Response to Reviewer #1

In the future, I would like to see new research insights by incorporating a future climate state (e.g., initialize the model using present and future climate and understand how results differ from those derived using IMPALA data), perturbation experiments to better understand orographic, dynamic and thermodynamic processes, and diurnal cycle (e.g., Alber et al https://doi.org/10.1016/j.atmosres.2021.105869). The opportunities for high spatio-temporal process-based research work over Africa is quite limitless.

Re: We thank the reviewer for the very helpful suggestion. In our future work, we will consider conducting the analyses suggested by the reviewer. We have added relevant discussions in the revised manuscript, copied below:

Lines 526-529: "In addition, we will conduct spatial–temporal, process-based research by incorporating future climate states and perturbing MPAS-A to better understand orographic, dynamic, and thermodynamic processes, as well as diurnal cycles (e.g., Alber et al., 2021)."

Minor comments: Line 97-99: Why is this paragraph italicized?

Re: We have removed the italics from this paragraph.

Figure 1a: Can you replace this figure with something better such as a variable resolution MPAS Voronoi mesh version (e.g., https://mpas-dev.github.io/atmosphere/atmosphere.html) .

Re: Since the grid we use has a variable resolution ranging from 3 km to 60 km, the 3 km Voronoi mesh is difficult to represent clearly over the Congo Basin. Therefore, we retained the original figure but added a title and marked the location of the Congo Basin for clarity. The MPAS Voronoi mesh shown on the official website (mpas-dev.github.io) is much coarser. Most studies typically display contours for the high-resolution mesh, similar to what we show (e.g., Fig. 1 in Feng et al., 2021; Fig. 1 in Núñez Ocasio et al., 2024).

Reference:

Feng, Z., Song, F., Sakaguchi, K., & Leung, L. R. (2021). Evaluation of Mesoscale Convective Systems in Climate Simulations: Methodological Development and Results from MPAS-CAM over the United States. Journal of Climate, 34(7), 2611-2633. https://doi.org/10.1175/JCLI-D-20-0136.1

Núñez Ocasio, K. M., Davis, C. A., Moon, Z. L., & Lawton, Q. A. (2024). Moisture dependence of an African easterly wave within the West African monsoon system. Journal of Advances in Modeling Earth Systems, 16, e2023MS004070. https://doi.org/10.1029/2023MS004070

Figure 2: The figure caption for Figure 2 can be re-worded for better clarity.

Re: Revised accordingly, copied below:

Lines 255-260: "Figure 2. (a) Daily mean basin-averaged runoff (kg/m^2/day) from GLDAS for 2023 (bars), along with climatological mean (solid gray line) and the climatological mean ± 1 standard deviation (dashed blue lines) for the period November 1–30. The period analyzed in this study is within the turquoise box. (b–d) Same as (a), but for precipitation (mm/day) from three observational datasets. The climatological period for each dataset corresponds to its available data range, as detailed in Section 2.1."

Response to Reviewer #2

Major points:

All Figures

The figures are generally nicely designed and well described in the captions. However, the labels and units are often missing. While these are mentioned in the captions, I feel that the figures could be substantially improved by also adding labels and units to the axes and colorbars.

Re: We have added labels and units for all figures in the revised manuscript.

Figure 2

It would be great to also have an idea about the variance and not only the mean of the climatological values. You could, for example, do this with shading or other lines for quantiles. I think this would be particularly interesting, as, from my perspective, the enhanced values of runoff and precipitation during the period of interest do not seem very impressive at first glance, considering that this was a 60 year return time event. Having some idea about the variability could probably help making it clearer. Or were there other conditions that could explain this huge flooding despite the not extremely pronounced enhanced precipitation?

Re: We thank the reviewer for the very helpful suggestion. We have added the climatological mean ±1 standard deviation in Figure 2 to illustrate the variability of daily mean runoff and precipitation. We also explain the occurrence of the severe flooding despite the absence of an exceptionally strong increase in precipitation in the revised manuscript, copied below:

Lines 238-250: "Figure 2a shows daily runoff in November 2023 (bars) compared with the climatological daily mean (gray line) and the climatological mean ±1 standard deviation (dashed blue lines). Extreme runoff anomalies, reaching up to 50% above the climatological mean, occurred during November 21–25, with values exceeding one standard deviation between November 23–25. All three rainfall datasets also exhibit consecutive days of positive precipitation anomalies, especially in GPM IMERG and CPC gauge-based data (Fig. 2b–d). GPM IMERG shows three consecutive days above +1 standard deviation. In addition to the precipitation anomalies observed during November 21–25, the preceding dry conditions—particularly the near- or below- –1 standard deviation of precipitation prior to November 15, as indicated by CMORPH CDR—may also contribute to the extreme flooding. The antecedent dry conditions would have reduced soil moisture retention capacity, leading to increased runoff during subsequent heavy rainfall events. Such conditions could enhance flood risks, even when the precipitation event itself is not exceptionally intense (e.g., Barendrecht et al., 2024)."

Figure 3 (c)

How can the violin plot show durations of > 3h for DSL if, by definition, a DSL's duration must be <= 3h?

Re: This was a typographical error. The threshold of <3 hours for DSL was used in Núñez Ocasio et al. (2020b). However, the TAMS package used in this study applies an updated threshold of <6 hours for DSL, following Table 2 in Núñez Ocasio & Moon (2024). We have corrected Table 1 and revised the corresponding discussions in the manuscript accordingly, copied below:

Lines 206-207: "Table 1 shows the MCS classification criteria adopted from Núñez Ocasio & Moon (2024) for use in TAMS,…"

Reference:

Núñez Ocasio, K. M. and Moon, Z. L.: TAMS: a tracking, classifying, and variable-assigning algorithm for mesoscale convective systems in simulated and satellite-derived datasets, Geosci. Model Dev., 17, 6035–6049, https://doi.org/10.5194/gmd-17-6035-2024, 2024.

L279-282

I have difficulties with the interpretation of this sentence and the references. Does this mean that compared to observations MPAS generally has stronger convection and moisture convergence? If so, it is not entirely clear on what this is based on to me. In two cited papers (Raghavendra et al., 2022 and Feng et al., 2023b) MPAS is not used. Is it because the dynamics and physics of WRF are similar to MPAS and thus also the characteristics in preciptation? If yes, this should be made clearer. Furthermore, I don't necessarily see increased precipitation in Rahghavendra et al. (2022) compared to observations – maybe I missed it. I also find the word "overestimation" not ideal, considering that observations often also have some errors (as we can see in the differences in Fig. 2) and, in this case, the CMORPH dataset is used for comparison, which shows the least amount of precipitation of the three datasets for the considered period of time.

Re: We agree that WRF and MPAS share the same physical schemes (Skamarock et al., 2012); therefore, we refer to studies such as Raghavendra et al. (2022) and Feng et al. (2023b). In Figures 3e–g of Raghavendra et al. (2022), there are periods when the Control run exhibits higher precipitation rates than the IMERG observations, with differences reaching up to 6 mm h^{-1}. Similarly, in Figure 10g of Feng et al. (2023b), the mean precipitation feature (PF) rain rate in WRF is substantially higher than in the IMERG observations over the Amazon. In addition, following the reviewer's suggestion, we have removed terms such as "overestimation" and "bias," and acknowledge that both observations and simulations involve inherent uncertainties. We have revised corresponding discussions in the manuscript accordingly, copied below:

Lines 293-297: "Compared to observations, the model exhibits higher rainfall magnitudes for all four categories (Fig. 3d), consistent with previous studies (e.g., Raghavendra et al., 2022; Feng et al., 2023b) comparing rainfall between observations and WRF, which form the basis of MPAS-A's physics schemes (Skamarock et al., 2012). These differences may arise from uncertainties in both the model simulations and the observational data."

Reference:

Feng, Z., Hardin, J., Barnes, H. C., Li, J., Leung, L. R., Varble, A., and Zhang, Z.: PyFLEXTRKR: a flexible feature tracking Python software for convective cloud analysis, Geosci. Model Dev., 16, 2753–2776, https://doi.org/10.5194/gmd-16-2753-2023, 2023.

Raghavendra, A., Xia, G., Zhou, L., & Jiang, Y. (2022). Orographic enhancement of rainfall over the Congo Basin. Atmospheric Science Letters, 23(4), e1079. https://doi.org/10.1002/asl.1079

L407 ff

I don't think such strong and specific conclusions can be drawn based on the analysis ("vertical wind shear explains X% of the total variance"). While I think the results shown in Fig. 10 can quantify the importance of shear to some extent, I don't think that a causality ("explains") with a specific number can be concluded from linear regression results which still only show correlation. The "explains 65%" is also mentioned on line 479. Using the same argument, also the sentence on line 495 and line 32 in the abstract are a bit too strong.

Re: We agree with the reviewer that linear regression analysis cannot fully establish causality. Accordingly, we have replaced the word "explain" and softened the related statement. We have revised corresponding discussions in the manuscript accordingly, copied below:

Lines 32-33: "The shear extends up to 400 km ahead of the convection center, exhibits a strong association with Tb variability,…"

Lines 420-422: "Figure 10a shows that vertical wind shear located approximately 1.5° to 4° ahead of the MCC-south center corresponds to 35%–65% of the total variance in Tb, with the highest values around 2° west of the MCC center."

Lines 496-498: "MCC-south is associated with a more favorable pre-existing shear structure extending up to ~400 km ahead of the system, which exhibits a strong association with Tb variability,…"

Lines 513-514: "Furthermore, the roles of vertical wind shear and the AEJ in MCS developments are investigated."

Minor points:

L67

As a reader, I'd find it nice to also have the other two core regions of convection mentioned here. While this might be common knowledge for researchers focusing in that area, others might be curious.

Re: Revised accordingly, copied below:

Lines 68-70: "It houses one of the three core regions of convection in the tropics (the other two locate over the Maritime Continent and the Amazon Basin) and the world's second-largest rainforest (Washington et al., 2013)."

L199

"< 235 K regions that contain embedded < 219 K" regions

Re: Revised accordingly.

Figure 3 (a)

I would prefer for less elevation categories or have them more dense (i.e. 300m bins) with a maximum extension. Now, there are almost no grid points > 2000m and the ones that are are so close to each other, that it is difficult to see which category they belong to. Going not all the way to 3200m with the scale would likely improve the plot (sharper gradients for lower levels) without loosing too much information about the upper levels.

Re: Revised accordingly.

Table 1

I would still list the "< 219K region has area of >= 4000 km^2" in the table (there's enough space). While this is written in the text, it would improve the value of the table on its own. Also, the shape criteria for MCC is listed but never mentioned in the text. Maybe add a sentence regarding this to the text to help the reader understand without having to check in the referenced literature.

Re: We have updated Table 1 and added discussion about the shape criteria, copied below:

Lines 267-269: "Following the strictest criteria in Table 1, MCCs with an elliptical shape ($\varepsilon = \sqrt{1-(b^2/a^2)} \leq 0.7$) generally initiate in the eastern portion of the basin and propagate the longest distance…"

L361

I would write the whole word in the title ("African Easterly Jet") for better readability.

Re: Revised accordingly.