Federico Gómez-Delgado
Referee comment on the article:

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

By George Blake, Katerina Michaelides, Elizabeth Kendon, Mark Cuthbert, and Michael Bliss Singer

General comments

This paper examines how climate model representation of convection affects vadose-zone hydrological simulations across an aridity gradient in the Horn of Africa. Using four sites – humid (Ethiopian Highlands), semi-arid (southern Kenya), arid (eastern Ethiopia), and hyper-arid (northern Somalia) – the authors compare the convection-permitting model CP4A with the parameterized model P25, benchmarked against satellite rainfall (IMERG) and potential evapotranspiration (hPET) datasets. One-dimensional hydrological responses are simulated using Hydrus 1-D.

I believe this study contributes to the advancement of hydrological modelling, through an Earth System approach, by demonstrating how effects such as the 'drizzle' bias introduced by some climate models when parameterizing the average effects of convection on precipitation can be effectively corrected (especially for use in drylands), by adopting a convection-permitting approach. This improves vadose-zone hydrology projections in drylands, which is an important step toward generating global change scenarios relevant to protecting lives and livelihoods. These scenarios, within a framework of hydrological modelling at the watershed scale (ideally validated with field observations), would allow for more realistic recommendations for long-term socioeconomic planning.

In my assessment, the climatological and soil physics analyses are methodologically valid, well-supported bibliographically, and adequately discussed in the paper. Furthermore, the article is very well written, with high-quality tables and figures, and solid statistical calculations and reporting to support interesting comparisons and inferences.

I also find it interesting that through the analysis of water movement in unsaturated soils and the quantitative estimation of the main vertical flows (infiltration, evaporation, transpiration and drainage), it is possible to verify the premise that the moisture lost to evaporation is greater if climate models that parameterize convection are used (producing lower intensity rainfall distributed more evenly over time), while a convection-sensitive model simulates greater penetration and retention of water in the root zone, producing greater transpiration, in a sustained manner over time.

Notwithstanding the above, noting that neither the contrast between convection-permitting and convection-blind climate models, nor the employed modelling strategy, are strictly novel (since the use of climate models to provide inputs to 1-D partial hydrological modelling can be found in different studies around the world, and noting also that this paper does not employ primary field/observational data), I consider that the manuscript should provide a more precise narrative regarding its scientific scope and relevance within the broader domain of hydrological sciences, highlighting not only the findings reached (as acknowledged above) but also the limits of the proposed modelling strategy, given that only some of the "containers" and larger-scale processes of the hydrological cycle are analysed here, leaving out others such as watershed-scale surface hydrology and hydrogeological processes. Such a simplification of the hydrological system could only be justified if the hypothesis that such processes are negligible in this region of the world could be demonstrated by extensive bibliographic documentation. This would lead to the strong assumption that vertical transport dominates the hydrological system, and that this condition has low spatial (horizontal) variability in the study area. Otherwise, point modelling of vertical water flows in a few sites would limit the ability to make a sufficiently comprehensive inference about the availability/scarcity of water resources for human and ecosystem use.

In summary, this study offers a well-executed comparison of climate models coupled with a robust soil physics analysis, which is however tested through discrete modelling of a vertical profile at only four sites along a long subcontinental aridity gradient. It focuses on routing water dynamics in the unsaturated zone of the soil (modelled assuming a constant uptake layer of 3 meters along the entire transect, and a simplified routine for estimating surface runoff), without considering horizontal movements of water over the soil in the form of Hortonian or saturation-excess overland flow, return flow, and surface channel flow (surface hydrology), nor shallow subsurface movement as inter- or preferential flow (subsurface hydrology), nor multidirectional interaction or exchange with the saturated zone and aquifer systems (groundwater hydrology), which, even in arid to semi-arid climates, can provide baseflow in perennial or quasi-perennial rivers.

Considering the above, I would suggest the following:

1. Reformulate some statements in the analysis of results and conclusions (in this review, I propose some wording for your consideration)

2. To expand the interesting discussion in lines 563-575 by adding more references (without this becoming an exhaustive meta-analysis) that delve deeper, through observational and/or modelling studies, into the possible effect on water resource distribution caused by watershed-based surface hydrological processes in this vast study region (ephemeral and permanent fluvial hydrology – especially in the presence of perennial or quasi-perennial rivers, as well as flood and flash flood events) and hydrogeological processes (aquifer recharge and baseflow through groundwater return in the form of gravity, fault-controlled, or capillary rise/artesian flow, among others), elaborating on how the assumptions of the approach used here can be improved in order to more comprehensively generalize the modelling results and conclusions to the landscape scale, and making recommendations for future research. This may also contribute to a more multidisciplinary approach and balance (in favour of hydrology) the significant analytical weight the article places on topics of climatology, soil physics, and agronomy, as seen in the discussions and the solid bibliographical support provided, for example, in lines 525-553, 559-561, and 576-590. I believe the exercise proposed here may also enrich not only the title and abstract but especially the conclusions of the article.

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.”

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".

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?

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

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?

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

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

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)

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?

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.

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…”

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.

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.

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?

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).~~,~~ ~~i~~Its 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…”

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.

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.

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

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

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

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.

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?

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?

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.”

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.

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…"

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…"

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?)

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)

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

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"?

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

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

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)?

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?

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?

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.

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?

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.

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…."

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 ~~is~~ appears to be more sensitive to the impact of climate model representation of convection on rainfall…."

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…"

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

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)…."

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

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

L.144: "…(hPET, Singer et al. 2021)…"

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.

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

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

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

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

L.173: Missing units!

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

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

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

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

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

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."

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

L.307: "…Kernel density estimate (KDE~~kde~~) plots of CP4A…"

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

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

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

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

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

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

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

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 delivereds~~s~~ 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) …"

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)

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…"

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

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

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

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?

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)

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)

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

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

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

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…"

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

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…"

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

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