

Author comment to RC1 in “Droughts in western Central Europe and associated atmospheric circulation patterns since 1844”

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5 Comment of the reviewer

Comment of the authors

Neimry et al. have analysed historical drought trends in western Central Europe (BE,NL,LUX,partFR,partDE). Using three different reanalysis products and four meteorological stations, they calculate SPI3 and SPEI3 and analyse trends and circulation associated with drought events. I find parts of the study interesting, for example the circulation drivers, the long historical perspective and the decadal variability in drought occurrence shown in Figure 5. However, I sometimes got lost in reading the results sections where many statistics are noted and discussed, often in a very lengthy way.

My first point of advise to the authors is thus to make an attempt at shortening the manuscript, by being more direct or where possible leaving out some explanations or moving things to the supplementary materials. An example, at the start of your data section:

15 *“In addition to the identification of droughts, an objective is to connect them to atmospheric circulation. Consequently, reanalyses are pivotal datasets for this study. A reanalysis is a combination of historical weather observations and weather forecasting models, using a method called data assimilation (Slivinski et al., 2019; Valler et al., 2024). This approach provides a comprehensive dataset that represents fields consistent with each other through the application of physical laws within the model and the elimination of spatial and temporal gaps.”*

20 In my opinion this whole bit can go. The objectives are already listed in the introduction, and I think the readers of WCD can be expected to know what reanalyses datasets are. Hopefully by critically assessing the text as a whole, you will be able to achieve a more streamlined and clear manuscript, that better showcases your work and results.

A substantial effort will be made to streamline the manuscript. In particular, sections containing background information that can reasonably be assumed for the readership of WCD will be shortened or removed, including the general description of reanalysis datasets.

In addition, the Data and Data Evaluation sections will be shortened and merged. Section 6.2 will be revised to focus more clearly on the key findings, avoiding detailed repetition of results already shown in the figures. The Discussion section will be substantially reorganised and shortened, with several subsections merged or reduced to better emphasise the interpretation,

significance, and limitations of the results. Overall, these revisions are expected to reduce the length of the manuscript by approximately 30%, while improving its conciseness and focus and preserving its core scientific content.

My second point is a hesitation regarding the fact that all analysis (outside Fig 8a/b) is related to SPI or SPEI trends/values, rather than the actual physical input variables of precipitation, temperature and evapotranspiration. I struggle with terms like ‘dryness’, when SPI/SPEI indicate a relative position on the wet/dry spectrum. Please consider performing or adding (in the SI?) Some analysis on the physical variables, and using the language to go with that (related to my point on clarity before). E.g. I believe that when you write about a ‘decline in dryness’ measured by SPI, you in fact mean an increasing precipitation trend. As a physically trained climate scientist that is easier to interpret.

The physical input variables underlying drought will be analysed to complement and clarify the physical interpretation of the results. These additional analyses will be provided in the Supplementary Material to ensure conciseness of the main manuscript.

In addition, we agree that the term “dryness” is not the most appropriate wording in the context of SPI-3 and SPEI-3, which represent standardised indicators of relative hydroclimatic anomalies and drought conditions rather than direct measures of physical dryness. The terminology will therefore be revised throughout the manuscript to better reflect the meaning and direction of the analysed indicators. Depending on the context, references to “dryness” will be replaced by more precise terms such as “dry or wet conditions” or “drought conditions”, while references to “dryness trends” will be replaced by “drying/wetting tendencies”, “hydroclimatic trends”, or wording directly referring to the underlying variables (e.g. precipitation or AED), where appropriate.

Finally, I miss a critical evaluation in the discussion of the validity of your results and assumptions. Throughout the results section there are quite a few places where the three reanalysis products thoroughly disagree with each other, even when a common period is used. No statements or conclusions are made regarding this, whilst for me this is very important. Furthermore, given your interesting Fig. 5, should you not comment more on the influence of interannual/multidecadal variability on trends in relation to climate change trends (signal to noise discussion)? In a similar matter, I miss a critical discussion on the use of Thornthwaite, which as far as I know is a dated method of calculation PET. I fully understand that when only monthly data is available, this is your only option, but this will influence your computed trends and results, so I expect a critical discussion of implications.

In the revised manuscript, we will substantially restructure the Discussion by removing overly descriptive analyses and by adding a dedicated subsection on scope and limitations. This section will explicitly address differences between the reanalysis products and their implications for the robustness of the results, the role of multidecadal variability in modulating apparent trends, and the limitations associated with the use of the Thornthwaite AED (or PET) and its potential influence on SPEI-based trends. To support this discussion, additional analyses comparing Thornthwaite and Penman–Monteith estimates will be

provided in the Supplementary Material. These additions ensure a more balanced interpretation of the results and help avoid
60 overstatement of the main conclusions.

As such, my advise to the editor is to offer the authors a chance to reword, and consider the manuscript again after major
revisions have been made.

We thank the reviewer for their thorough evaluation of the manuscript and are grateful for the time and effort dedicated to
providing detailed and constructive comments, which have significantly helped improve the quality of the work. We address
65 all comments point by point below.

Major comments

- I'm based in the Netherlands, a prominent part of your region of interest. I'm most familiar with the Dutch literature
on drought, and these are often missing in your reference list. I list some, as a suggestion to extend your literature
review, note this is not a complete list. These are a mix of trend analyses, event analyses and future projections: Philip
70 et al. (2020), Zscheischler et al. (2020), Huang et al. (2021), Van der Wiel et al. (2021), Mens et al. (2022), Aalbers
et al. (2023).

The referenced studies will be incorporated into the manuscript and will help strengthen and complement the literature review,
particularly in the Discussion section. We appreciate the inclusion of studies from the Dutch literature, which provide important
regional context for drought variability, recent events, and future projections in western Central Europe.

- Line 157: I'm confused how one can reliably add detail to datasets by interpolation, as you do by upscaling from 1.8
75 degree resolution to 0.25 degree resolution. Have you verified what the resulting (spatial) fields look like?

We fully agree that interpolation does not add spatial detail or new information, and this was not the intention. The interpolation
of 20CRv3 and ModE-RA to the ERA5 grid was performed solely to enable spatial comparison on a common grid. This
approach retains the higher spatial resolution of ERA5 within this relatively small study area, rather than rescaling the data to
80 a coarser grid and thereby losing information. To address the reviewer's concern, we have visually inspected the resulting
spatial fields and confirm that patterns are preserved after interpolation. At the aggregated regional scale, this resampling has
no significant impact on the conclusions. This point will be clarified in the revised manuscript, where we will also emphasise
that the analysis focuses on mid- to large-scale features and that small-scale differences remain subject to uncertainty due to
resolution mismatches.

- Why have you chosen to use ERA5-land rather than normal ERA5? You don't use any of the land surface variables
85 in your analysis, that would benefit from the additional offline land-model calculations?

ERA5-Land was initially considered to assess the sensitivity of the evaluation to spatial resolution, as it provides a higher spatial resolution than ERA5. We acknowledge, however, that ERA5-Land is not simply a higher-resolution version of ERA5, but a separate land-surface reanalysis.

90 Since ERA5-Land did not provide additional relevant information for the analysis and was not used in the core methodological framework, we decided to remove it entirely from the manuscript to avoid unnecessary complexity and potential confusion for the reader. ERA5 will also replace ERA5-Land in Figure 1 to ensure consistency with the datasets used throughout the analysis. This simplification does not affect the results or conclusions of the study.

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- Line 306: In your analysis of the quality of the different reanalyses datasets, you fully rely on SPI/SPEI variables which are normalised, derived variables. Please check the reliability of input variables as well, e.g. PR, TAS, PET. Similarly, I think your analyses of trends may be helped by analyses of the input variables rather than SPI/SPEI alone.

In the revised manuscript, the evaluation will be extended to include the main input variables used to compute the drought indices, namely precipitation, temperature, and atmospheric evaporative demand (AED/PET), in addition to SPI-3 and SPEI-3. These additional analyses will be provided in the Supplementary Material to ensure a concise main manuscript. Analysing 100 these variables directly will also improve the interpretation of the observed trends by providing a clearer physical understanding of the processes underlying the hydroclimatic changes identified through the drought indices.

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- Line 205 and 327: Thornthwaite, through its solo dependence on temperature, likely shows a stronger (too strong) trend over the analysis period towards higher PET values. This can somewhat be seen in Figure S1, where the red line has a stronger slope towards low SPEI values (drought). Similarly, in line 327, SPEI-3 drought occur at the end of the time series, whilst SPI-3 droughts occur more at the beginning. Can you analyse, maybe in a similar figure to S1, how the PET differences are, i.e. without the step towards SPEI? Please then also add a point in the discussion on this, and how you think the choice for Thornthwaite has impacted your results (section 7.4 I guess).

110 Additional analyses comparing AED (or PET) methods and their respective trends will be conducted and included in the Supplementary Material to ensure conciseness of the main manuscript. These results will allow a clearer assessment of the sensitivity of SPEI-based conclusions to the choice of AED formulation. In addition, the implications of using Thornthwaite AED, and its potential influence on long-term trends in SPEI-3, will be explicitly discussed in the revised manuscript within a new section addressing the scope and limitations of the study.

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- Line 403: what does SPI3 minus SPEI3 physically mean, it is a difference between normalised quantities that partially rely on the same input variable. Wouldn't it make more sense to assess the trend in (normalised if you wish) PET, rather than in this strange quantity?

We agree that the physical interpretation of the SPI-3 minus SPEI-3 difference was insufficiently explained in the original manuscript. The intention is not to interpret this quantity as a direct physical variable, but rather as an indicator of the relative contribution of atmospheric evaporative demand (AED/PET) to drought conditions compared to precipitation alone. Since SPI-3 is based solely on precipitation whereas SPEI-3 additionally accounts for AED through the climatic water balance, their difference highlights the extent to which variations in AED amplify or mitigate drought conditions beyond precipitation-driven variability. We chose this approach rather than analysing AED trends alone because AED exhibits a widespread long-term increase across seasons, as will be shown in the Supplementary Material, while the relative influence of AED on drought conditions depends on its balance with precipitation changes. The SPI-3 minus SPEI-3 comparison therefore helps identify the seasons during which increasing AED contributes most strongly relative to precipitation variability. This interpretation will be clarified in the revised manuscript.

- Section 6.2: This section is very dense with numbers and comparisons, but as a result difficult to follow and distill your main results from. There are a lot of differences mentioned, which are partially attributed to differences (biases?) in reanalysis datasets, but without coming to a conclusion. The influence of multidecadal variability is not mentioned, even though we know (and you showed in Figure 5) that this is of large influence especially for precipitation.

Section 6.2 will be substantially revised to improve readability and focus on the main findings. The amount of numerical detail and inter-comparisons will be reduced, and the key messages will be clarified and more clearly emphasised. We will also provide clearer conclusions within the section, rather than only listing differences.

The influence of multidecadal variability will not be explicitly discussed in this section, as it will instead be addressed in detail in the new scope and limitations subsection of the Discussion section, in order to maintain a clearer separation between results and interpretation.

Minor comments:

- Line 21: “raising particular concern” while I agree there is concern, I’m not sure it is more concerning in this region than in other regions globally. The Mediterranean, or Sahel, see larger trends towards droughts, transitioning to seasons without significant rainfall. The trends in Central Europe are relatively smaller, the climates relatively wetter and cooler.

We agree that drought trends and associated impacts are more severe in other regions of the world. Our intention was not to imply that Central Europe represents the region of greatest concern globally, but rather to highlight the relatively recent emergence of increasing drought conditions in a region that has historically been considered comparatively wet and temperate. To avoid overstating this aspect, the term “particular” will be removed from the revised manuscript.

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- Line 45: the transition zone between drier South and wetter North in climate projections is also a prominent feature of the Netherlands national climate scenarios (Van der Wiel et al. 2024).

The reference to van der Wiel et al. (2024) will be added to the manuscript to complement the literature review regarding Central Europe as a transition zone between the drying conditions projected for Southern Europe and the wetter conditions projected for Northern Europe.

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- Line 67: You only use CHDs six times in the manuscript, do you need the abbreviation?

We agree that the abbreviation was not used frequently enough to justify its introduction. The abbreviation “CHDs” will therefore be removed in the revised manuscript.

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- Line 120/Figure 1: Generally I’d advise to avoid using reanalysis-based precipitation products, can you recalculate these values using (for example) the E-OBS gridded observational data? Do mention this limitation in your discussion as well.

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Since the study is entirely based on reanalysis datasets, we consider it more consistent to use the same framework for the descriptive climatological figures, including mean annual precipitation. The purpose of Figure 1 is primarily to provide a general climatic context for the study region rather than a precise observational reference. Nevertheless, we acknowledge the limitations associated with reanalysis-based precipitation fields, particularly regarding spatial precision. This point will be discussed more explicitly in both the Data Evaluation section and the new “Scope and Limitations” subsection of the Discussion.

- Line 157: Please add details on your method to bias adjust to the 1979-2008 climatology, do you remove all trend or calculate the bias in this period and adjust the full dataset according to this bias?

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The manuscript will be clarified to better describe the adjustment procedure. No detrending was applied and the temporal variability of the datasets was preserved. Monthly anomalies from 20CRv3 and ModE-RA were first calculated relative to their respective climatologies and then multiplied by the corresponding 1979–2008 ERA5 climatology to express all datasets on a common climatological reference framework for comparison. This procedure preserves the relative temporal variability of each dataset while adjusting the climatological baseline to match ERA5. The purpose of this adjustment is therefore not to remove long-term variability, but to ensure comparability between reanalyses while retaining their inherent temporal evolution.

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- Line 191: I think you have calculated SPI/SPEI at grid point level, and then spatially averaged, but am not 100% sure due to “In order to obtain the SPI-3 and SPEI-3 values for the entire western Central Europe region, precipitation and PET values for all land cells have been averaged over the study area.”, can you clarify?

We clarify that SPI-3 and SPEI-3 are computed at both the grid-cell level and the regional scale. For the regional-scale indices, precipitation and atmospheric evaporative demand (AED/PET) are first area-weighted across all land grid cells within western Central Europe, and the indices are then computed from these aggregated time series. This approach is adopted because the standardisation underlying the indices is non-linear; therefore, the index computed from aggregated inputs is not equivalent to the spatial average of grid-cell indices. Following Spinoni et al. (2016), aggregation prior to index computation preserves a more consistent regional drought signal. The manuscript will be revised accordingly to remove the ambiguity.

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- Line 205: Thornthwaite, through its solo dependence on temperature, likely shows a stronger (too strong) trend over the analysis period towards higher PET values. This can somewhat be seen in Figure S1, where the red line has a stronger slope towards low SPEI values (drought). Can you make a similar figure to S1, just for PET, to quantify this?

As mentioned in the response to the major revision comment above, additional analyses will be conducted accordingly. The requested comparison will be included in the Supplementary Material, where AED/PET-only trends will be explicitly shown to quantify differences between methods and to better illustrate the sensitivity of long-term AED trends to the choice of formulation.

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- Line 225: your reference period for circulation is different from the period for SPI/SPEI calibration, is this done on purpose?

We thank the reviewer for pointing this out. This was indeed a mistake in the manuscript. The Z500 anomalies are calculated relative to the 1947–2008 reference period, and not the 1979–2008 period as originally stated. The 1979–2008 period was only used, as described in the Data section, for the adjustment between reanalyses. This will be corrected and clarified in the revised manuscript.

- 190
- Line 262: “Whilst the aforementioned two variables are hypothesised to remain constant across time for each reanalysis, this is not the case for the former.” I’m not a native speaker, but I don’t think this makes grammatical sense. ‘The former and the latter’, refers to a list of two items, you have three.

We agree that the original wording was grammatically incorrect and confusing. The sentence will be revised to: “While the latter two features are assumed to remain constant over time for a given reanalysis, this is not the case for observational input.”.

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- Line 272: the Climate Explorer asks to be referred to as a dataset/tool as well, see <https://climexp.knmi.nl/citation.cgi>.

The appropriate reference and citation to the Climate Explorer citation guidelines will be added to the revised manuscript.

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- Figure 3: the SPI reference period is different in the caption than in the main text, the y-label should state SPI-3 [10-year], the figure doesn’t include labels a) and b), I suggest adding to the caption that the shaded area is in the colour of the dataset with the large error relative to the station measurement.

205 The “-3” will be added to the y-axis label in Figure 3 to clarify that the figure shows low-frequency SPI-3 variability. In addition, the reference period in the caption will be corrected. The missing panel labels (“a” and “b”) will also be added to the figure. Finally, the caption will be revised to clarify that the shaded areas are shown in the colour of the datasets considered unreliable over the extended periods.

- Line 357: why does ERA5 ‘support’ this finding, if it is the only reanalysis in Figure 5 that includes this decade?

210 We agree that the wording “ERA5 supports” is misleading, as ERA5 is the only dataset covering the 2010s in this analysis and therefore does not provide an independent confirmation. The sentence will be revised accordingly to remove the implication of cross-dataset validation. It will instead simply report the ERA5-based result for this period without suggesting additional support from other reanalyses.

- Line 362: I’m not sure what you mean with 16th driest conditions here.

215 We agree that the original wording was unclear. The intention was to refer to the theoretical expectation that, under a standard normal distribution, approximately 16% of values fall below -1 (SPI/SPEI drought threshold). The sentence will be revised to explicitly refer to this definition and to remove the ambiguous wording “16th driest conditions”.

- Figure 5: please do not use the rainbow colour scale, and please consider having a discrete colour bar (e.g. 10 colours) rather than a continuous, to help reading the figure. The discs are very difficult to see, maybe you can consider a different way of showing those. Just brainstorming: an additional subfigure with a line graph, or larger discs below the map.

220 We agree that the original colour scale was not optimal for interpretation and readability. The figure will therefore be revised using a perceptually uniform colormap from the *viridis* R package (Garnier et al., 2018), adapted into a diverging scheme and discretised into classes to improve interpretability while remaining suitable for colour-blind readers. The size of the discs will also be increased in the revised figure to enhance readability.

- Line 369: what is the mean dryness? If it is the mean SPEI3 value, is it not by definition that the two quantities are closely correlated?

225 We agree that the wording “mean dryness” was unclear and could be misleading, and that the interpretation of this metric was not sufficiently justified. In addition, we acknowledge that the reported relationship would largely reflect the definition of the SPEI-3 index itself. For these reasons, the entire paragraph will be removed in the revised manuscript, as it did not provide additional insight beyond what is already captured by the index definition.

- Figure 6: a) please replace the rainbow colour scale, consider choosing a white-centred colour scale instead. Why is De Bilt/Frankfurt/Paris not included as a disc? b) I had to zoom in to about 200% to be able to read these graphs,

consider modifying such that that is no issue anymore (line width, text size, font weight etc). Maybe all the slope values and p-values can be written in a table?

235 Figure 6 will be revised accordingly. The original colour scale will be replaced by a perceptually uniform diverging colormap with a neutral midpoint to improve interpretability while avoiding misleading visual emphasis. The station representation will also be clarified in the caption: Uccle is the only station shown as a disc, as it is the only station with a complete record covering the full 1947–2023 period within the datasets used in this study; this will be explicitly stated.

240 In addition, to improve readability, the slope and p-values will be removed from the plots and compiled into a separate table accompanying the figure. This change will improve the legibility of the panels and allow clearer presentation of the statistical results without overcrowding the graphics.

- Line 380: this is slightly silly thing, but SPI or SPEI don't measure 'dryness', they are an indicator of the relative dry or wet state of the past months. As such I don't think the term 'dryness trends' are correct, maybe just (meteo)hydrological trends? Also in caption of section 7.3.

245 We agree that the term "dryness" was not the most appropriate wording in the context of SPI-3 and SPEI-3, which represent standardised indicators of relative hydroclimatic anomalies and drought conditions rather than direct measures of physical dryness. The terminology will therefore be revised throughout the manuscript to better reflect the meaning and direction of the analysed indicators. Depending on the context, references to "dryness" will be replaced by more precise terms such as "dry or wet conditions" or "drought conditions", while references to "dryness trends" will be replaced by "drying/wetting tendencies", "hydroclimatic trends", or wording directly referring to the underlying variables (e.g. precipitation or AED), where appropriate.

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- Figure 7: can you add somewhere (maybe in the caption) over how many seasons these were calculated? So for ERA5, 31% of how many droughts are related to the Baltic High? This may give the reader a sense of (statistical) stability.

The caption of Figure 7 will be revised to include the number of seasonal drought events considered for each dataset, allowing the reader to better assess the statistical robustness of the reported percentages (e.g. ERA5: 31% of a total of N events associated with the Baltic High). This additional information will improve the interpretability and context of the results.

- 255
- Figure 8: a) please specify what the box (P25/P75?) and whiskers (min/max?) show, b) why not show this as four bar charts?, c) please replace the rainbow colour map, why not include De Bilt/Paris etc as a disc?

(a) The figure caption will be revised to explicitly define the boxplot elements, including the quartiles, median, whiskers, and outliers, as these definitions may vary depending on the plotting convention used. (b) We agree that bar charts allow a more direct comparison between AED and precipitation anomalies in this case. The figure will therefore be revised accordingly. (c)

260 The original colour scale will be replaced by a perceptually uniform diverging colormap with a neutral midpoint to improve interpretability while avoiding misleading visual emphasis. The station representation will also be clarified in the caption:

Uccle is the only station shown as a disc, as it is the only station with a complete record covering the full 1947–2023 period within the datasets used in this study; this will be explicitly stated.

- Line 510: “(Fig. 6b, Fig. S8b and Fig. S9b)” should this be Fig 8b?

265 We thank the reviewer for pointing this out. This was indeed a typographical error. The reference will be corrected from “Fig. 6b” to “Fig. 8b” in the revised manuscript.

- Figure S11: What are the units on the y-axes? What is shown on the x-axis, are these all identified droughts consecutively? So a modified time axis?

270 The figure caption will be clarified to specify the units shown on the y-axes and to explain that the x-axis does not represent continuous chronological time, but rather a concatenated sequence of identified drought events displayed consecutively for comparison purposes.

- Line 550: what do you mean here by “given the natural variability”? If 1921 was a large outlier, than would we expect an even larger (or slightly smaller if due to PET trends) to occur in the next 100 years?

275 We agree that the original wording was unclear. The paragraph will be revised to better distinguish the perspectives provided by SPI-3 and SPEI-3. We will emphasise that, while increasing precipitation trends partly explain why the most extreme SPI-3 drought occurred earlier in the record, the increasing influence of AED in SPEI-3 may contribute to recent droughts reaching comparable severity. In addition, we will note that the storyline approach (van der Wiel et al., 2021; Aalbers et al., 2023) suggests that 2018-like drought events are expected to become more severe under increasing AED. The ambiguous expression “given the natural variability” will therefore be removed.

280 • Line 551: maybe add some literature on multi-year droughts in the region? Van der Wiel et al. (2023) or Van Mourik et al. (2025).

Van der Wiel et al. (2023) is already cited in the following sentences in relation to the drivers of multi-year drought events. We will add Van Mourik et al. (2025) to further complement the discussion on multi-year droughts in the region and to strengthen the relevant literature context.

285 • Line 565: “Some studies have been linking” should probably be “Some studies have linked”

The sentence will be revised accordingly: “Some studies have been linking” will be replaced with “Some studies have linked” in the revised manuscript.

- Line 640: degree symbol missing. Furthermore, is CC scaling relevant to year-round precipitation? I always assumed it was mostly associated with convective summer rain events, rather than all precip year round? In the KNMI23 projections, extreme precip sometimes goes beyond CC (Van Dorland et al. 2023).

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We agree that Clausius–Clapeyron (CC) scaling is most robustly established for short-duration convective extreme precipitation events, and that its mechanistic applicability to year-round mean precipitation is debatable. In the revised manuscript, we will therefore remove the extended discussion on CC scaling to streamline the manuscript and better focus on the role of atmospheric dynamics. We will instead emphasise that, while the observed year-round precipitation trends are broadly consistent with thermodynamic forcing, the divergent seasonal trends cannot be explained by thermodynamics alone, highlighting the role of dynamical changes in atmospheric circulation.

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- Line 735: I think you mean negative precipitation anomalies rather than precipitation deficits. Deficits have been defined in many different ways in the literature so might cause confusion. (Also please check for other places in the manuscript.)

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We agree that the term “precipitation deficits” may be ambiguous. The manuscript will therefore be revised, and “precipitation deficits” will be replaced with “negative precipitation anomalies” where appropriate, including in the highlighted instance and other occurrences throughout the text to ensure consistency and avoid confusion.

- Line 746: Maybe add to this paragraph something short on temporally compounding or multi-year droughts?

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The paragraph will be reorganised and moved to the Conclusions section in the revised manuscript. In addition, a brief discussion of temporally compounding and multi-year droughts, as well as their potential impacts, will be added to complement the discussion of associated challenges.

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