal wind shear in six nodes based on SOM analysis.

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

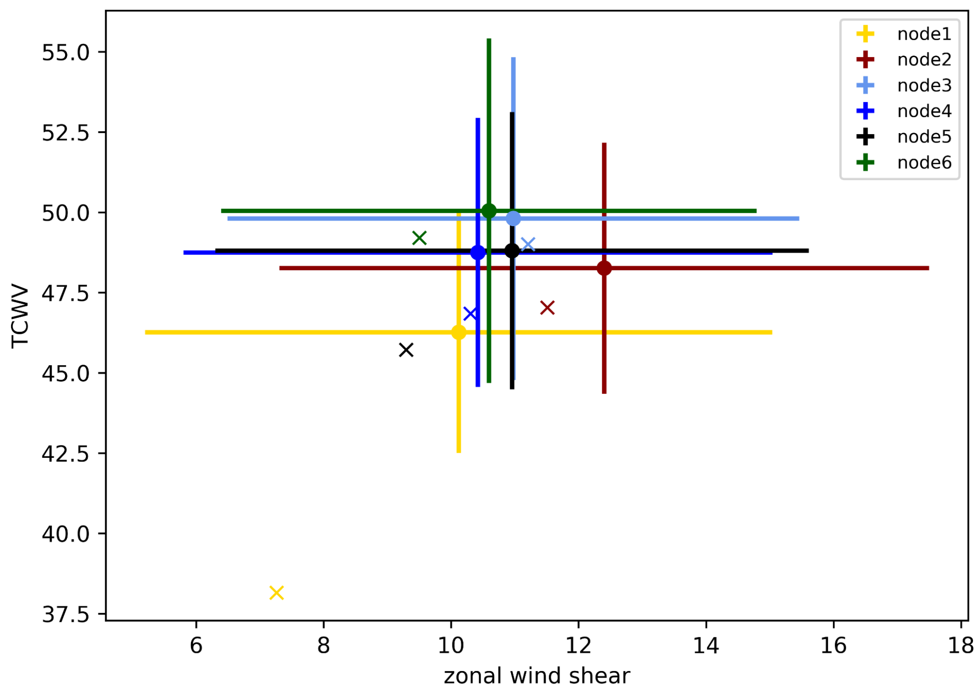
As said earlier, CIN is not considered in the manuscript because we concentrated on parameters that create instability and promote strong updraft.

- 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.



Referee #2: We thank the referee for their valuable input, which helped to improve the clarity of our manuscript and figures. The line references below refer to the newly changed manuscript. Additionally, we provide a document with tracked changes.

General comments:
1) The SOM analysis should be presented as a whole, and the reason for rejecting certain nodes from the analysis should be better explained. No discussion is dedicated to the SOM configuration, primarily the choice of the number of nodes, but also other SOM parameters (neighborhood size, topology, initial coverage space, etc.). The robustness of the SOM clustering is not evaluated. A significance test should be added for the detected geopotential patterns, and the SOM errors (quantification and topological) should be discussed. Furthermore, the low number of nodes under consideration for this study appears to not fully justify a SOM analysis in the first place. Seeing as each node roughly corresponds to a certain season and is treated as a seasonal mean, it appears that the information presented here can be yielded by a simple seasonal decomposition. Ideas to enrich the SOM analysis and the gain from it can be found in the literature quoted by the authors. Otherwise, the authors may consider replacing the SOM analysis with a simple seasonal decomposition. Please also refer to more specific comments in this regard, below, and the following highly relevant references with very similar motivations and methodologies:

- Liu, Y., Weisberg, R. H., and J. I. Mwasiagi (Eds.): A review of self-organizing map applications in meteorology and oceanography, Self-Organizing Maps: Applications and Novel Algorithm Design, InTech publications, Rijeka, Croatia, 2011.
- Gueye AK, Janicot S, Niang A, Sawadogo S, Sultan B, Diongue-Niang A, Thiria S 2010 Weather regimes over Senegal during the summer monsoon season using self-organizing maps and

hierarchical ascendant classification. Part I: synoptic time scale. Climate dynamics. doi:10.1007/s00382-010-0782-6
- Espinoza, J. C., Lengaigne, M., Ronchail, J., and Janicot, S.: Largescale circulation patterns and related rainfall in the Amazon Basin: a neuronal networks approach, Clim. Dynam., 38, 121–140, https://doi.org/10.1007/s00382-011-1010-8, 2012
- Givon, Y., Keller Jr, D., Silverman, V., Pennel, R., Drobinski, P., & Raveh-Rubin, S. (2021). Large-scale drivers of the mistral wind: link to Rossby wave life cycles and seasonal variability. Weather and Climate Dynamics, 2(3), 609-630.

The comments and suggested references are well received. However, we will like to address these comments as follows:
First, we do state that we use a 9-node SOM however, 6-nodes are representative of the West African monsoon pattern and that is why we present the 6-node states. We should mention that to obtain the best SOM, the third stage of the SOM process evaluates the quantization and topological error. An optimal SOM is obtained when the average Euclidean distance is the minimum (the quantization error is the smallest) and when the proportion of all data vectors for which the first and second best matching units are not adjacent is also minimum (the topological error is the lowest). Once the average quantization error has been minimized, the relationships between the predictor and node data are investigated.

Third, we do not agree with using a simple seasonal decomposition method for this analysis as the SOM has clear strengths when compared with a simple seasonal decomposition. For instance, a simple seasonal decomposition may not identify the combined or mixed pre/post-monsoon states (secondary states) and is likely to identify states as either pre or post-monsoon (primary states), and we will be clearly losing vital information (e.g., Rousi et al. 2015).

In testing for the significance of the identified SOM states, there are several studies (e.g., Hewitson and Crane, 2002; Rousi et al. 2015; Espinoza et al 2012) that support our methodology on the fact that the SOM methodology is data-dependent and such the dominant patterns are representative of the data, thus in the current study a significance test is not necessarily needed. Also, the initial coverage space is mentioned in the data section (domain). We must also highlight here that the SOM is a neural network algorithm as clearly shown in the reviewer's suggested literature (Espinoza et al. 2012).

Nonetheless, we have made additions to the methodology section to provide further clarity and to reflect the reviewer's suggestions.

In this study, the SOM is randomly initialized allowing for hidden patterns and structure in the geopotential height at 925 hPa to be discovered while the algorithm iteratively updates the weights of the nodes to better represent the data. The strength of initializing the SOM this way lies also on its robustness to noise and outliers as a result of the algorithm applying a competitive learning structure to the data which then allows for the formation of distinct clusters. The SOM_PAK algorithm allows the SOM process to minimize quantization and topological errors at the mapping stage when choosing the best SOM as outlined in Lennard and Hegerl (2014). However, there is a trade-off when choosing the size of the SOM, as this is dependent on the need to generalize circulation states for analyses or the need to capture predominant spatial characteristics that affect the local climate. Thus, in this study, we have tested several sizes of the SOM and have arrived at using a 9-node SOM. As depicted in Fig. S1 for a 9-node

SOM, it is evident that some nodes are still redundant, and this is a compromise on states not being overly generalized while capturing the dominant spatial characteristics over the region. Here, we agree on six nodes, which allow distinct synoptic states to be reproduced while grouping nodes that are similar. This grouping was done based on similarities in atmospheric patterns and seasonal frequency from the 9-node case.

2) The choice of low-level geopotential heights as a clustering agent should be better motivated, given the relatively low correspondence between it and the low-level winds in the domain, which are described as the main process driver throughout the manuscript. Have the authors considered directly classifying the wind field?
The choice of low-level geopotential height was made in this manuscript because we wanted to consider somehow the influence of the West African Heat Low (WAHL) in influencing instability. At low levels, the geopotential height well describes the strength of the WAHL (Lavaysse et al. 2009; Biasutti et al. 2009).

3) Nodal trends – this section appears unrelated to the motivations of the paper and is very slim. I suggest a deeper analysis to explore, for instance, corresponding trends in MCS events. Otherwise, consider removing this section.
Thank you. This section has been removed.

4) MCS data – I think this data should be further explored. For one, it can be better presented using a density plot. Secondly, spatial variability should be discussed and possibly explained, with an emphasis on variations between nodes and seasons within the nodes. Finally, it's worth checking for MCS behavior on off-season node days.
Thank you for this great suggestion. We consider this suggestion to be an added value to this idea of MCS's impact on the climate of SWA. However, this manuscript tries to understand the conditions surrounding/ favorable to the formation of MCSs. Therefore, we concentrated on the mean position of MCS. Further research work we will consider in the future will pay more attention to the spatial variability of MCSs.

5) The link to predictability can be improved. For instance, can we learn anything from a lagged correlation between nodal transitions and MCS density?
Thank you for the suggestion. We have considered the suggestion and we will be interested to work on that in our future research which will improve the link to predictability.

Specific comments:

- L24: Too vague. What is the input used for classification? i.e., how do you define a "synoptic circulation-type"?

The 925hPa geopotential height is used as input to train the SOM. The archetypal modes of the geopotential height obtained are used to describe the characteristic circulations over the region.

- L32: Unclear. Do you mean vertical/ horizontal wind shear? what is the field under discussion here?

Here we talk about the zonal wind shear. We have made changes to the manuscript to reflect the exact field under discussion

- L35: The use of the term “shear” or “wind shear” when alternatively referring to vertical and zonal shear is confusing. You should specify which shear is under consideration throughout the paper.

Throughout the manuscript, wind shear or shear is used to represent zonal wind shear. We have therefore replaced “wind shear and shear” with “zonal wind shear” throughout the manuscript.

- L49: Missing a link to WAM. The change of subject is too sudden and does not flow from the previous paragraph. Consider opening the section with lines 53-54

We have taken note of this missing link. We have replaced that statement with “One major atmospheric disturbance that contributes to the WAM is the presence of Mesoscale Convective Systems (MCSs) which supply around 30-80 % of the total rainfall during the WAM (Klein et al. 2018)”.

- L94: “large-scale patterns” - Too vague. You should name the parameter used for the classification here.

Parameters used to represent large-scale patterns have been stated to clarify the statement on L94.

- L121: “SWA domain” - This domain should either be specified in latitude and longitude boundaries or displayed in a figure earlier on. Possibly both.

The domain of SWA has been shown earlier in Figure 2 with the latitude and longitude boundaries clearly specified in the figure caption.

- L124: This section requires more detail. For instance, what is the SOM topology? It would be useful to add a neighbor distances map and to evaluate SOM errors. The number of members in each cluster should also be given, preferably in Fig 1.

This has been taken into consideration and changes have been made to the section to capture the above suggestion. Based on generated Sammon maps, we were able to detect any error in the SOM easily. These Sammon maps use a non-linear mapping technique to create a two-dimensional image of the reference vectors where the distance between node vectors approximates the Euclidean distances in data space. We obtain a very ordered Sammon map in the training of the SOM, which made for a robust interpretation of nodal relationships. Similar types of circulation are close to each other in the SOM space and dissimilar circulations are furthest from each other, which is a characteristic of self-organizing maps.

- L128-129: This statement is true for many optional classification inputs. In the present study, the focus is on the tropics where geostrophic balance is not obvious, as seen by your results. Therefore, the choice to classify patterns using geopotential heights should be justified.

Different training variables were used to capture the regional atmospheric circulation. Our choice of the geopotential height at the low level was based on its ability to represent the impact of the West African Heat Low on the WAM cycle and also to identify the seasonal monsoon synoptic states over West Africa.

- L131: Each method has its advantages and disadvantages, and each can be more suited for a different study. Refrain from making conclusive statements.

We agree with the reviewer's view on refraining from making conclusive statements. In this line, we were merely reiterating what the literature says, however, we have modified this in the manuscript.

- L132: "data is not discretized and orthogonality is not forced" – Again, these are not clear advantages. The SOM's strengths and weaknesses should be discussed in the context of the present study.

The SOM strengths and weaknesses are discussed briefly in our study and the references given are for further reading. Indeed these strengths are well documented and are clear advantages to methods such as PCA and K-means clustering.

- L142: More information is required on what led to the choice of 9 clusters. Have you evaluated the network errors under the different configurations (SOM size and other parameters) to show that 9 is the most compatible?

The network errors are under different configurations for different sizes (4x4, 3x4, 3x3, and 2x3). On testing various sizes, a 9-node SOM was selected that adequately picks out the seasonal variation of rainfall over the region of study. The 2x3 resulted in a more generalized circulation archetype whiles the 3x3 represented a wider range of circulations with fewer redundancies.

- L153: Why not compare to non-MCS days within the node? This may highlight the signal you are after.

Thank you for the suggestion. This is well agreed but the main focus of the manuscript was to understand the synoptic state of the environment on MCS days. A look at non-MCS days can be done as future work to elaborate on signals.

- L154: regarding the T-test – on Which confidence level was it conducted? have you used any method to detect false positives in the multi-gridded test? See Wilks 2016 for example. Wilks, D.: "The stippling shows statistically significant grid points": How research results are routinely overstated and over-interpreted, and what to do about it, B. Am. Meteorol. Soc., 97, 2263–2273, https://doi.org/10.1175/BAMS-D-15-00267.1, 2016.

The T-test conducted was on a 95% confidence level. Anomaly plots highlight only regions at this confidence level (0.05 significance level) as well as wind vectors.

- L165: Why is the complete SOM not shown? This is not clear. If you choose to discard nodes altogether, you should show the full SOM map (9 nodes) first, then explain why not all nodes are relevant, and which ones were removed. The resulting 6-node SOM map should be shown in the context of the full SOM map, as the node locations on the SOM map are crucial for the SOM interpretation. This also raises the question: are the panels in Figures 2-9 arranged correctly? i.e., are neighboring nodes in these Figures also neighbors in the full SOM map? I suggest repeating the analysis for 6 nodes if that's what you end up analyzing, while completely removing irrelevant dates from the SOM input.

The complete SOM has been shown in an attached supplementary material (Figure S1) with the monthly distribution of node cases. Similar nodes from the 9-node case were combined in attaining the 2 x 3 nodes. We grouped nodes (1, 4, 7), and (6, 9) and kept 2, 3, 5, and 8 separate. That would also give 6 groupings seemingly representative of pre- and post-monsoon (1, 4, 7), peak-monsoon (6, 9), pre-monsoon only but different patterns (2 and 3), and post-monsoon only but different patterns (5 and 8). Nodes 1, 4, and 7 were considered as having out-of-monsoon conditions, with MCSs more likely far south. Nodes 6 and 9 show somewhat more Sahelian conditions while the tendency for monsoon retreat conditions was evident in nodes 5 and 8. We also observed pre-onset conditions in node 2 and in weaker terms in node 3. We have added some additional text to provide clarity for readers in Section 3.1.

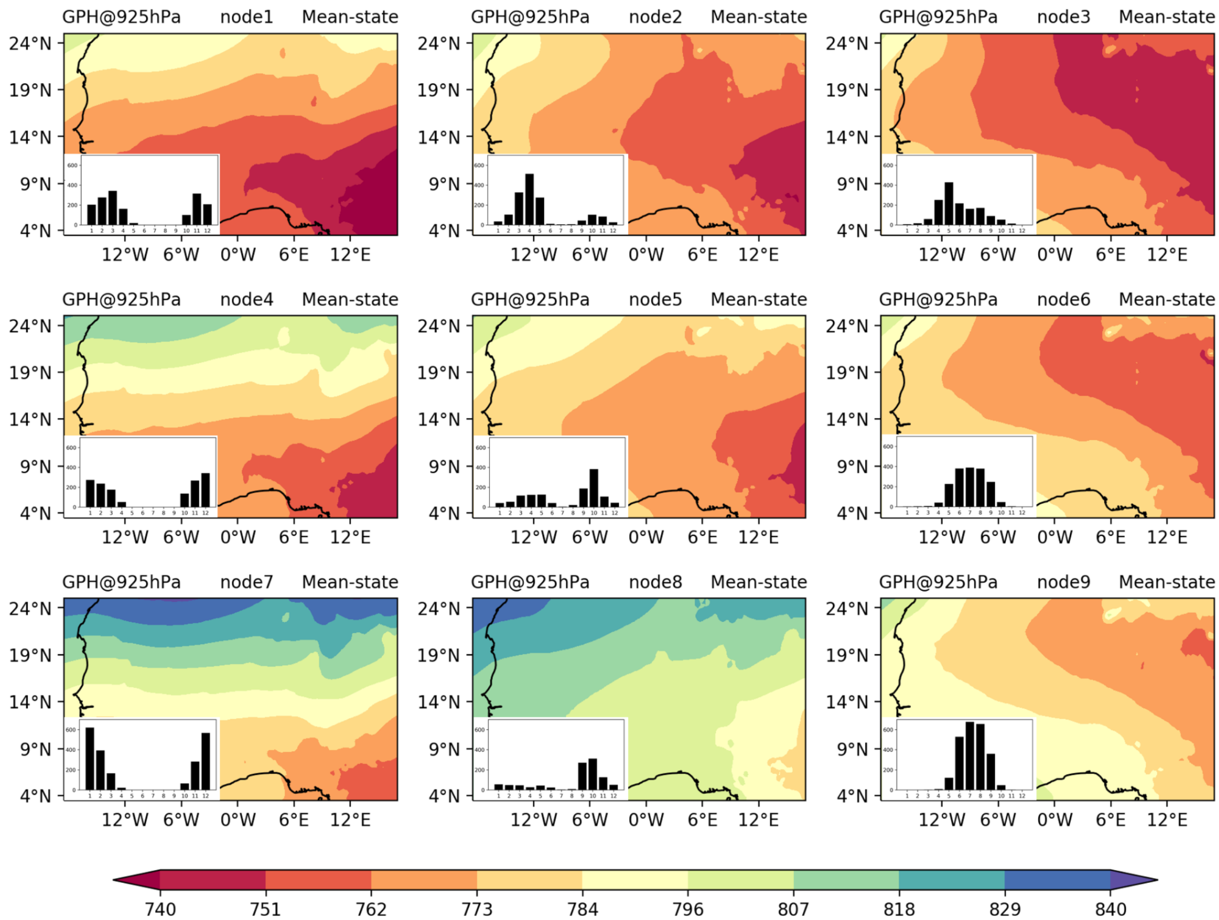


Figure S1. The 3 x 3 SOM using daily ERA5 geopotential height Z at 925 hPa for Western Africa for the period 1981–2020. Insert is the monthly distribution of node cases based on the 3 X 3 SOM analysis

- L167-168: Even if some nodes are ignored, the numbering of the nodes should be as in the full SOM analysis, to be consistent with the complete SOM map.

We made sure the arrangement is consistent with the complete SOM with each node following distribution as in the 9-node case.

- Figure 1: Add the total number of members in each node. Consider normalizing per year and not per month.

This suggestion is respectfully disagreed with, as the purpose of the paper is to develop an understanding of the characteristics of the WAM and its association with MCSs. One of the main characteristics of the WAM is seasonal variability, so normalizing per year will not reveal this variability associated with it. Again, it will be difficult to attain the respective monsoon conditions such as pre-, peak- and post-monsoon when normalized per year.

- Figure 2: Grey grid can be removed to improve visibility. Also, be consistent with X-label intervals. Clarify whether these are daily means or 12 UTC composite.

The grey grid has been removed to improve visibility and the x-label has been made consistent. It has been clarified that they are 12 UTC composite.

- Figure 3: The low correspondence between winds and geopotential heights in the tropical region raises the question: what is the value of classifying by geopotential if it's not indicative of the flow field? Why not directly classify the velocity/ wind-speed fields?

The choice of low-level geopotential height was made in this manuscript because we wanted to consider somehow the influence of the West African Heat Low (WAHL) in influencing instability. At low levels, the geopotential height well describes the strength of the WAHL (Lavaysse et al. 2009; Biasutti et al. 2009). The wind field considered here is the mid-level winds which are consistent with geopotential height in that the mid-level easterly winds follow the northward and southward movement of the heat low.

- L211-218: This subsection is too slim. Either remove it or expand it to get to a conclusion. At the present state, this subsection does not contribute to the main motivation of this study and possibly draws the reader's attention from the main storyline.

This subsection has been removed as suggested.

- Figure 6: This domain should be shown earlier when first presenting the SWA domain.

The domain of SWA has been shown earlier in Figure 2.

- L258: unclear. Why does high humidity lead to cooling?

Thank you for this comment. High humidity does not necessarily lead to cooling. High humidity is just the introduction of more water vapor, which can be in a warmer or cooler atmosphere. The statement on 'enhanced moisture' has therefore been omitted.

- L279: This point was given as a well-known fact in the introduction, so I don't see what is the novelty here.

This point has been removed from this section

- L290: "eastern patterns" – This is not evident in figure 6. This issue should be discussed

Corrected

- L140: "pure node analysis" – What do you mean by this?

This statement has been removed.

Technical corrections

- L25: which=that.

Changed

- L38: variabilities=variability.

Changed

- L42: “Change, 2014” is not a reference, or is missing from the reference list.

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.

- L88: environments= parameters?

Replaced ‘environments’ with ‘environmental parameters’

- L97: Is this a correct use of the word stratify? Seems confusing to me. How about grouped/ separated?

Changed. We replaced ‘stratify for’ with ‘grouped into’

- L108: product= data source.

Replaced ‘product’ with ‘data source’

- L126: daily=daily mean.

Added ‘mean’ to ‘daily’

- L182: SOMs=SOM.

Removed ‘s’ from ‘SOMs’

- L200: “much more strengthened” – Rephrase. Consider "Intensified", "Increased" and so on.

Replaced with “intensified”

- L211: “A further” = Further.

Corrected

- L213: during=within.

Replaced

- Figure 4: The term "moving mean" seems more fitting.

Replaced

- L225: This second subtitle is redundant.

Based on the structuring of the results, the analysis has been grouped under various subtitles, of which the second subtitle well describes the analysis beneath it. We would therefore want to leave the subtitle as such.

- L244: Repetitive.

The statement on L244 has been removed

- Figure 7: The colors appear saturated. Expand the color map beyond 2K to avoid this.

The color map has been expanded for figure 7

- L274: observes= demonstrates/ exhibits.

Replaced observes with exhibits

- L290: observe = show/ depict.

Replaced observe with depict

- L308-311: Long sentence, consider splitting.

Thank you for this observation. The sentence has been split to read as follows: "Node 1 climatological conditions depict both, very low initial shear and TCWV. This illustrates the relatively storm-hostile mean conditions for this node, predominantly representing dry season conditions and explaining the low storm frequency of only 0.13 per day."

- L323: "making node 2…" This sentence is unclear, rephrase.

The sentence has been corrected by removing the last part.

- L330: "This season" – Unclear which season is that.

The sentence has been corrected to capture "this season" as "the monsoon season".