Reviewer Comments

General Comments

This manuscript addresses an important relevant topic by investigating extreme precipitation over southern Africa using high-resolution (~16 km) regional climate model (CCLM) simulations. The study has potential value for publication in the journal *Weather and Climate Dynamics*; however, the manuscript in its current form requires substantial revision.

The main concerns relate to the robustness and interpretation of the extreme precipitation analyses, the physical realism of some simulated extremes, the simplified synoptic classification methodology, and the overall structure and clarity of the manuscript. Several methodological choices and interpretations require stronger justification and supporting references, while some sections and figures would benefit from substantial tightening and clearer presentation, respectively.

Specific Comments

Major comments

Comment 1

Lines 12–13: Please explicitly state the latitudinal and longitudinal extent of the Summer and Winter Rainfall Zones in the Abstract. In addition, Figure 1 should clearly delineate all regional boundaries and key locations described in the manuscript (e.g., SRZ, WRZ, coastal strip, Cape Town region, Good Hope Line, Cape Basin, Great Lakes, Drakensberg). This would substantially improve readability and spatial interpretation throughout the manuscript.

Comment 2

The manuscript highlights the important role of synoptic disturbances in driving intense precipitation; however, the Introduction lacks sufficient discussion of the relevant synoptic-scale systems. For instance, studies along the Australian east coast have shown that extreme precipitation events are often linked to East Coast Lows forming over the East Australian Current system and nearby regions. These systems can include embedded thunderstorms activity with spatial scales smaller than the ~16 km resolution used here (see *https://doi.org/10.1007/s00703-015-0382-4*). In this context, it would be useful to clarify whether mesoscale convective systems play a role in the study region, and if so, to provide some justification for the chosen 16 × 16 km resolution, especially given that convection is likely parameterised.

Comment 3

Please justify the choice of JRA-55 as the driving reanalysis for the CCLM hindcast, particularly given the widespread use and higher spatial resolution of ERA5, which is also used later in the manuscript for the KwaZulu-Natal event analysis.

The manuscript also states that the driving global model FOCI has a horizontal resolution of ~0.1° around Southern Africa (Line 137), which appears comparable to or potentially higher than the regional model CCLM (~16 km). If so, the added value of the regional downscaling should be clearly discussed and justified.

Comment 4

The analysis of extremes during El Niño and La Niña years is interesting; however, the motivation for focusing specifically on El Niño–Southern Oscillation is not clearly introduced. The Introduction should provide background on the role of large-scale climate modes in regional rainfall variability and extremes. In particular, the potential influence of the Southern Annular Mode on southern African rainfall and storm tracks should at least be acknowledged and discussed.

Comment 5

The reported daily precipitation maxima (e.g., 2225 mm on 24 February 2001; Line 242) appear exceptionally high and require further validation. It is unclear whether these values are physically realistic, particularly given the model resolution and likely use of parameterised convection. I strongly recommend evaluating these extremes against observations (e.g., CHIRPS or station data) and/or relevant literature to assess their plausibility and potential model biases.

The future analysis also appears to use the full simulation period (2014–2099) rather than fixed climatological windows. Using consistent fixed-length periods (e.g., 30-year windows) would provide a more robust comparison between historical and future climates.

Figure 6 would benefit from either showing climatological annual maxima or using a non-linear colour scale, and the statistical significance of trends in Figure 7 should be explicitly indicated.

Comment 6

Figure 8: The historical and future simulations appear broadly similar, with only limited local differences. I strongly recommend showing difference fields (future minus historical) in addition to the absolute fields to more clearly highlight climate change signals. A similar approach should also be considered for other historical-future comparisons throughout the manuscript.

Comment 7

The colour scale in Figure 9 is dominated by a very small number of extreme grid points and does not adequately represent the broader spatial variability. A non-linear colour scale is strongly recommended. Also, the isolated daily precipitation maxima (>200 mm) appear inconsistent with the broader distribution, where the 99th percentile remains below ~40–45 mm. This again raises concerns regarding potential model artefacts or overestimation associated with the model resolution and parameterised convection. These extremes should be carefully verified and discussed.

Comment 8

Lines 293–299: The attribution of precipitation extremes to tropical cyclones and cut-off lows appears overly simplified and insufficiently justified. The classification relies solely on the location of minimum sea-level pressure, without adequate methodological detail or supporting references. This approach may misclassify dynamically distinct systems (e.g., tropical lows, hybrid systems, extratropical cyclones, cold fronts, or mesoscale convective systems), thereby limiting the physical interpretability of the results. In addition, the absence of supporting diagnostics or visualisation makes the validity of these attributions difficult to assess. A clearer and better-referenced methodology, including supporting diagnostics and discussion of limitations, is required.

Comment 9

Section 5.6: If I understood it correctly – this section mixes a single observed event (08–16th of April 2022), ERA5 climatological extremes, and model-simulated historical and future extremes in a way

that is not fully physically or statistically comparable. As a result, several interpretations appear overstated and require stronger justification.

In particular, the claimed “good agreement” (Line 344) between ERA5 and the simulations is not convincingly demonstrated, as it compares a single event with multi-decadal maxima rather than event-based simulation and further validation. Furthermore, the hindcast simulation does not include the April 2022 event, limiting direct model evaluation.

The discussion of future extremes (e.g., approximately doubling of maximum rainfall) also relies heavily on single extreme values rather than a formal extreme value framework and should therefore be interpreted with greater caution.

Lines 364–389: The circulation-based interpretations (e.g., cyclone influence – also not clearly mentioned tropical or extratropical cyclones, SLP intensification as key driver) are visually inferred but not supported by objective diagnostics or a cyclone identification methods. A clearer separation between event evaluation, climatological analysis, and future projections is strongly recommended.

Comment 10

It is strongly recommended to revise the figure titles and in-panel labels for clarity and consistency. In many cases, the titles require multiple readings to understand (e.g., Figure 10). Figures should clearly and explicitly describe what is being shown. Figures should also be cited more precisely within the text. For example, references such as “Fig. 10” do not clearly indicate which panel(s) the reader should examine. Several figures also could potentially be merged to improve conciseness (e.g., Figures 2 and 3; Figures 6 and 7).

Comment 11

The Discussion and Conclusion sections should be clearly separated. At present, a substantial amount of interpretation is embedded within the Results section, which weakens the manuscript structure. The Discussion should focus on interpreting the main findings (after considering all the referees’ comments) in the context of previous studies, supported by appropriate references, rather than introducing extensive discussion throughout the Results section.

Minor comments

Comment 1

Lines 25–27: This sentence appears to summarise the work undertaken in the manuscript and would be more appropriate near the end of the Introduction, where the study objectives are typically presented. A similar point applies to Lines 62–64.

Comment 2

Several statements and/or findings in the Introduction are not adequately supported by references (e.g., Lines 28–29, 36–37, 56–58, 106–108). It is also unclear whether some results originate from the present study (e.g., Lines 80–83). If so, they should be moved to the Results section; otherwise, appropriate references should be provided.

Comment 3

The Introduction is generally well written; however, the overall flow could be improved. In particular, the progression from previous work to research gaps, motivation, and study objectives is not always clear, and some research gaps are introduced too late in the section (e.g., Lines 106–115).

Comment 4

The use of the 99th percentile of daily precipitation is reasonable and widely used; however, sub-daily extremes are often more sensitive to climate change and may show stronger intensification signals. While I recognise that long-term hourly observations may be limited, the authors should briefly justify the use of daily-scale extremes and discuss how this choice may influence the interpretation of the results, particularly if mesoscale convective processes contribute to regional extremes.

Comment 5

Lines 216–221: The use of 15-year samples (for CHIRPS) to estimate return levels potentially associated with longer return periods raises concerns regarding robustness, as this requires extrapolation beyond the available data range. The associated uncertainties should therefore be quantified and discussed more explicitly.

Comment 6

Several sections, particularly within the validation and results, would benefit from substantial tightening. Many paragraphs contain excessive reporting of individual values, which obscures the main scientific message. For example, Section 5.2 reads more like a listing of values than a synthesis of key findings. In many cases, the historical and future values are also very similar, which further reduces clarity.

Comment 7

Line 276: Please clarify what is meant by the "timing of extreme events". It is currently unclear whether this refers to intraseasonal, seasonal, or interannual variability. If such analyses were performed, they deserve clearer explanation and interpretation.

Comment 8

Figure 10: It is unclear how the number of extreme events has been calculated, particularly in Figures 10e–f. The reported values (~6000 events per year) appear unrealistically high and are difficult to interpret. Please clarify how extremes are defined and counted (e.g., grid-point exceedances versus spatially aggregated events) and verify whether these values are physically meaningful.

Comment 9

Figure 11: The reported correlation values (approximately −0.2 to +0.2) appear weak, and it is unclear whether they are statistically significant. Given that the correlations are generally low, the robustness and interpretability of this analysis are questionable. If these relationships are weak or non-significant, this section could be substantially condensed.

Technical Corrections

Comment 1

Lines 6–7: Please ensure that all abbreviations are defined at first occurrence throughout the manuscript, including in the Abstract (e.g., CCLM, CORDEX). In addition, mathematical symbols should be used where appropriate (e.g., "×" instead of "x").

Comment 2

Line 60: The sentence beginning with "Her dissertation…" would benefit from revision.

Line 87: Please revise “In the previously cited study” to “In the previous study, …” and place the citation at the end of the sentence to improve flow.

Comment 3

Line 182: Please explicitly clarify that ERA5 is a reanalysis-based product and not a purely observational dataset.

Comment 4

Line 298: Please verify whether this should be 40 °E rather than 40 °S.