Referee #2

Dear Reviewer,

Thank you for your careful review of our manuscript. Your comments are greatly appreciated and we think this new version of the manuscript responds to your concerns and provides an interesting contribution to the study of forecast extreme September-to-November rainfall events. Below, each comment was addressed separately in a concise manner, with the Referee's comment in *italic*, and Authors comment in **bold**. Proposed changes and additions are highlighted in red in the updated manuscript and underlined here.

SOME GENERAL COMMENTS

Referee:

1. *The motivation of the study is not made sufficiently clear. Thus, the authors need to highlight the novelty of the study in comparison with recent ones, in particular with the study by Tefera et al. (2025) which already characterises the relationship between rainfall in OND and ENSO-IOD in observations and in C3S seasonal models and also examines some extreme years. Therefore, the authors need to clarify what gap the present study intends to fill. Is it that the previous studies did not specifically use version 5.1 of SEAS5? Is it that large-scale drivers were not addressed before for the model?*

Authors: We thank the reviewer for drawing our attention to this point. According to the following reviewer's comment, we have rewritten the introduction section and better highlighted the motivation behind the study by showing its unique nature. Please see the "Introduction" section in the revised manuscript.

"Equatorial Africa (EA) exhibits a complex annual rainfall cycle shaped by the seasonal migration of the Intertropical Convergence Zone (ITCZ), local convection, and moisture transport from the Atlantic and Indian Oceans. Among the different seasons, September to November (SON) is particularly important, as it marks one of the peak rainfall periods for many EA countries and is frequently associated with severe hydrometeorological hazards such as floods and landslides (Moihamette et al., 2024; Gudoshava et al., 2022a; Kenfack et al., 2025; Nana et al., 2025). Understanding and predicting SON rainfall variability is therefore critical for risk preparedness and climate-sensitive planning across the region. The SON rainfall system in EA is influenced by a combination of local, regional, and large-scale drivers. Local factors include mesoscale convective systems and interactions between topography and atmospheric flow (Pokam et al., 2013). Regional circulation patterns, particularly over the eastern equatorial Atlantic and western Indian Ocean, further modulate moisture availability (Kuete et al., 2019; Longandjo and Rouault, 2020). At larger scales, Sea Surface Temperature (SST) variability in the Pacific, Indian, and Atlantic oceans plays a central role in shaping interannual rainfall anomalies (Pokam et al., 2014; Nicholson, 2015). In particular, El Niño Southern Oscillation (ENSO), the Indian Ocean Dipole (IOD), and Atlantic SST anomalies have been shown to influence SON precipitation extremes across EA (Preethi et al., 2015; Roy et al., 2024; Palmer et al., 2023; Nana et al., 2025). Years characterised by the co-occurrence of a positive IOD and strong El Niño such as 1997 and 2023 have produced widespread heavy rainfall over several EA regions (Okoola et al., 2008; Nana et al., 2025). These links underscore the importance of accurately capturing SST-driven teleconnections and associated atmospheric circulation patterns when forecasting SON rainfall.

Despite advances in global numerical weather prediction systems, forecasting SON precipitation over EA remains a persistent challenge. Sparse observational networks, limited understanding of regional climate dynamics, and model-specific errors contribute to substantial uncertainties in seasonal forecasts (Tanessong et al., 2017). While several studies have evaluated the skill of general circulation models over EA (e.g., Feudjio et al., 2022; Nana et al., 2024; Tanessong et al., 2024), important gaps remain particularly regarding the model's ability to reproduce SON extreme rainfall events and their associated large-scale drivers. Most existing evaluations focus on earlier SEAS5 versions or on mechanisms relevant to other seasons (e.g., MAM or JJAS), thus providing an incomplete picture of SON dynamics. These studies found that EA rainfall variability is mainly associated with several factors, including easterly and westerly waves, tropical cyclones, the Madden-Julian Oscillation (MJO) and sea surface temperature (SST) in the Atlantic, Indian and Pacific oceans. For example, Nana et al. (2024) demonstrated that the ability of seasonal forecast models to predict rainfall anomalies occurring over western EA during extreme South Atlantic Ocean Dipole (SAOD) events depends on their skill in forecasting the relationship between rainfall and SAOD, which decreases with increasing lead time. Their results showed that the ECMWF seasonal forecast system 5 (SEAS5) model best captures this relationship and the associated rainfall anomalies, a finding also supported by Gebrechorkos et al. (2022). Similarly, Mwangi et al. (2014) evaluated SEAS5 products against data from ten East African stations and found significant forecasting skill for both rainy seasons, with better performance in October–December (OND) compared to March–May (MAM). The ability of the SEAS5 model to simulate the drivers of extreme rainfall during MAM 2018–2020 over eastern EA has been analyzed by Gudoshava et al. (2024). The findings of this study indicate that the heavy rainfall events of March–May 2018 and 2020 coincided with an active MJO (Phases 1–4) or a tropical cyclone east of Madagascar. In contrast, the low rainfall observed during the same period in 2019 was linked to tropical cyclones west of Madagascar. Their study also concluded that underestimation of these extreme rainfall intensities was linked to inaccurate MJO forecasts and errors in tropical cyclone location and intensity. For the June–September (JJAS) season, the findings of Ehsan et al. (2022) establish that the spatial and temporal patterns of observed EA rainfall variability, as well as the key climatic features that drive EA precipitation excesses and deficits, are successfully captured by the SEAS5 model, when initialized in May and April. Recent analyses have begun to examine the role of large-scale climate modes in shaping extreme SON rainfall, but few studies have assessed how well seasonal forecast systems capture both the rainfall anomalies and the underlying physical mechanisms. For example, Tefera et al. (2025) showed that SEAS5 is able to capture hydroclimatic extremes linked to coupled IOD-ENSO modes during the first two lead times, but their assessment did not consider the most recent ECMWF system nor did it explicitly evaluate the associated atmospheric circulation patterns during SON. This gap limits our understanding of the forecast system's ability to represent the processes driving extreme rainfall variability during this crucial season.

Motivated by these limitations, the present study evaluates the performance of the latest ECMWF seasonal forecasting system, SEAS5.1 (Johnson et al., 2019), in simulating SON extreme rainfall events over EA using forecasts initialized in August and September. SEAS5.1 was selected due to its demonstrated skill in representing key global climate teleconnections such as ENSO and the IOD (Nana et al., 2024; Tefera et al., 2025), which exert strong influence on SON precipitation. In addition to providing an updated assessment of model skill, our study explicitly examines the large-scale physical mechanisms SST anomalies, moisture transport, zonal and Walker circulations that accompany extreme rainfall events. This dual approach offers a more comprehensive and physically grounded evaluation than previous studies, thereby contributing toward improved understanding and prediction of SON rainfall extremes in EA. Extreme rainfall events are among the most impactful climate hazards over EA, often leading to severe flooding, infrastructure damage, and socio-economic losses, yet their predictability at seasonal timescales remains limited. Understanding whether a state-of-the-art seasonal forecast system can realistically represent the large-scale drivers of these extremes is therefore essential. The remainder of the paper is structured as follows. Section 2 describes the SEAS5.1 model, the observational and reanalysis datasets, and the methodology. Section 3 presents the skill assessment of SEAS5.1. Section 4 focuses on rainfall composites and associated SST patterns during extreme SON years, and Section 5 analyzes the corresponding atmospheric circulation features. Section 6 concludes the study.**"**

Referee:

2. *The current structure of the Introduction makes it somewhat difficult to follow, and a more streamlined presentation would improve readability. I suggest the following:*

- *Start with a very brief description of the seasonal cycle of rainfall in EA and what drives this seasonal variability. This provides useful context for readers who may be unfamiliar with the region and justifies the focus on the SON season.*
- *Keep the description of mechanisms limited to SON, which is the season examined in this study. Although the current introduction summarises a wide range of relevant literature, reviewing mechanisms across all seasons may distract from the primary objective.*
- *In line with my previous point 1, emphasise the gaps that remain in the existing literature and explain how the objectives of this study will help address them. It is fundamental to make very clear why this study is necessary, and this is not clear in the current version of the manuscript.*

Authors: We thank the reviewer for drawing our attention to this point. Please see the "Introduction" section in the previous comment.

Referee:

3. *This study uses seasonal forecast data at lead month 0 (lead-0). I am unsure that this is standard practice in the analysis of large scale drivers or teleconnections. At lead-0, forecast skill will exhibit a substantial influence from atmospheric initial conditions and short-range predictability, while for lead-1, lead-2... the role of the ocean as a predictor becomes more important. For instance, Fig. 2 shows that lead-0 forecasts exhibit higher ACC than lead-1.*

However, it is unclear whether this increase in skill is due to better representation of teleconnections and large-scale drivers at lead 0, or whether it primarily reflects the influence of atmospheric initial conditions. Hence some results should be interpreted with caution. Moreover, SEAS5 forecasts initialised in September are not available at Copernicus CDS until 6 September (10 September for the rest of the seasonal models available at CDS), i.e. when part of the month has already passed, thereby limiting practical applications for climate services. For these reasons, I think that lead-0 seasonal forecasts are probably not the most suitable choice for the purposes of this study, unless the authors provide convincing arguments for their use.

<u>Authors:</u> We thank the reviewer for this important comment. In this study, *Lead-0* refers to forecasts initialized in September. This means that the forecasts for October and November correspond to *Lead-1* and *Lead-2*, respectively. Similarly, the August initial conditions (referred to as *Lead-1* in this study) indicate that forecasts were initialized in August; therefore, the forecasts for September, October, and November correspond to *Lead-1*, *Lead-2*, and *Lead-3*, respectively. With this definition, the initial conditions have a relatively limited influence on the model outputs across the different analyses, especially when compared to the dominant predictive role of oceanic conditions. Regarding the official release date of the forecasts (the 6th of each month), we agree with the reviewer. However, our focus is on the hindcast initial conditions, which are set on the 1st day of each month in the SEAS5.1 system. Therefore, our analysis is based strictly on these hindcast initialisation dates.

<u>Referee</u>:

4. *Section 4, on the large-scale drives, currently reads as if it were a standalone study. Integrating it a bit with the preceding discussion would improve the cohesiveness of the manuscript. How do the differences between model and observations on the physical mechanisms relate to the forecast skill and the ability to reproduce observed precipitation patterns in SW and WY?*

<u>Authors:</u> We thank the reviewer for this important comment. The changes have been made in the manuscript.

"Previous studies highlighted the fact that spatial pattern of extreme rainfall over EA is strongly influenced by SST anomalies in the surrounding ocean basins (Palmer et al., 2023; Roy et al., 2024; Nana et al., 2023,2025). Examining the associated SST composites therefore provides essential insight into the drivers of these rainfall extremes, and highlights the importance of accurately representing oceanic conditions in seasonal prediction models (Nana et al., 2024)."

<u>Referee</u>:

5. *Regarding the statistical significance of the results: the authors should be aware of the correction of p-values due to multiple testing. Each time an hypothesis test is carried out, there is a small albeit non-negligible probability of erroneously rejecting the null hypothesis. If just one test is carried out, this is not an issue. However, an enormous amount of tests are carried out when evaluating significance over a latitude-longitude grid, and consequently a number of erroneous rejections will arise and statistical significance is often overstated.*

Please see Wilks (2016) for a description of the problem and how to take into account test multiplicity.
The authors should either:

- *Take into account the multiple testing problem and correct the p-values in order to limit the false discovery rate.*
- *Keep the evaluation of statistical significance as-is in the current manuscript, but acknowledge in the Methods section that the correction of p-values due to multiple testing was not addressed. Figure captions should be adjusted as well, e.g. "the stippling occurs where X is locally significant at the 95% confidence level through a Student's t test" (i.e. emphasise that significance was evaluated just locally). The discussion should accordingly reduce the emphasis placed on significant results.*

Wilks (2016): https://doi.org/10.1175/BAMS-D-15-00267.1

Authors: We thank the reviewer for this important comment. The changes have been made in the manuscript. Please see the last sentences of section 2.2 and figure captions.

"A 5% significance level was applied throughout, with results considered locally statistically significant if $p < 0.05$. It is important to note that the correction of p-values due to multiple testing was not addressed, in accordance with Wilks, (2016).**"**

Specific comments

Referee:

1. Regarding Figure 1:

- *It is confusing to use different plot types for model lead-0 and lead-1 in Fig. 1a. Please use the same plot type for model data in order to allow a clearer model-observations comparisson.*
- *In Fig. 1a, the difference with model lead-1 is striking. In fact, I retrieved the SEAS5 data from CDS and tried to reproduce the same plot and found no lag between model lead-1 and model lead-0. There might have been an issue when selecting the lead time or plotting lead-1 data. Please check this.*
- *The authors indicate in the Methodology section that SEAS5 data from the August and September initialisations are used. However, in Fig. 1, model data throughout the year are represented. I suppose that more initialisations apart from August and September were used, but this is not specified in the manuscript. Finally, in Fig. 1c and 1d, how is the total annual precipitation computed? Is it from model or observations? Please clarify in the text.*

Authors:

- **We thank the reviewer for this comment. The suggestion has been taken into account, and the figure has been revised accordingly.**
- **The data were re-downloaded, the codes were carefully re-checked, and all calculations were repeated; however, the same results were obtained. These results are consistent with previous studies showing that the ECMWF model tends to overestimate JJA rainfall over Central Africa at Lead-0, as well as JJAS and OND rainfall over East Africa at Lead-0 and Lead-1. It should be noted that, as described in Sect. 2.1 of the manuscript, the**

analysis combines the first 25 ensemble members for the period 2017–2023 (and not 51 members) with the 25 members from the 1981–2016 period. Furthermore, as stated above, Lead-0 corresponds to September initialisation (i.e., Lead-0 for September, Lead-1 for October, and Lead-2 for November), whereas Lead-1 corresponds to August initialisation (i.e., Lead-1 for September, Lead-2 for October, and Lead-3 for November). This distinction explains and confirms the differences between the results obtained at Lead-0 and those at Lead-1.

- **We thank the reviewer for highlighting this point, which was insufficiently explained in the original manuscript. Here, the term *initialisation* refers specifically to the SON seasonal forecasts. For monthly data, lead times correspond to those indicated on the data download platform. For example, for January 1981, the initialisations correspond to January 1981 and December 1980 for Lead-0 and Lead-1, respectively; for February 1981, they correspond to February 1981 and January 1981; and for December 2024, to December 2024 and November 2024 for Lead-0 and Lead-1, respectively. The method used to extract lead times is consistent with that adopted by Ehsan et al. (2021). However, for SON, Lead-0 (Lead-1) corresponds to a September (August) initialisation, as explained above. The manuscript has been revised accordingly (see Sects. 2.1 and 3.1). In addition, total annual rainfall was computed from observations in Fig. 1b and from the model at Lead-0 (Lead-1) in Fig. 1c (Fig. 1d). To improve clarity and readability, the text has been revised to provide a more detailed description of the methodology used to extract the different variables at each lead time.**

“This means that the forecasts initialized in September correspond to Lead-1 and Lead-2 for October and November, respectively. Similarly, the August initial conditions indicate that forecasts were initialized in August; therefore, the forecasts for September, October, and November correspond to Lead-1, Lead-2, and Lead-3, respectively. The method used to extract lead times is consistent with that adopted by Ehsan et al. (2021). With this definition, the initial conditions have a relatively limited influence on the model outputs across the different analyses, especially when compared to the dominant predictive role of oceanic conditions.**”**

Ehsan, M. A., Tippett, M. K., Robertson, A. W., Almazroui, M., Ismail, M., Dinku, T., Acharya, N., Siebert, A., Ahmed, J. S., & Teshome, A. (2021). Seasonal predictability of Ethiopian Kiremt rainfall and forecast skill of ECMWF’s SEAS5 model. *Climate Dynamics*, *57*(11–12), 3075–3091. https://doi.org/10.1007/s00382-021-05855-0

Referee:

2. In the discussion of Fig. 6, the authors state “The SEAS5.1 captures these relationships reasonably well at both L0 and L1, but overestimated the correlations, ...”. I do not agree with this statement since it cannot be derived from the data shown in Fig. 6. When the ensemble mean is computed, part of the high-frequency internal variability is filtered out *and the part of the signal that remains is mainly associated with the lower frequency forcing and boundary conditions, in this case mainly from oceanic sources of predictability (IOD and ENSO). Conversely, this filtering is not present in the observations, and thus care should be taken when discussing the differences between model and observation. Hence, the fact that correlations with SST indices are higher in SEAS5 compared to observations may well be an artefact arising from using ensemble mean data. In order to assess whether there is a true overestimation of the correlation in SEAS5 related to some model deficiency or bias, the authors could compute correlations for the individual ensemble members. Comparing the observed correlation value with the distribution of correlation values from the individual ensemble members provides a robust framework to assess whether there is a systematic overestimation in the model.*

<u>Authors:</u> We thank the reviewer for drawing our attention to this point. We agree with the reviewer regarding the influence of ensemble averaging on the distribution of values. In this study, all analyses were first performed separately for each of the 25 ensemble members before deriving the ensemble mean, as described in the Methods section. Specifically, the ensemble mean was computed only after applying all diagnostics including correlation and regression analyses, rainfall indices, composite anomalies, moisture flux, and moisture flux divergence to each individual ensemble member, following the approach of Abid et al. (2023). The results presented in this study therefore address the reviewer's concern. For clarity, additional explanations have been included in the revised manuscript; please refer to the Methods section.

"All analyses were performed separately for each of the 25 ensemble members. The ensemble mean was then computed from the 25 members after applying all diagnostics to each individual member, including correlation and regression analyses, rainfall indices, composite anomalies, moisture flux, and moisture flux divergence, following the methodology of Abid et al. (2023).**"**

Abid, M. A., Kucharski, F., Molteni, F., & Almazroui, M. (2022). *Predictability of Indian Ocean Precipitation and its North Atlantic teleconnections during early Winter*. Springer Science and Business Media LLC. https://doi.org/10.21203/rs.3.rs-1730304/v1

<u>Referee</u>:

3. In the Methods section, it is explained that ERA5 data are used for the evaluation of the physical mechanisms. However, ERA5 precipitation data are represented in Fig. 8 and Fig. 9 and there is no mention of or discussion about ERA5 precipitation in these figures. Could you indicate what is the purpose of using ERA5 precipitation? If it is for validation with the CHIRPS database, there should be at least some sentence about it in the discussion.

<u>Authors:</u> We thank the reviewer for drawing our attention to this point. Indeed, ERA5 reanalysis precipitation was included in these figures in order to validate ERA5 against the CHIRPS reference dataset. The text has been revised accordingly to explicitly state this.

"The precipitation from the ERA5 reanalysis has been included in these figures in order to validate ERA5 with the CHIRPS reference.**"**

<u>Referee</u>:

4. In case that this study differs from the previous literature in that it uses version 5.1 of SEAS5, I think it would be convenient to briefly explain the main differences between version 5.1 and the previous version when the model is presented in the Data and Methods section.
<u>Authors:</u> We thank the reviewer for drawing our attention to this point. This has been added to the revised manuscript, in the Data and Methods section.
"From November 2022 onwards, the updated version SEAS5.1 is used, which differs from the original SEAS5 mainly by the adoption of a new interpolation tool and a revised 1° grid with half-degree–centered latitude/longitude points, ensuring consistency with other Copernicus Climate Change Service seasonal forecast systems. SEAS5.1 also provides an extended set of variables, including top solar incoming radiation, additional fields at the 1000 hPa pressure level, and separate surface and sub-surface runoff components. The underlying model physics remains unchanged between the two versions.**"**

<u>Referee</u>:
5. The analysis of composites of extreme events begins rather abruptly, moving immediately into the discussion of Figs. 7 and 8. Instead, it would be convenient to add a paragraph that serves as a link between the preceding discussion and the subsequent analysis of extreme events. This paragraph would be also useful to emphasise the motivation for the study of extreme rainfall events, which is not stated in the current manuscript. What are the main objectives of the analysis of extreme events?
<u>Authors:</u> We thank the reviewer for drawing our attention to this point. An explanatory paragraph has been added to the revised manuscript, both in the paragraph preceding Section 4 and in the final paragraph of the Introduction.
"Following the assessment of SEAS5.1 in simulating rainfall characteristics and their associated teleconnections with SST, the analysis is extended to a composite-based approach. This complementary framework allows a more detailed examination of the large-scale atmospheric and oceanic patterns associated with extreme rainfall events over EA. In particular, composites of precipitation, SST, and low-level wind fields are used to characterize the dominant circulation features and moisture transport pathways linked to these extremes. This approach provides additional physical insight into the mechanisms driving extreme rainfall beyond the skill-based evaluation of the model.

Extreme rainfall events are among the most impactful climate hazards over EA, often leading to severe flooding, infrastructure damage, and socio-economic losses, yet their predictability at seasonal timescales remains limited. Understanding whether a state-of-the-art seasonal forecast system can realistically represent the large-scale drivers of these extremes is therefore essential.**"**

<u>Referee</u>:
6. Figs. 6 and 8 seem to suggest an asymmetry in the teleconnections to EA rainfall. For instance, in Fig. 6 it appears that if only SST < 0 values are considered, the correlations with ENSO are not significant, while for SST > 0 the positive correlations become more apparent. Fig. 8 appears to confirm this, in the sense that the weak rainfall years composite is not the exact opposite pattern to the strong one. In fact, SST anomalies over the ENSO region and

Indian Ocean are weak and generally not statistically significant in the weak rainfall years composite. However, it is not until the discussion of Fig. 11 that these differences in the magnitude of the anomalies are mentioned. I think the apparent asymmetry should be discussed earlier.

Authors: We thank the reviewer for drawing our attention to this point. Sentences addressing the asymmetry have been added to discuss the precipitation and SST anomaly results. Please refer to the last paragraph of Section 4 in the revised manuscript.

"The observed SST anomalies, as well as rainfall anomalies (Fig. 8) stronger during SY than during WY, are well simulated by the model at these two Lead-time.**"**

Referee:

7. Lines 545-547: I do not agree that the patterns are "strong opposite" looking at Fig. 10. This is in fact the lack of symmetry in the teleconnection I was mentioning in my previous comment.

Authors: We thank the reviewer for drawing our attention to this point. We agree with the reviewer regarding the asymmetry observed in Figs. 8 and 10, as mentioned previously. The text has been revised and adjusted accordingly; please see the first sentence of Section 5.

"Previously, observed and reanalysis, as well as predicted composite SST anomalies over the Atlantic, Indian, and Pacific oceans showed a strong and significant composite anomalies pattern during both strong and weak years (but more pronounced during SY than WY), which shows that EA rainfall has diverse dynamical linkages from these oceanic regions.**"**

Referee:

8. The results from the study by Tefera et al. (2025) can also be cited in lines 412 and 436.

Authors: We thank the reviewer for drawing our attention to this point. The study by Tefera et al. (2025) has been added to the manuscript.

Referee:

9. Line 118. I could not find in the references the study by Tanessong et al. (2025). Do you mean Tanessong et al. (2024)? The SEAS5 model is not used there.

Authors: We thank the reviewer for drawing our attention to this point. The study by Tanessong et al. (2025) is not included because it is still under review; we had initially expected it to be published earlier. The Introduction has therefore been revised, and this reference has been removed.

Referee:

10. I suggest that Fig. 9 is moved to Supplementary material, as its discussion is very short and it serves as a confirmation of the previous findings.

Authors: We thank the reviewer for drawing our attention to this point. Figure 9 has been moved to the Supplementary Material, as this result is not central to the main conclusions of our study.

Referee:

11. Line 204: Could you indicate what interpolation technique was used?

Authors: We thank the reviewer for drawing our attention to this point. To bring all data to the model grid, linear interpolation was applied. The text has been revised accordingly; please see the last sentence of Section 2.1.
"For consistency in comparison, both observed and reanalysis datasets are regridded to a 1° × 1° horizontal resolution based on linear interpolation and to seven pressure levels (1000, 925, 850, 700, 500, 400, and 300 hPa)."

Referee:
12. Although it is stated later on in the manuscript, please indicate in the Methods section that the N34 and DMI indices used are standardised indices.
Authors: The Niño-3.4 and DMI indices have already been defined in the Methods section, along with a description of how they were calculated.
"This analysis uses two SST indices: the Niño 3.4 index (N34) and the Dipole Mode Index (DMI). The N34 index, used as a proxy for the ENSO, is defined as the area-averaged SST anomaly over the region 5° S–5° N, 170°–120° W (Trenberth, 1997). The DMI (Saji et al., 1999), which represents the IOD, is calculated as the difference between the area-averaged SST anomalies in the western Indian Ocean (WIO; 10° S–10° N, 50°–70° E) and the eastern Indian Ocean (EIO; 10°S-0° N, 90°–110° E)."

Referee:
13. Line 593: I think that you mean the western part of EA, not eastern.
Authors: We thank the reviewer for drawing our attention to this point. This refers to the western part of EA, not the eastern part of EA.

Referee:
14. Line 895: This reference follows a different format compared to the rest. Please ensure consistency.
Authors: We thank the reviewer for drawing our attention to this point. We have inserted the updated version; please refer to the "References" section of the revised manuscript.
"Nana, H. N., Gudoshava, M., Tanessong, R. S., Tamoffo, A. T., and Vondou, D. A.: Diverse causes of extreme rainfall in November 2023 over Equatorial Africa, Weather Clim. Dynam., 6, 741–756, https://doi.org/10.5194/wcd-6-741-2025, 2025."

Referee:
15. The subpanels of Fig. 6 (a, b and c) are not labeled.
Authors: We thank the reviewer for drawing our attention to this point. The figure has been redrawn and the labels (a, b, and c) have been added.

Referee:
16. Colourbar units are missing in Figs. 4 and 5.
Authors: We thank the reviewer for drawing our attention to this point. The figures have been redrawn, and the units have been added.
"**mm day^{-1} °C^{-1}**"

Minor language, formatting and/or consistency corrections
Referee:

1. Line 104: Replace "equatorial Africa" with EA (abbreviation already defined). Check throughout the manuscript.
Authors: **We thank the reviewer for drawing our attention to this point. The manuscript has been carefully reviewed again, and the suggested modifications have been implemented.**

Referee:
2. The current manuscript mixes British spelling (e.g. "organised" in Line 164, "standardised" in Lines 468 and 479) and American spelling (e.g. "normalizing" in Line 270, "characterized" in Line 629). Please check throughout the manuscript and ensure consistency.
Authors: **We thank the reviewer for drawing our attention to this point. The manuscript has been thoroughly reviewed again, and the necessary modifications have been made.**

Referee:
3. Line 328: "...strength of SEAS5.1 to simulated SON rainfall..." does not sound correct. Please clarify this sentence.
Authors: **We thank the reviewer for drawing our attention to this point. The sentence has been revised.**

Referee:
4. Lines 371-372: "... internal variance is dominated by the external variance". This statement may be misleading, as if the external variance was part of the internal variance. I suggest replacing it with something like "... external variance outweighs the internal variance...".
Authors: **We thank the reviewer for drawing our attention to this point. Done**

Referee:
5. Line 455: "... ENSO has an indirect effect through IOD conditions, ..." (add "effect").
Authors: **We thank the reviewer for drawing our attention to this point. "effect" has been added.**

Referee:
6. Line 469: Typo: "periode" is "period". Please check throughout the manuscript as this typo appeared several times.
Authors: **We thank the reviewer for drawing our attention to this point. The manuscript has been thoroughly reviewed again, and the necessary modifications have been made.**

Referee:
7. Lines 472-473: The sentence starting with "The criteria..." is grammatically incorrect. Please revise it.
Authors: **We thank the reviewer for drawing our attention to this point. The sentence has been revised.**

Referee:
8. Line 484: please correct "years capture" to "years captured" (add the d).

__Authors:__ **We thank the reviewer for drawing our attention to this point, "d" has been added.**

__**Referee**__:
9. Line 489: replace "weak column" with "second column".
__Authors:__ **We thank the reviewer for drawing our attention to this point. This was an error, and the suggested correction has been applied.**

__**Referee**__:
10. Line 546: a comma is missing between "Atlantic" and "Indian".
__Authors:__ **We thank the reviewer for drawing our attention to this point. Done**

__**Referee**__:
11. Line 584: I suggest replacing "positive and negative" with "strong and weak".
__Authors:__ **We thank the reviewer for drawing our attention to this point. Done**

__**Referee**__:
12. Line 590: replace "underestimate" with "underestimation"
__Authors:__ **We thank the reviewer for drawing our attention to this point. Done**

__**Referee**__:
13. Line 620: this phrase reads better if re-structured "... over which the AEJ components (black dashed contours) at 15° E, and specific humidity (red contours) calculated between 10°E and 30°, are overlaid".
__Authors:__ **We thank the reviewer for drawing our attention to this point. Done**