Review V2 for “Classification of Large-Scale Environments that drive the formation of Mesoscale Convective Systems over Southern West Africa” by Nkrumah et al.

Overview

Compared to the initial version, the authors were able to make some improvements on the paper. However, after having received the responses, I do not feel that every question I had was fully answered. Furthermore, the authors should overall consider transferring some more of the explanations they have given in response to my questions to the main text. I therefore recommend minor revision.

General comments/questions on the responses

- The authors argued in their response that the “scope of the work looked at investigating environmental conditions favourable for MCSs over the region”. One aspect of it includes under which conditions MCSs might be triggered in the first place and further develop in southern West Africa. Therefore, it was a bit disappointing to see that there was no attempt to address some of the general comments I had (e.g CIN). For instance, it is known that MCSs can develop in high-CAPE/high-CIN situations where high CIN inhibits a premature initiation of smaller-scale convection that allows CAPE to further build up. Once CIN breaks down or is overcome, e.g. through moisture convergence or convergent motions at elevated terrain, vertical wind shear becomes relevant for the consequent evolution. While this has been observed for the midlatitudes and also partly for the Sahel, MCSs southern West Africa may be initiated differently in a moister environment. I do believe that this aspect is missing in the paper and is not beyond the scope. My suggestion: Have a look into anomalies of CIN and moisture (flux) convergence the same way as CAPE.

Specific comments/questions on the responses

- On the question why 925 hPa specific humidity was replaced by TCWV: *“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 (i.e. 925 hPa humidity) we were looking at an environment that is suitable for instabilities in the atmosphere, of which humidity forms a part.”* I think this needs to be added to the text then since there was no motivation given of why TCWV was suddenly used in Fig. 10 and not elsewhere.

- On the question how the authors determined the rainfall amount: “*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.*”. Why did the authors use the HQ fields of IMERG only? If really variable “HQprecipitation” was used then the authors should have experienced large data gaps since PMW satellites alone cannot fully cover the region at a given timestep. Sure that the variable “precipitationCal” was not used instead?

- On why no MCS are seen in DJF in Fig. 5: “*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.*”. Then Fig. 5 is misleading, and the x-axis should be truncated to the relevant months. Is this also the case for the numbers in Fig. 6? In any case, unless I missed it, this should be mentioned in the data section as well.

Other specific comments/questions

L219: 925 hPa is not exactly surface level.

L235: “The patterns demonstrate northward transport…”. Of what?

L241: “Generally, the presence of zonal wind shear can be seen as a necessary condition…” For what?

Fig.4: How is it possible that the wind shear is negative? Is the directional information included as well, i.e. the direction of the shear vector? Then the authors need to provide more information on the sign of the wind shear.

Fig. 8: Following on the comment on Fig. 4 above, a negative anomaly in wind shear can have a different meaning when wind shear itself can be negative. Node 3 for instance, where the western Sahel exhibit a negative anomaly on climatologically negative wind shear values. What does that mean?