The Impact of Convection-Permitting Rainfall on the Dryland Water Balance

These are the comments in response to the review by Dr Bo Huang.

We would like to thank Dr Bo Huang for his helpful comments on our manuscript. Below please find responses to each comment, which we hope help clarify some of the points raised.

1. The authors use the FAO Penman-Monteith method (Section 2.2) to calculate potential evapotranspiration (PET) and list seven atmospheric variables. However, the equation itself and the role of these variables in its application are not explicitly provided. Additionally, Penman-Monteith method uses wind speed at 2m height but this manuscript does not clarify how wind speed measured at 10m height is adjusted to 2m. Please include the equation, explain the variables, and describe the methodology for converting 10m wind speed to 2m (e.g., logarithmic wind profile adjustment or FAO-recommended constants).

Thank you for spotting this. We actually did convert wind speed from 10 to 2 meters, but we omitted to clarify this in the methods. The manuscript will be updated to include this step. We converted the original climate model output which is for 10 meters above the land surface to the required 2 metre value using the logarithmic velocity profile above a short grass surface:

$$u_2 = u_z\left(\frac{4.87}{\ln(67.8z - 5.42)}\right)$$

Where u_z is the wind speed at height z above the land surface (10 meters in this case) computed as $u_z = \sqrt{u^2 + v^2}$. It is worth noting that where the shape of the velocity profile does not follow this form, errors may arise.

For brevity we didn't include the Penman-Monteith equation itself as it is heavily documented elsewhere, but we appreciate that clarity would be improved if we explicitly outlined its use in this study. We follow the methodology outlined by Singer et al (2021).

We used the Pen-Montieth equation for reference crop evapotranspiration as described in Allen et al (1998) to compute PET at an hourly resolution (t) at each pixel (x) in our domain:

$$hPET_{x,t} = \frac{0.408\Delta(R_n - G + \gamma(\frac{37}{T_a + 273})u_2(e_s - e_a)}{\Delta + \gamma(1 + 0.34u_2)}$$

Where R_n is hourly net radiation (MJ m^{-2}), G is the soil heat flux (MJ m^{-2}), γ is the psychometric constant (kPA °C^{-1}), Δ is the slope of saturation vapour pressure (kPA °C^{-1}), T_a is hourly air temperature (°C), e_s is saturation vapour pressure (kPa), e_a is the actual vapour pressure, and u_2 is the converted (from 10 m above the land surface) wind speed (m s^{-1}) at 2 m above the land surface.

For use in the above equation, e_s and e_a are calculated using the Tetens equation (Tetens, 1930) using hourly air temperature (T_a) and dew point temperature (T_{dew}) as detailed below (calculations are in in °C after converting from K):

$$e_s = 0.6108\exp\left(\frac{17.27 * T_a}{T_a + 237.3}\right)$$

$$e_a = 0.6108\exp\left(\frac{17.27 * T_{dew}}{T_{dew} + 237.3}\right)$$

Slope of saturation vapour pressure (Δ) and the psychrometric constant are calculated as follows:

$$\Delta = \frac{4098e_s}{(T_a + 237.3)^2}$$

$$\gamma = \frac{C_p * P}{\varepsilon * \lambda}$$

Where P is atmospheric pressure, C_p is the air's specific heat at constant pressure based on the ideal gas law with a value of 1.013×10^{-3} MJ kg^{-1} per °C, ε is the ratio of the molecular weight of water vapor to that of dry air (0.622), and λ is the latent heat of vaporization, (2.45 MJ kg^{-1}).

Net radiation (R_n) is estimated using net solar (R_s) and thermal radiation (R_t) as (all values in MJ m^{-2}):

$$R_n = R_s - R_t$$

Finally soil heat flux (G) is estimated as:

$$G = \begin{cases} G_{day} = 0.1 * R_n \\ G_{night} = 0.5 * R_n \end{cases}$$

Where the soil heat flux (G) is estimated to be 10% of net radiation (R_n) during the day and 50% during the night (as the night-time heat flux is negative). At each pixel we use net solar radiation to define day and nighttime periods. Unlike many other PET datasets, nighttime PET values have not been automatically set to zero.

2. The climatological analysis appears to aggregate data across the entire study period. Are there any differences between wet and dry seasons? Given the region's likely seasonal contrasts (Figure C1), will the results/signal change in wet and dry seasons?

Thank you for this comment which makes a good point. Yes, we did also analyse at the seasonality in the rainfall metrics. For brevity and to reduce the number of figures we decided to only report the annual results. However, we can add the below figure to supplementary material, which shows the 'drizzle' effect in P25 is evident in all the seasons including the MAM and OND rainy seasons and the dry season(s) JF and JJAS.

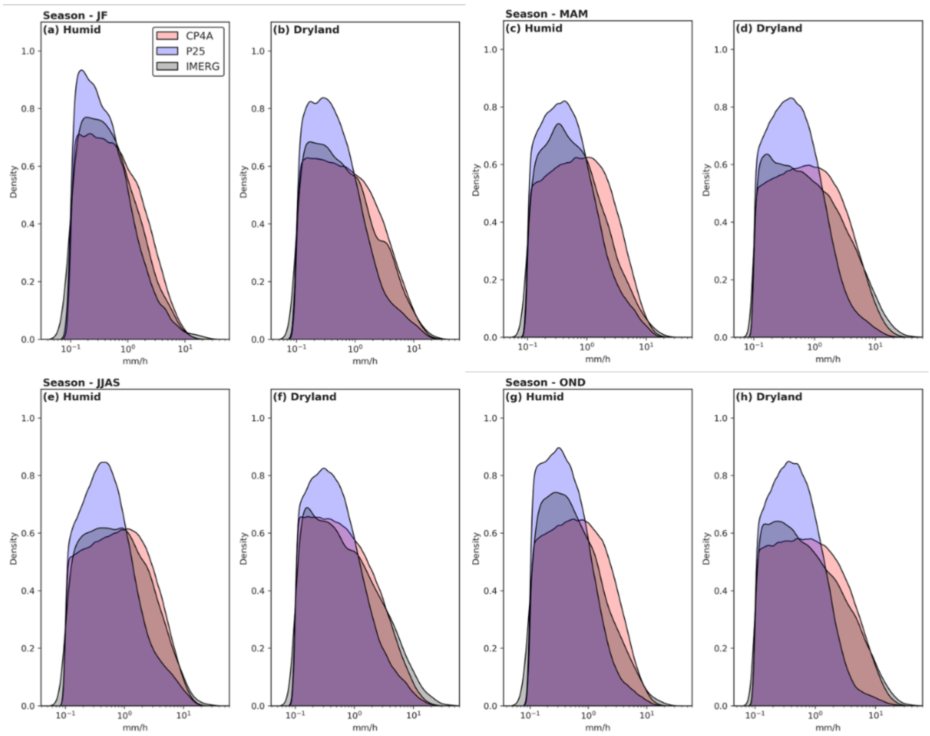

Figure C1. Rainfall KDE Plots. Kernel density estimate (kde) plots of CP4A, P25, and IMERG hourly rainfall in humid (AI >= 0.65) and dryland (AI < 0.5) regions of the Horn of Africa for JF (a-b), MAM (c-d), JJAS (e-f), and OND (g-h). Plots exclude dry hours by dropping any hours that receive < 0.1 mm/hr of rainfall. Note this figure is included in Appendix C and is labelled as Fig. C1.

3. The aridity index (AI) is referenced repeatedly (Lines 37, 226, 527), but its definition (e.g., ratio of precipitation to PET or another formula) is not provided. Please clarify the specific equation or source used for calculating AI to ensure reproducibility and reader comprehension. Or directly cite the reference to indicate the four regions other than use value of aridity index.

We use Aridity Index (AI) as P/PET (Zomer et al, 2007). Under section 2.4.2 we will add the following text and table (which will be labelled Table 1 in the main body of text): Given the sensitivity of dryland hydrology to rainfall characteristics, we wanted to establish whether relative differences in hydrological outcomes between Hydrus simulations (when forced with CP4A and P25 rainfall/PET) varied with aridity. Hence, we ran four 1-D hydrological simulations along an aridity gradient across the Horn of Africa (HOA), ranging from humid to hyper-arid (Fig. 1). Here we classify aridity based on aridity index (AI = P/PET) values taken from the CGIAR-CSI (Consortium of International Agricultural Research Centres' Consortium for Spatial Information) (Zomer et al., 2007) using the classification of Mirzabaev et al (2019).

The Aridity Index (AI) is a numerical indicator of climatic aridity based on long-term precipitation deficits relative to atmospheric water demand:

$$AI\ (Aridity\ Index) = MAP\ /\ MAE$$

Where MAP is mean annual precipitation and MAE is mean annual potential evaporation, CGIAR-CSI calculate both MAP and MAE using data obtained from WorldClim Global Climate Data (Hijmans et al, 2005). CGIAR-CSI outputs AI values at 1 km resolution, which can be used to define the climate type based on the climate classification of Mirzabaev et al, 2019, shown in Table 1.

Climate Type	Aridity Index
Hyper-Arid	AI < 0.05
Arid	0.05 <= AI < 0.2
Semi-Arid	0.2 <= AI < 0.5
Dry Sub-Humid	0.5 <= AI < 0.65
Humid	AI >= 0.65

Table 1 – Climate classifications based on aridity index thresholds taken from (Mirzabaev et al, 2019).

4. Figures 7 and 9 present soil moisture distributions, but the frequency ranges differ significantly between the two. What explains this discrepancy? Additionally, Figure 9 is described as including green and red dashed lines in the caption, but these are not visible in the figure as provided. Please resolve this inconsistency.

Discrepancies between soil moisture distributions reflect soil properties at each site, total rainfall, rainfall characteristics, and PET. For example, at our more arid sites (Sites A and HA) soil moisture is limited to 30-50% saturation, whereas Sites SA and HU see soil moisture saturations reach greater than 80%. This is likely due to the higher total rainfall and lower PET at these sites compared to our more arid locations. However, there are also differences in soil moisture distributions between Site A and Site HA, with soil moisture on average being higher at our hyper-arid site. This may appear counter-intuitive given these is our most arid location, but soil moisture is also sensitive to root water uptake. At Site A shrubs are present throughout the profile, and take up 12-21% of infiltration, while Site HA is bare soil. We have also shown that soil moisture is sensitive to rainfall characteristics, at Site HA more rainfall is delivered via heavy events (30-82% vs 12-49%) and the magnitude of extreme (99^{th} percentile) rainfall is also higher (17.9 vs 11.2 mm/h). You can see the deeper penetration of wetting fronts at Site HA in fig 8.

Apologies - the caption of Figure 9 needs updating. The correct caption should read: Soil Moisture Distributions with Wilting Points. Modelled distribution of soil moisture at 1.2 mbgl at Site SA (a) and A (b) using P25 (blue) and CP4A (red) rainfall and PET. The dashed orange line represents the wilting point range for Acacia shrubs, based on taking the upper and lower Feddes' parameters given in Table A2 (wilting point = *P2H*) (Sela et al, 2015).

5. Figure 10 is labelled as depicting rainfall and PET but does not include these variables in the presented panels. Please revise the caption to accurately reflect the displayed data. Figure 10 is labelled as depicting rainfall and PET but does not include these variables in the presented panels. Please revise the caption to accurately reflect the displayed data.

We apologise for this error. The plot previously included rainfall and PET but they were removed as it made visualising the other variables more challenging. Cumulative rainfall and PET are given in Table 1. Additionally, infiltration largely follows cumulative rainfall, with differences between P25 and CP4A Hydrus simulations largely due to the infiltration partitioning between different stores. If it would improve clarity, we can add rainfall and PET to Figure 10.

If rainfall and PET is removed, the Figure 10 caption will read: Cumulative Infiltration Runoff, Evaporation, Transpiration, and Deep Infiltration. Modelled components of the water balance using CP4A (solid lines) and P25 (dashed lines) rainfall/PET as input for Hydrus 1-D. Plots show the 500 infiltration (a), surface run-off (b), bottom drainage (c), evapotranspiration (d), evaporation (e), and transpiration (f) at our semi-arid, arid, and hyper-arid locations.

6. The symbol "θs" is used extensively without definition, which may confuse readers unfamiliar with soil moisture notation. Please explicitly define "θs" or use a reader friendly abbreviation.

On first use in the manuscript θs is defined – please see line 419: Fig. 7a-d show histograms of depth-integrated θs (% soil saturation in entire 300 cm of the soil profile). But to increase clarity we can more explicitly define θs in 2.4.3 (Hydrological Model Set Up, Data, and Sensitivity). The revised text could read: Throughout this manuscript we will express soil moisture as a saturation percentage (given the symbol θs), which reflects the proportion of pore spaces that are filled with water relative to if all pore space is saturated (eg 100% means all pore space is filled with water, 0% mean all pores are filled with air). Pore space differs by soil properties hence a relative metric is used to compare our hydrological sites.

7. Duplicate scenario name in Table A1, consider to re-name these scenarios.

Table A1 details the soil parameters used in our Hydrus simulations, to establish whether relative biases in hydrological outcomes when driving Hydrus using CP4A/P25 remained regardless of soil parameters, we utilised three soil parameters sets (as stated in section 2.4.3 and Appendix A).

These parameters were based calculated using the Genuchten-Mualem (Van Genuchten, 1980) equations with soil texture values taken from the iSDAsoil database (Hengl et al, 2021). iSDA provides a lower and upper bound of sand, silt, and clay percentages, our default ('def') parameter set was estimated using the mid-point of these lower and upper bounds. To create our low ('lowK') and high hydraulic conductivity ('highK') soil parameters, we used the lower and upper bound of the sand percentage respectively. We then proportionally adjusted the silt and clay percentage to ensure values equalled 100. We used the same labelling for each site as they follow the same methodology and are designed to be comparable across sites. Of course, soil parameters will differ based on the relative proportion of sand, silt, and clay at each site.

References

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Hijmans, R.J., Cameron, S.E., Parra, J.L., Jones, P.G. and Jarvis, A., 2005. Very high resolution interpolated climate surfaces for global land areas. *International Journal of Climatology*, 25(15), pp.1965-1978.

The Impact of Convection-Permitting Rainfall on the Dryland Water Balance

These are the comments in response to the review by Dr Gómez-Delgado.

We would like to thank Dr Gómez-Delgado for his very detailed and thoughtful review. His comments are very helpful and we are glad to have the opportunity to elaborate on some of the issues raised. Below we respond to each of the comments (in red).

General comments

"the manuscript should provide a more precise narrative regarding its scientific scope and relevance within the broader domain of hydrological sciences" and the "limits of the proposed modelling strategy" should be explicitly outlined.

We acknowledge that one-dimensional point-based hydrological modelling is a simplification of the hydrological system in the Horn of Africa and that it does not capture watershed-scale surface hydrological pathways and hydrogeological processes. However, the focus on 1D processes was a deliberate decision that would most effectively isolate the impact of rainfall characteristics – specifically the representation of convection on rainfall intensity-duration – on vertical vadose zone hydrological partitioning, without the complexity that would be introduced by lateral and non-local processes if modelling was carried out at a basin or regional scale. Runoff generation in drylands is predominantly via the infiltration-excess overland flow mechanism (Hortonian overland flow) which is also a function of rainfall intensity and infiltration rate. However, we exclude the consideration of runoff generation and subsurface lateral flows as they tend to add more water to downslope locations which may then infiltrate or evaporate. We wanted to strip away these added water sources to simply understand the balance between evapotranspiration, soil moisture and deeper drainage. This balance alone, provides valuable insights into water available for plants and groundwater recharge vs that lost back to the atmosphere, purely based on rainfall representation. We can add more text in the revised manuscript (in Section 2.4) to justify the approach and to acknowledge the hydrological processes excluded. We will also caveat our approach by expanding the discussion in lines 563–575 to include literature on surface and subsurface hydrological processes in drylands and their response to rainfall characteristics.

Critically for a dryland context, we will emphasise literature demonstrating the importance of surface flows (runoff) in their contribution to focused recharge in drylands, and the ephemeral flow or pooling that leads to transmission losses is only initiated by high-intensity rainfall events. Given that our results show that CP4A better represents the upper tails of the rainfall distribution (i.e., reduced underestimation of 99th percentiles relative to IMERG), it is reasonable to infer that using CP4A to drive a basin/regional-scale hydrological model could lead to greater non-local recharge and more realistic runoff patterns than P25. We will also strengthen our argument by noting that CP4A-driven Hydrus simulations show higher surface runoff, which at a landscape scale could contribute to lateral redistribution and recharge processes.

We appreciate the reviewer's emphasis on the importance of scaling these insights to broader hydrological systems and believe that strengthening the discussion with additional literature can provide more context around the implications of these results at a landscape scale. But a larger scale spatial analysis is beyond the scope of the current study and would be better achieved in a separate paper.

We hope that these clarifications and our planned revisions address the reviewer's points and reinforce the value of our study as a focused, process-oriented investigation into the hydrological impacts of convective representation in climate models.

Below are our responses to Dr Gómez-Delgado's specific comments :

Specific Comments

In L.14-15 you state “However, rainfall datasets used in hydrological modelling and assessments of water resources are typically derived from climate models.” I suggest removing the word “typically,” considering that many hydrologic modelling applications, not only in research but also for operational purposes (as part of early warning systems, for example), rely on inputs of observed precipitation from weather stations, numerical weather prediction (NWP) models, radar or satellite estimates, or others. Another option might be to say “However, in the absence of precipitation estimates based on observations, rainfall datasets used in hydrological modelling and assessments of water resources are typically derived from climate models.”

We will rewrite the above sentence to clarify that we meant that climate model output is typically used in the context of modelling future projections of water resources.

L.28, 30 & 199: “bottom drainage” is not a universal term in hydrology and is mostly linked to the conceptualization of the modelling process, so to start with, you may want to elaborate a little more on this, for example, by phrasing it here as you did in L.207: "drainage below the soil profile".

For brevity and to maximise understanding to a wide audience, in the abstract we can simply add a bracket to provide more context, so that L.28 reads: “bottom drainage (indicative of potential recharge)”. We will update L199 to read: “We used Hydrus 1-D v4.17 (Šimůnek et al., 2012) to simulate dynamic changes in ….. bottom drainage when forced with each climate model rainfall and PET, where in this context bottom drainage refers to any drainage out of the 1D soil profile (note - while this water could contribute to groundwater recharge it could also still be lost to transpiration).”

L.30-31: when you say “…means surface runoff is up to ten times higher and bottom drainage up to 25 times higher…” are you talking in terms of flow rate or in terms of total depth/volume?

Here we are referring to total cumulative volume over the ten-year simulation. We will update the above to make this clear.

L.31: I would rather say: “…We conclude that dryland vadose zone hydrology is highly sensitive to climate model representation of convection…”

Yes, we agree that this clarification is useful.

L.32-33: when you say “…forcing hydrological model projections with convectional climate models that parameterise the average effects of convection risks underestimating future crop health…” But viewed from another perspective, a convection-permitting model would simulate longer dry periods (increasing water stress) and more intense rainfall events (risk of crop damage or flooding), which could imply worse (but more realistic) crop health compared to the output of the conventional model. If so, wouldn't conventional climate models be mistakenly "more optimistic" and thus overestimate future crop health?

Thank you, this is a really good point that we hadn’t fully considered. Here we have made this assertion based on lower soil moisture and longer periods where acacia shrubs are below the wilting point. But we would agree that crop health is dependent on other factors such as heat stress and potential flooding. To make such a statement we need to conduct specific crop modelling, so we will rewrite the above sentence to: “…forcing hydrological model projections with convectional climate models that parameterise the average effects of convection risks underestimating the soil moisture critical for crops …”. In the discussion where we also consider crop health, we will incorporate your comments provided above.

L.38: I would say “…by limited and highly variable rainfall that varies greatly in time and space, where high temperature…”

We can change the sentence as suggested.

L.39: proposed amendment: “…exceeds the available moisture supply stored in the soil…”

We don’t feel this amendment is necessary, as the atmospheric moisture demand does not just exceed moisture supply in the soils, it can also exceed moisture supply from ephemeral water bodies such as rivers and lakes can often dry out during the dry season.

L.51: proposed amendment: “…drylands cover ~45% of the Earth’s land surface…” (as we know, water covers ~71% of the Earth's total surface)

We will update to "…drylands cover ~45% of the Earth's land surface…"

L.70-71: in this statement: "…with temporal offsets between potential evapotranspiration (PET) and rainfall capable of directly influencing impacting soil moisture…", knowing that PET is a theoretical concept of evaporative demand potential, and that although it experiences temporal variations, it is of a continuous nature, what does a "temporal offset between PET and rainfall" mean? Can you explain this a little more in detail?

While PET does evolve seasonally and temporally its variability is far more limited compared to rainfall. The point we were trying to emphasise here is that PET alone is capable of impacting hydrological metrics relevant to human well-being. For example, Kimutai et al (2025) suggest that PET was a key driver of the recent 2020-2023 multi-season drought. We can simply alter the above statement to read: "In addition to rainfall characteristics, the dryland water balance is also sensitive to atmospheric evaporative demand (PET or potential evapotranspiration), both in how high PET impacts antecedent soil moisture conditions (soils quickly dry out between rainfall events) (Zhang and Shilling, 2006; Nazarieh et al., 2018; Cuthbert et al., 2019; Schoener and Stone, 2019; Schoener, 2021; Boas and Mallants, 2022) and its direct impact on agricultural yields and drought severity (Porporato et al., 2002; Lobell et al., 2011; Vicente-Serrano et al., 2018; Tugwell-Wootton et al., 2020; Kimutai et al., 2023)."

L.79-80: regarding your statement: "...when runoff is significant enough to generate flow in dry channels, leading to localised transmission losses…", I want to note that under the traditional concept of a hydrologic system model, runoff over land or discharge in rivers or canals are considered either variables or outputs. While the analysis and conclusions of this study are not explicitly posed in terms of such a hydrologic system model, they are at least framed at a landscape scale. Therefore, it might be more appropriate to conceptualize runoff or streamflow as variables subject to system transformation functions, rather than as a "transmission losses" which is why I suggest reviewing the terminology used here. So, we could rather say that when runoff is significant enough to generate flow in dry channels, "it runs off at localized points", or something along those lines.

We can rewrite the above to make it clear that "...when rainfall is heavy enough to generate surface runoff, a certain proportion can enter dry river channels and generate locally substantial flows that can lead to localised transmission losses…",

L.198-199: What about infiltration in this list of modelled processes? "We used Hydrus 1-D v4.17 (Šimůnek et al., 2012) to simulate dynamic changes in surface runoff, evaporation, transpiration, soil moisture, and bottom drainage when forced with each climate model rainfall and PET…"

Yes, thank you for spotting this oversight. Will add infiltration to the above.

L.105-106: In relation to this statement: " Furthermore, no studies to date have assessed how model representation of convection can impact the atmospheric variables that control PET. ", I did a very quick search for possible studies addressing this topic, and I found some references that might be relevant (in fact, the first recommended reference includes as first author one of the co-authors of this paper). Such references, along with others worth exploring, could be included here as part of a more detailed bibliographic review:

- Kendon, E. J., Stratton, R. A., Tucker, S. O., Marsham, J. H., Berthou, S., Rowell, D. P., Roberts, N. M., and Finney, D. L.: Convection-permitting climate simulations for South America with the Met Office Unified Model: model evaluation and climate change impacts, Clim. Dynam., 61, 3517–3539, https://doi.org/10.1007/s00382-023-06853-0, 2023.

- Hohenegger, C., Dirmeyer, P. A., D'Andrea, F., and Pritchard, M. S.: Weaker land–atmosphere coupling in global storm-resolving simulation, Proc. Natl. Acad. Sci. USA, 121, e2314265121, https://doi.org/10.1073/pnas.2314265121, 2024.

- Skinner, C. B., Poulsen, C. J., and Eltahir, E. A. B.: How does the explicit treatment of convection alter the precipitation–soil hydrology interaction in the mid-Holocene African Humid Period?, Clim. Past, 19, 637–652, https://doi.org/10.5194/cp-19-637-2023, 2023.

- Omotosho, J. B., and Abiodun, B. J.: Sensitivity of dynamical downscaling seasonal precipitation forecasts to convection and land surface parameterization in a high-resolution regional climate model, Adv. Meteorol., 2019, 6010674, https://doi.org/10.1155/2019/6010674, 2019.

We will clarify what we mean. These studies look at internally simulated evapotranspiration (ET) from the climate model. We computed PET using the Penman-Monteith equation from model outputs of the atmospheric variables needed (2 m zonal (u) wind speed (m s-1), 2 m meridional (v) wind speed, 2 m dew point temperature (K), 2 m air temperature (K), surface net solar radiation (J m-2), surface net thermal radiation (J m-2), atmospheric surface pressure (Pa)) . While internal ET is useful for exploring land-atmosphere interactions — particularly feedbacks between soil moisture and precipitation — it reflects the model's internal assumptions about land surface properties, such as soil type and vegetation. In the case of CP4A, for example, ET is calculated using a uniform sandy soil across the entire domain, which limits its applicability in spatially heterogeneous hydrological studies.

By contrast, externally calculating PET from model-derived atmospheric variables provides a consistent measure of atmospheric evaporative demand, independent of land surface parameterizations. For hydrological purposes where more detailed or locally calibrated soil and vegetation data is available, it is important to use the potential atmospheric evaporative demand computed using atmospheric model outputs. We will revise the manuscript to clarify this distinction and better justify the use of externally computed PET in the context of hydrological modelling. Full details on the rationale behind this approach is given in Appendix A.

L.134-136: could you review the paragraph: "However, it is important to note that CP4A uses a uniform soil map that assumes all soils to be sandy, which risks poor representation of soil moisture – precipitation feedbacks that are critical …". I don't think it's sufficiently clear, as it discusses two ideas (soil type/precipitation feedback) without sufficiently establishing the relationship or causality between them.

We will update the above section to clarify the point, so that it reads: "However, it is important to acknowledge the uncertainties associated with CP4A, one being that the use of a uniform soil map (assumes all soils to be sandy) could impart biases in the spatial pattern of rainfall across the Horn of Africa. As in water limited regions such as the Horn of Africa, soil moisture (partly a function of soil properties) directly regulates evapotranspiration and can contribute to precipitation via moisture recycling (Seneviratne et al, 2010), so a uniform soil map risks poor representation of the soil moisture – precipitation feedbacks that are critical to inducing convective rainfall and a realistic spatial pattern of rainfall (Taylor et al., 2011, 2012; Hsu et al., 2017; Zhou et al., 2021).

L.141-143: two datasets (IMERG, Huffman et al., 2012; and hPET, Singer et al., 2021) are used in this study as references for rainfall and hourly PET. Verifying the high quality of these products, which also have extensive coverage and are openly accessible, makes me wonder about the utility/gain of using models like P25 or CP4A for any water resources application. Would it be possible to delve deeper into this?

While IMERG and hPET are indeed high-quality products that can be used for a wide range of water resources applications, they cannot be used to model future water resources. Studies that consider the impact of future climate change on water resources typically use climate model output. Hence, we want to assess how model choice may impact future projections (solely in terms of rainfall characteristics and PET dynamics). I think a potential issue throughout the manuscript is that we have not made it explicitly clear enough that we are focussing on CP4A/P25 as climate model output is the key resource when considering future climate change impacts on water resources (but comparing them to historical model output as we have observational data to compare it to).We will update the manuscript accordingly to reflect these points.

L.143-147: While recognizing the very high quality of the IMERG product, the fact that very good quality meteorological station records could be available at certain sites makes me believe that it would be more prudent to slightly reword this statement to read: "IMERG utilises space-based radar, passive microwave, infrared, and rain gauge data from the Global Monthly Precipitation Climatology Centre (Huffman et al., 2012)., iIts high spatial (30' mins) and temporal resolution (half-hourly) means it is the most appropriate for evaluating dryland rainfall metrics (Ageet et al., 2022) in the absence of good quality local weather station records in the immediate vicinity where an analysis will be run. However, IMERG is only available from June 2000, so we can only compare CP4A/P25 to 6.5 years of rainfall data…"

We can include this caveat. While there are some (very limited) weather stations available in the region, they are not available at hourly resolution and often have frequent gaps. We also wanted to run the rainfall/PET comparison across the entire Horn of Africa drylands rather than at specific sites.

L.202-205: Most of the studies cited in these lines adopt a simulation strategy similar to the one presented here, in which processes such as surface runoff, horizontal subsurface flow, aquifer recharge and return flows, flash floods and flooding, etc., are considered negligible (although in some cases, observations of groundwater levels at specific sites are used to validate the models). For example, Boas and Mallants (2022) assume runoff and hysteresis are negligible in the context of their study in arid zone environments of central Australia. I believe it would be important to understand the arguments and assumptions employed by these authors when providing a justification for (or stating the limitation of) neglecting all these processes in the present study.

As we mention above, we are not saying that these lateral flows are not important in arid regions. We are deliberately adopting a simple 1D approach to demonstrate the direct impact of rainfall characteristics on the water balance by stripping away the complexities. This comment will be addressed by the general edits to the paper, where we explicitly acknowledge the rationale for the approach and the processes neglected.

L.212-213: in the sentence: "…includes a sink term to account for root water uptake…", it would be worth checking whether it is possible to homogenize the terms root water uptake with transpiration.

This is how transpiration is referred to within Hydrus documentation (root water uptake).I will alter the above to read: "…includes a sink term to account for root water uptake (hereby referred to as transpiration) …". We will then only use transpiration after this point.

L.218: "Hence, we ran four 1-D vadose-zone hydrological simulations along…"

Accepted

L.227: "…To ensure our one-dimensional vadose-zone hydrological simulations isolate…"

Accepted

L.229: in the statement: "…mean annual rainfall and PET was broadly comparable…" what do you mean by "broadly comparable"?

We agree that broadly is too vague without further clarification. We choose our four study sites in grid cells where CP4A/P25 simulated rainfall and PET that was within +/- 35% of each other, as it is very challenging to find grid cells where total rainfall and PET are exactly identical. For example, P25 simulated 15%, 12%, 23%, and 33% higher total annual rainfall at sites HU, SA, A, and HA respectively. And P25 simulated 15%, 3%, 13%, 15% higher total annual PET at sites HU, SA, A, and HA respectively. We can add this additional information to increase clarity on what we mean by 'broadly' in this case.

L.230-232: in the statement: "…this ensures that if fluxes such as soil moisture or bottom drainage are higher when forcing Hydrus with CP4A rainfall, it is reflective of differences in rainfall characteristics rather than simply higher annual totals.", if I understood the exercise correctly, this would only hold as long as the model parameters are kept constant.

If the reviewer means Hydrus parameters, then yes, these are kept constant between the CP4A and P25 runs. What we wanted to emphasize here is that one may expect fluxes such as soil moisture and bottom drainage to broadly follow annual rainfall totals. e.g. if P25 simulates higher total rainfall we might expect higher soil moisture and bottom drainage. Whereas given CP4A simulates lower total rainfall, if driving Hydrus with CP4A results in higher soil moisture and bottom drainage (versus P25) it cannot be argued that this is simply a function of it simulating higher total rainfall. We can alter the above lines to make this clearer.

L.234: the title of Table 1 indicates that the rainfall and PET simulated by CP4A are in bold, but it doesn't indicate where the non-bolded figures come from. Are the non-bolded from P25?

Yes, the non-bolded figures are from P25, we will update the caption and figure to make this clear.

L239: the title of Fig. 1 indicates that the site identifier for the Ethiopian Highlands wetlands is "Site HU," however, the figure itself labels it as "Site H." Can you verify the consistency of the use of "HU" and "H" for this site throughout the document?

Thank you for spotting this oversight. In earlier version our humid site was labelled as H rather than HU. I will ensure all labelling is consistent. HU – Humid, SA – Semi-Arid, A – Arid, and HA – Hyper-Arid.

L.242-244: I would rather say: “Our experimental one-dimensional Hydrus simulations examine how climate model representation of convection can control how moisture propagates vertically through the vadose zone of a particular site hydrological system, rather than aiming to reproduce ‘realistic’ hydrological simulations.”

Yes, we are happy with this rewording and will alter the text accordingly.

L.247-249: please note that the following assumption: “All Hydrus simulations utilised a three-meter soil profile (preliminary simulations suggested minimal water fluxes below this depth at some locations) with a free draining bottom boundary (no interactions between water Table and soil profile above).” is very strong, especially in semi-arid but especially in humid hydrological systems. Again, this should prevent us from overtly generalizing these results to the scale of basin or landscape hydrologic systems.

I believe here you are noting that interactions between the water table and unsaturated zone is important in semi-arid to humid regions and that we should make it explicitly clear that this is why our results should not be overtly generalised to a basin/landscape scale. While we agree with this, we feel that with the additional manuscript revisions detailed above that we will have sufficiently enhanced clarity about the scope of this work and its limitations. But we can provide evidence from the literature that water table depth is generally deep in drylands, including specifically in the Horn of Africa (Bonsor and MacDonald, 2011; Fan et al, 2013).

L.259: I would reintroduce the reference here, this time in the main text body of the manuscript: “…from the iSDAsoil database (iSDA, 2024), which applies…”

Accepted

L.302-303: how could you prove the statement: “CP4A does not simulate the same ‘drizzle effect’ in drylands and offers a clear improvement in the frequency of dryland rainfall…”

We believe this is demonstrated in Figures 3 and 4a-c. The drizzle effect simulated by P25 can be clearly seen in Figure 3 with the peak in rainfall frequency of intensities < 1 mm/hr. We can add an additional graphic/circle/shape and labelling to emphasise this is the drizzle effect. It can also be seen in Figure 4b where P25 simulates most rainfall being delivered via low-intensity rainfall (< 1 mm/hr). Whereas CP4A does not replicate this peak in low-intensity rainfall in Figure 3 and better agrees with IMERG in terms of how much annual rainfall is delivered as drizzle in Figure 4.

L.304-305: the statement: “Using the Kolmogorov-Smirnov (KS) test shows that while there is still a statistically significant difference in the distribution on rainfall relative to IMERG …” is not clear to me. Please elaborate a little more on how the test was used and what the hypotheses were (the difference between which distributions is being tested?)

Thank you for pointing this out. We agree that the original sentence lacked clarity regarding the application of the Kolmogorov–Smirnov (KS) test. We have revised the sentence to explicitly state which distributions are being compared and the nature of the statistical test.

"To quantitatively assess differences in rainfall frequency distributions, we used the Kolmogorov–Smirnov (KS) test with a null hypothesis for each comparison that the modelled distribution of hourly rainfall intensities (based on all hours with rainfall > 0.1 mm/h) is drawn from the same distribution as IMERG. Both P25 and CP4A show statistically significant differences from IMERG ($p < 0.05$), but the KS statistic is markedly lower for CP4A (0.03) than for P25 (0.24), indicating that CP4A more closely reproduces the observed rainfall intensity distribution."

L.319-320: in the statement: “While both climate models replicate the spatial pattern of CDD observed in IMERG (CDD is higher in drylands), the relative biases of P25/CP4A compared to IMERG are opposing…”, please explain better what I have underlined (maybe you could give some examples to better illustrate what you are saying)

We were aiming to emphasise that while both models simulate higher CDD in dryland areas to the east, but their bias relative to IMERG differs. Meaning that while P25 underestimates the number of CDD relative to IMERG, CP4A overestimates the number of CDD. We will update the above to read: “While both climate models replicate the spatial pattern of CDD observed in IMERG, where CDD is higher in drylands (compared to the

Ethiopian Highlands), P25 underestimates CDD length compared to IMERG and CP4A overestimates CDD length….",

L.352: in "…the median value is just 36% higher in CP4A vs P25…", what do you mean by just?

Here we used 'just' as we were comparing 36% to >110% - "Differences between CP4A and P25 are more muted in humid regions, where the median value is just 36% higher in CP4A vs P25 (vs > 110% in drylands), and IQR ranges overlap …." But we agree that using the word 'just' is misleading and will be removed.

L.354: in "…dryland regions (7.1 mm vs 5.8 mm), although this may be related to the use of wet rather than all hours when computing percentiles…" is this comparison referring to CP4A vs P25, or wet vs dryland? What do you mean with "the use of wet"?

We clarify that the comparison in this sentence (7.1 mm vs 5.8 mm) refers to the 99th percentile of rainfall in humid versus dryland regions, respectively, using P25. By "the use of wet," we mean that percentiles were calculated based on all hours with rainfall ≥ 0.1 mm/h, rather than including all hours (both wet and dry). This approach focuses on the distribution of rainfall intensities during actual rain events, rather than across the full time series. We acknowledge that this method can influence the percentile values, especially in models like P25 that overestimate the number of wet hours (particularly in dryland regions, as shown by the drizzle bias in Figure 3). As a result, the 99th percentile may appear lower in drylands — not necessarily because rainfall extremes are weaker, but because the large number of light rain events dilutes the distribution of wet-hour rainfall. We will revise the manuscript to make this distinction clearer.

L.363-364: since you indicate that "…Given we have used the IMERG 95th percentile as our threshold, we are more focused on comparing CP4A and P25 to each other rather than IMERG.", you should perhaps exclude Fig.5 (d) from the mosaic: if it is not directly comparable with (e) and (f) this could cause confusion

Yes, we agree with you. We will swap Figure 5d with Figure B1 which shows the magnitude of the IMERG 95th percentile. This will allow readers to understand the spatial variability in the threshold magnitude we are using for Figures 5e and 5f. We will ensure Figure 5d uses a different colour scheme to 5e and 5f.

L.370: in "…values are 21.5% and 7.8% respectively…", is this comparison referring to CP4A vs P25 or to Ethiopia vs Somalia?

This is comparing CP4A and P25. The values refer to median contributions in arid regions of the Horn of Africa – which is primarily in eastern Ethiopia and Somalia. We can rewrite the above to: "In arid regions (eastern Ethiopia and Somalia) this difference becomes more pronounced in comparison to humid regions, with the median contributions of heavy rainfall being 21.5% and 7.8% in CP4A and P25 respectively.

L.376-378: in "…Bottom Panel - Percentage of mean annual rainfall that falls during 'heavy' rainfall events, in this context we are defining a 'heavy' rainfall event as the 95th percentile of IMERG rainfall (wet hours).", does this description of the Bottom Panel apply to Fig. 5 (d)?

Yes correct. We are defining heavy rainfall as the IMERG 95th percentile. It does apply to Figure 5d. But as we are swapping Figure 5d for Figure B1 this caption will be updated.

L.386: in "…CP4A simulates PET that exceeds 2000 mm a-1 in just 18% of cells…" do you mean that PET exceeds 2000 mm yr-1?

Yes, we do mean yr^{-1}. We can update it from a^{-1} to yr^{-1}. We will ensure this labelling is consistently used throughout the manuscript.

L.421-422: in "…and distributions (Kolmogorov-Smirnov) at all sites, the differences are more pronounced in drylands (KS statistics…", when you say "all sites" does this also include Site HU? When presenting KS statistics, are you reporting the Test Statistic (D), the P values, or something else?

Yes, when stating all sites we are including Site HU. When we are not including Site HU we will use "all dryland sites". And we are only reporting the Test Statistic in the main body of the text, as all P values are statistically significant. We will make it clear throughout the manuscript where KS is used, we are only reporting the Test Statistic.

L.425-426: your statement "…differences at site A are more pronounced if we consider depth-integrated θs at 1.2 mbgl, as below this depth there are minimal fluxes…" is, again, an extremely strong assumption.

This isn't an assumption, it is based on our simulations and is shown in Figure 8c/d. We report depth-integrated θs values up to 1.2 mbgl as below this there are minimal fluxes (very little moisture reaches any deeper). So, reporting the relative differences in depth-integrated θs down to 3.0 mbgl will be lower as both CP4A and P25 simulate minimal moisture fluxes below this depth, so θs below 1.2 mbgl is mainly a function of the initial conditions used.

L.507-509: is the statement "…this metric is not indicative of groundwater recharge, as in reality it is unlikely the water table would be so shallow, and moisture could still be lost to transpiration through deep rooted shrubs (Stone and Kalisz, 1991; Maeght et al., 2013; Shadwell and February 2017)." soundly supported by literature for your study transect, or is it otherwise a risky assumption?

While the water table is shallow enough to be influenced by seasonal rainfall across much of the Horn of Africa, studies that have explicitly estimated water table depth estimate it to generally be far deeper than 3 meters below ground level. Bonsor and MacDonald (2011) model that water table depth in the Horn of Africa exceeds 50 meters across much of the region, with no areas seeing a water table shallower than 7 meters below ground level (Bonsor and MacDonald, 2011). While Fan et al (2013) generally estimates water table depths of 5 to 80 plus meters below ground level, with a small fraction shallower than 2.5 meters below ground level along the Kenyan coast. This is typical of many dryland regions.

There is also robust evidence that dryland acacia shrubs are particularly deep rooted and are capable of extracting water at depths water deeper than 3 meters, they can extract water directly from the water table (Stone and Kalisz, 1991; Maeght et al., 2013; Shadwell and February 2017).

Either way, as we are not explicitly trying to model representative fluxes, even if such an assumption was not fully supported by the literature it would not undermine the purpose of this study or the reporting of bottom drainage.

L.551: the statement "…while most hydrological studies tend not to distinguish between soil evaporation and transpiration…" may be irrelevant, considering that although they are not the majority, there are numerous examples of hydrological model applications (including basin-scale models) in which such a distinction is made.

We will edit the sentence to remove that part and retain "our results show moisture lost to evaporation is far higher when forcing Hydrus with P25, while using CP4A increases the volume of water available for transpiration within the root zone, meaning transpiration is higher and continues longer into the dry season (Folwell et al., 2022)."

L.592: again, the range of relevant hydrological processes can be expanded here: "…as this risks producing misrepresentative projections of metrics such soil moisture, transpiration, overland flow, floods and flash floods, baseflow (groundwater return) and groundwater recharge, which could contribute to sub-optimal decision making around long-term land use or water supply policy…."

We apologise but we are unclear on what the reviewer is noting here, with further clarification we will be more than happy to respond.

L.605-606: Again, considering that several leading processes that can produce large-scale flow transfers are not part of the modelling strategy here, in order to avoid overreaching in this conclusion, I would rather say: "…Our results also show suggest that while PET can influence hydrological vadose-zone outcomes, dryland hydrology appears to be more sensitive to the impact of climate model representation of convection on rainfall…."

Yes, we are happy to add 'appears' to these conclusions.

Technical corrections

Below I recommend technical and typographical corrections to this manuscript, and some typing suggestions.

L.26: "…where at each of our four sites sties Hydrus…"

Thank you for spotting this – will we update the spelling of sites.

L.68: "…the dryland water balance is also sensitive to how synchronicity between rainfall and evaporative demand impacts…"

Thank you for spotting the missing to. This sentence will be also updated based on a previous comment.

L.97-98: "…parameterised climate models, and critically if s dryland water partitioning sensitive to climate model representation of convection (via its impact on rainfall and PET)…."

Will change is to if.

L.124: "…(Stratton et al., 2018; Kendon et al., 2019)…"

Noted, thank you.

L.136-137: "…There are also clearly limitations with results based upon…"

Apologies but we are not sure what the change is here.

L.144: "…(hPET, Singer et al. 2021)…"

Noted, thank you.

L.146: "…its high spatial (30' mins) and temporal resolution (half-hourly) means it is the most appropriate…". Please note that the single prime (') is the SI-accepted symbol for the unit of the minute of arc.

This is an error, it should read its high spatial (0.1 degrees) and temporal resolution (half-hourly).

L.152: "… it is available at a high spatial (0.1 degrees) and…"

Apologies but we are not sure what the change is here.

L.158: "2. 10 m meridional (v) wind speed (m s-1)"

No longer in manuscript – text was altered.

L.166: "…While other Several studies have demonstrated…"

Noted, this will be updated to: "Several studies …"

L.172: "2. Maximum precipitation dry spell length" Missing units!

Noted, will add units (days)

L.173: Missing units!

Noted, will add units (mm/hr)

L.178: "…Furthermore, precipitation dry spell length…"

Noted, will add precipitation.

L.187: "…(hour with rainfall >= 0.1 mm of rainfall)…"

Will alter to "(any hour with >= 0.1 mm of rainfall)

L.260: "…range of fine- and coarse-scale…"

Noted, will alter to simply "fine and coarse-scale"

L.284: "…PET as well as rainfall, so we also in order to assess whether the impact…"

We will alter to "…PET, to ensure we are only capturing the influence of rainfall characteristics on water partitioning, we also forced Hydrus with climate model rainfall but replaced climate model PET with gridded hPET values (see Section 2.2)."

L.289-290: title of Fig. 2 is divided (one part before and the rest after the figure)

Noted, will change (assuming the reviewer is referring to the figure caption). Thank you.

L.290: "…to note that in the above figure above represents both shrubs, maize, and bare soil, represented, whereas only one vegetation type can be modelled."

Will alter to "It is important to note that in the above figure both shrubs, maize, and bare soil are represented, whereas only one vegetation type can be modelled in any one simulation."

L.303: "…of dryland rainfall., iIn both humid and dryland regions CP4A still simulates…"

Will alter to "CP4A does not simulate the same 'drizzle effect' in drylands and offers a clear improvement in the frequency of rainfall, although (in both humid and dryland regions) CP4A simulates fewer rainfall events with an intensity of > 10 mm/hr compared to IMERG.

L.307: "…Kernel density estimate (KDEkde) plots of CP4A…"

Thank you, will alter.

L.333: the legend of the horizontal axes of the three upper figures should be better positioned

Apologies, but we are not sure what needs to be altered here.

L.337-344: All this information could be better presented in a table.

Yes, we agree. Will alter.

L.347: "…climate models underestimate the magnitude of wet extremes events relative to IMERG…"

Yes agree, will alter.

L.355: "…computing percentiles, as P25 dramatically significantly overestimates the frequency of rainfall (most notably in drylands)."

Yes agree, will alter.

L.373: considering the title of Fig. 5 (b), shouldn't the title of Fig. 5 (c) be "CP4A - IMERG"?

Yes, thank you for catching this oversight. It should read CP4A – IMERG.

L.374-378: please check the clarity of the title of Figure 5 and, in general, of the titles of all figures in the manuscript

Noted.

L.393: "…diurnal cycle (Fig. 6d-&f) and replicate the hPET seasonal cycle (Fig. 6e-&g)…"

Noted, will alter.

L.397-399: All this information could be better presented in a table.

Yes, we agree, will make this change.

L.407: "…At each hydrological study site, CP4A and P25 correctly simulated the seasonal cycle of rainfall and tended to produce broadly comparable seasonal totals (Appendix C - Figure C1 1C), although on average P25 delivered higher annual rainfall (Table 2). Both models also produce comparable seasonal PET totals and simulate the same seasonal cycle, although P25 simulates substantially higher PET during JJAS at Site HA (Appendix C – Figure C2 2C) …"

Thank you, will update the tenses.

L.422-425: All this information could be better presented in a table. Also, why are the sites listed in this order: SA, A, HA, and HU? Wouldn't it be easier to interpret the results if they were sorted by aridity level? Same applies for Figure 7 (L.435)

Yes, we would agree with both points raised here. We will make these changes.

L.445-446: "…vegetation health modelling. For example, while differences in median depth integrated θs at Site A is are less than three percentage points, shallower wetting fronts…"

Noted, will alter.

L.459: "…higher soil moisture in Hydrus CP4A simulations was were a function of…"

Noted, will alter.

L.465: "…for longer using CP4A (41% vs 24%), compared to P25. The reduction is especially…"

Noted, will add compared to P25.

L.471: Please note that Figure 9 is never mentioned in the manuscript!

Thank you for capturing this oversight – in lines 445 – 449 where we are referring to Figure 8, we are meant to be referring to Figure 9. This will be updated.

L.472: in "Figure 10 details how water is partitioned between surface runoff, evaporation, transpiration, and bottom drainage…" this procedure is done for which of the study sites?

This is done for all sites but in Figure 10 only the three dryland sites are presented.

L.475-476: Note that what is indicated in this text "Figs. 10a - f shows substantially higher transpiration at Sites SA (2392 mm vs 1724 mm) & A (893 mm vs 694) when using CP4A rainfall…", is something that cannot be seen anywhere in Fig. 10 (the modelling sites are not indicated here)

This can be seen in Figure 10f. The dark blue is the semi-arid site and green is the arid site, with the dashed line being the P25 simulation and solid line being the CP4A simulations. We can update the figure capture and/or the legend to make this clearer.

L.485-486: please note that the green dashed line (a), and the red dashed line (b) cannot be clearly seen in the current version of Fig. 9 (there seems to be a colouring problem in the figure)

Both subplots have two orange dashed lines – the figure caption was not updated and is referring to an old version of the plot. The caption will be updated and colours altered to increase clarity.

L.492: "…despite mean annual lower PET being is lower…"

Noted, will alter.

L.498: Figure 10 presents the results from which of the study sites?

The three dryland sites. This will be made clearer both in the caption and main body of text.

L.505: "…In dryland locations, between 6% and 10% of rainfall is lost to runs off when…"

Noted, will alter.

L.520-522: "…fundamentally opposing manners (light/frequent vs heavy/infrequent), resulting in differing hydrological 1D modelling outcomes when their output is propagated through a vadose-zone hydrological model. This study also verifies that while dryland vadose-zone hydrology is more sensitive to PET than humid regions, differing hydrological outcomes water flows are primarily driven by rainfall…"

Noted, will alter.

L.534: "…Our modelling demonstrates that the vertical water partitioning in drylands is sensitive to…"

Noted, will alter.

L.538: "…produce such differing hydrological vadose-zone flow outcomes when propagated through a simple 1-D model highlights the importance of carefully selecting driving datasets in vadose-zone, or more comprehensive hydrological studies…"

Noted, will alter.

L.544: "…when forcing hydrological models with CPM rainfall (Ascott et al., 2023; Archer et al., 2024),. And while no flood…"

This will be updated to: "Although even in humid regions, studies have found differences in hydrological outcomes between CPMs and parameterised climate models, such as increased surface runoff (Folwell et al., 2022) and higher flood risk when forcing hydrological models with CPM rainfall (Ascott et al., 2023; Archer et al., 2024). No flood risk assessments using CP4A has yet been conducted across the HOA at time of writing, however, it is reasonable to assume that greater sub-daily rainfall extremes (Bethou et al., 2019; Kendon et al., 2019; Finney et al., 2019, 2020) and surface runoff will result in greater flood hazard potential in the HOA when forcing hydrological models with CPM rainfall."

L.590: please add the references in "…Taking a 'storyline' approach (add reference) built around stochastic scenarios…"

Thank you for noticing this absence, we will add the following reference: Shepherd, T.G., Boyd, E., Calel, R.A., Chapman, S.C., Dessai, S., Dima-West, I.M., Fowler, H.J., James, R., Maraun, D., Martius, O. and Senior, C.A., 2018. Storylines: an alternative approach to representing uncertainty in physical aspects of climate change. *Climatic change*, *151*, pp.555-571.

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