Response to comments made by Reviewer 2 on a manuscript entitled 'Changes in mean evapotranspiration dominate groundwater recharge in semi-arid regions'

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Reviewer 2

General comments

General comment 1: Dear Editor, I have carefully read the submitted manuscript, which aims to evaluate the impact of the variation in mean and standard deviation of the statistical distributions of rainfall (R) and potential evapotranspiration (ETref) on groundwater recharge (GR). I am in trouble, because the objective of the study seemed quite interesting and totally in line with the HESS journal topics, but I note that behind the desire to internationalize the study with global datasets (a condition rightly much appreciated by the journal) there are major uncertainties on the definition of the parameters (GR).

Reply to general comment 1: *Our precise definition of groundwater recharge is specified in the second sentence of the introduction. Due to the lack of information regarding the variability of the water table depth in all the locations considered, we are limited in considering the amount of water infiltrating deep enough so that it is not lost to evaporation, transpiration, or runoff. The depth of 5m that we considered satisfies these requirements, given the sparse vegetation in those locations. The main difference between our definition and the definition suggested by the reviewer is the travel time. The travel time differences can induce a lag between the infiltration to the depth we considered and the actual depth of the water table. However, for the long-term simulations we considered, and given the 200 years of spin-up we used to ensure independence from the initial conditions, the accumulated infiltration flux is not expected to change much by the different travel times.*

We revised the second sentence of the introduction as follows: R is the amount of water infiltrating the soil deep enough such that it is not lost to evaporation, transpiration, or runoff. Note that this definition is not the same as the definition of some authors, which define it as the amount of water replenishing the aquifer (Healy and Scanlon, 2010) (the main difference is the travel time)(lines 11-15).

General comment 2: on the relevance of the simulations to the reality of the physical data (ETref and R variability)

Reply to general comment 2: *Common climate projections suggest that changes to the potential ET are mostly limited to the range of 10% Basso et al. (2021); Guo et al. (2023). Changes to the precipitations are much larger. To illustrate the latter we present in the figure below the predicted changes for a location in Australia. The different models suggest different changes spanning the range we considered. From a theoretical point of view, it is interesting to study the effects of small and large changes in climate conditions as we did.*

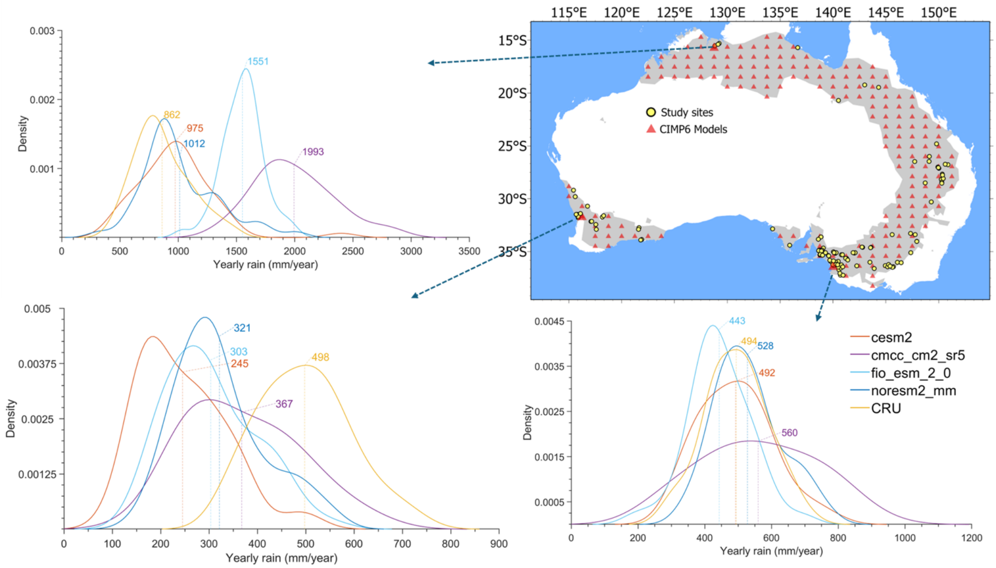

Figure 1. Probability density functions (PDF) of the yearly rainfall (mm/year) at three selected locations in Australia. The PDF of the CRU data represents the yearly rain statistics from 1958 to 2014. The yearly rain PDFs of four CIMP6 models (*cesm2*, *cmcc_cm2_sr5*, *fio_esm_2_0*, *noresm2_mm*) are accompanied with the CRU data to display future projections for yearly rain (2050-2099).

General comment 3: and on the initial hypotheses (infiltration and soil heterogeneity considered only for the first cm of topsoil).

Reply to general comment 3: *We considered the top heterogeneity to evaluate its effect on evaporation, runoff, and deep drainage. We found that there were no significant changes in deep drainage rate (our definition of groundwater recharge) when accounting for the top soil heterogeneity (relative to considering a uniform column with the hydraulic parameters of the top (0–5cm) soil layer). This is valid for the many semi-arid locations we considered where the travel time (to the depth where the infiltration rate was recorded in the simulations) is much shorter than the duration of the simulation.*

We added a reference to the work of Or et al. (2013) which supports our assumption (line 98)

Reply to general comment 4: I don't want to sound too critical, but I think there are some gaps in this study, which need to be fixed at least by defining the limits more and also recalibrating the text and parameter definition. For example, Regarding the GR parameter there is a big divergence between what I mean (and also other authors) and what I read here. I suggest the authors to keep this reference in mind: https://doi.org/10.1017/CBO9780511780745.

Reply to general comment 4: *The R was defined in the original text. Following the comment, we added an explanation that this definition differs from the definition used by some other authors. We also added a citation of the book listed by the reviewer. We added the following text: 'Note that this definition is not the same as the definition of some authors, which define it as the amount of water replenishing the aquifer (Healy and Scanlon, 2010) (the main difference is the travel time).'(lines 11-15).*

General comment 5: The last analytical part which considers the derivative of the groundwater recharge (GR) and Rainfall (R) ratio (GR/R) with respect to ET is not correct, in my opinion. Actual evapotranspiration depends both on rainfall and potential evapotranspiration and cannot be considered as independent. Based on that part, conclusion section could be misleading too.

Reply to general comment 5: *Indeed, we find that in many locations there is no linear relationship between the actual evapotranspiration (E_a) and the potential evapotranspiration (E_p). In the revised text, we explain that under the assumption of $E_a = f(E_p/P)$, with f being any function, the effects of changes in E_p or in P are expected to be the same as long as the ratio, E_p/P, remains constant (which is the kind of changes we considered in our paper). We believe that the observed difference in the responses to changes in E_p and changes in P is due to the fact that the E_a depends on the water content of the top soil layer. The water content of the top soil layer is a function of the specific temporal fluctuations of the precipitation and the potential evapotranspiration as well as of the soil hydraulic parameters.*
We revised the paragraph to reflect the changes outlined above (lines 238-242).

General comment 6: I would like the authors to explain more in depth the differences between this new study and the previous one (also published on HESS, https://doi.org/10.5194/hess-27-289-2023).

Reply to general comment 6: *Our previous paper that was published in HESS* ***'The effects of rain and evapotranspiration statistics on groundwater recharge estimations for semi-arid environments'*** *addressed the issue of correct representation of observation data in synthesized stochastic time series of rain and potential evapotranspiration. It presented a novel method to generate daily precipitation (P) and E_p time series according to the monthly empirical distributions of climate records. The observation data includes many different aspects of the statistics, and when a synthetic series is generated, some of the characteristics are often lost. Therefore, we examined the sensitivity of R fluxes calculations to the correction methods and pointed out the best method for adequate simulations of groundwater recharge.*
In this paper, we address a totally different question. We analyze the effects of changes in the statistics of E_p and P on R. The method suggested in the previous paper is used in the current paper to generate the stochastic series, and then the effects of changes in the mean, standard deviation, and frequency of extreme events on R are examined. To the best of our knowledge, this question has not been addressed in previous studies.
A few studies focused on groundwater recharge under projected climate conditions (based on specific GCM simulations) and reached contradicting conclusions regarding future groundwater recharge. Our study explains the effects of different changes

to the statistics and, thereby, the origin of different outcomes while using different GCM simulations.

In our current study, we attempted to address this complexity by separately considering different changes in climate statistics. Namely, we consider separate changes in the mean, standard deviation (STD), and extreme events of E_p and P statistics (relative to the measured statistics) in multiple locations across the globe. We demonstrate that the effect of changes in the average E_p on R is substantially greater than changes in the average P and changes in the STD of the E_p and the P. Changes in the statistics of extreme rainfall events have a much greater effect than extreme E_p events on R. In general, we found nonlinear responses of the R to changes in the climate variable statistics. We suggest that the differences in climate statistics may explain the contradictory results of previous R studies.

General comment 7: It would be desirable for the authors to make the raw datasets and part of the simulations available and public as supplementary material. Surely, it would also be helpful for us reviewers to confirm or resolve some doubts.

Reply to general comment 7: *All data is available in Hydroshare at the following link: http://www.hydroshare.org/resource/5a28b74c55c74a7e9ad2b59a0a5d9ab3*

Specific comments

Specific comment 1: Line 3: indices, parameters or "characteristics" of the statistical distribution?

Reply to specific comment 1: *We mean characteristics, just as written. The characteristics include changes to the mean, variance, frequency of extreme events, and other changes. It is not a simple change in parameters of a known analytical form of the E_p distribution.*

Specific comment 2: Line 3: Why do you rename Potential Evapotranspiration as ETref? Usually it is widely accepted the abbreviation PET. I suggest you to use it to be clearly understood by all the readers. ETref is usually the reference EvapoTranspiration, related to specific crop, isn't it? Could you explain better the differences (maybe in the introduction section)?

Reply to specific comment 2: *We accept the comment and changed the notation.*

In the revised version E_p represents the potential evapotranspiration. E_p is explicitly specified in section 2.2, "The daily E_p values were calculated according to the Penman-Monteith method using climate variables such as mean, maximum, and minimum temperatures, vapor pressure, cloud cover, and wind speed (Harris et al., 2014)."

Specific comment 3: Line 5: standard deviation (STD). The first time you present an abbreviation, even if widely known, please always report the extended word before.

Reply to specific comment 3: *We accept the comment and changed the text accordingly (Line XXX).*

Specific comment 4: Lines 7-8: Actually, this seems to be normal, isn't it? The extreme statistics is mainly used for rainfall variability analyses.

Reply to specific comment 4: *We found that changes in the mean E_p have a greater impact on R/P than changes in the mean precipitation. Therefore, we find it interesting that for changes in the extreme value statistics, the impact of changes in E_p is weaker than the impact of changes in P.*

Specific comment 5: Lines 11-12: some suggestions on other recent studies focusing on groundwater recharge and related water management issues:

In particular:

A recent book describing methods for assessing groundwater recharge (physical methods, inverse methods, lumped models, water budgets etc..) https://doi.org/10.1017/CBO9780511780745

Recent studies based on GR estimation using different methods:

https://doi.org/10.5194/hess-25-2931-2021

https://www.sciencedirect.com/science/article/pii/S2352801X18302662

https://link.springer.com/article/10.1007/s40710-024-00688-5

https://www.sciencedirect.com/science/article/pii/S0022169417307436

https://www.sciencedirect.com/science/article/pii/S2214581821001464

Reply to specific comment 5: *We thank the reviewer for specifying these references.*

The references are cited in the revised manuscript (lines 12-13).

Specific comment 6: Lines 12-13: "Groundwater recharge is defined in a general sense as the volume or process of downward flow of water reaching the water table, forming an addition to the groundwater reservoir" (Hartmann A., The Hydrology of Groundwater Systems - From Recharge to Discharge, in Encyclopedia of Inland Waters (Second Edition), 2022)

I think this definition is more correct. In my opinion, one of the potential weaknesses of this study is assessing the GR without adequately considering the geological characteristic of the unsaturated zone in the study areas. GR is the water volume/flow that actually reaches the water table. Is your control volume limited to the topsoil? What do you mean with "deep enough"?

Reply to specific comment 6: *Please see our response to the reviewer's general comment 4. By "deep enough," we mean a depth from which there is no water loss for ET. Specifically, in this study, this depth was set to 5m below the surface. The difference between our definition and other definitions was specifically mentioned at the beginning of the Introduction section.*

Specific comment 7: Lines 16: Other suggestions on references:

https://www.sciencedirect.com/science/article/pii/S0013935123023368

https://doi.org/10.3390/w15142610

https://doi.org/10.7343/as-2019-373

https://link.springer.com/article/10.1007/s10113-023-02114-2

Reply to specific comment 7: *We added the suggested references to the revised manuscript (lines 19-20).*

Specific comment 8: Line 34: 'potential ET (ETref)'

Reply to specific comment 8: *See our response to the reviewer's specific comment 2. ETref was replaced by E_p*

Specific comment 9: Lines 42-43: I also read your previous paper (https://doi.org/10.5194/hess-27-289-2023, 2023.). Can you explain what novelty does this new study bring? What are the main differences?

Reply to specific comment 9: *Please see our response to the reviewer's general comment 6. For convenience, we respond here again. Our previous paper that was published in HESS* ***'The effects of rain and evapotranspiration statistics on groundwater recharge estimations for semi-arid environments'*** *addressed the issue of correct representation of observation data in synthesized stochastic time series of rain and potential evapotranspiration. It presented a novel method to generate daily precipitation (P) and E_p time series according to the monthly empirical distributions of climate records. The observation data includes many different aspects of the statistics, and when a synthetic series is generated, some of the characteristics are often lost. Therefore, we examined the sensitivity of R fluxes calculations to the correction methods and pointed out the best method for adequate simulations of groundwater recharge.*

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A few studies focused on groundwater recharge under projected climate conditions (based on specific GCM simulations) and reached contradicting conclusions regarding future groundwater recharge. Our study explains the effects of different changes to the statistics and, thereby, the origin of different outcomes while using different GCM simulations.

In our current study, we attempted to address this complexity by separately considering different changes in climate statistics. Namely, we consider separate changes in the mean, standard deviation (STD), and extreme events of E_p and P statistics (relative to the measured statistics) in multiple locations across the globe. We demonstrate that the effect of changes in the average E_p on R is substantially greater than changes in the average P and changes in the STD of the E_p and the P. Changes in the statistics of extreme rainfall events have a much greater effect than extreme E_p events on R. In general, we found nonlinear responses of the R to changes in the climate variable statistics. We suggest that the differences in climate statistics may explain the contradictory results of previous R studies.

Specific comment 10: Line 44: '... assuming that this flux reaches the water table' In my opinion, this could be a too much strong condition, difficult to be respected in some specific hydrogeological frameworks.

Reply to specific comment 10: *For the hydrological framework considered in this manuscript, this assumption is more than reasonable because we found no evidence for water loss to the atmosphere from depths of a few tens of cm and deeper.*

Specific comment 11: Lines 57-58: why? I mean, did you choose them specifically?.

Reply to specific comment 11: *We did choose the locations specifically. First, we only considered locations with semi-arid or arid conditions. Secondly, to avoid the need for specific vegetation water uptake characteristics that we do not have we focus on locations where the water uptake by vegetation may be neglected.*

Specific comment 12: Line 84: again, this is another very strong assumption not applicable to many contexts

Reply to specific comment 12: *Please see our response to the reviewer's specific comment and general comments regarding our definition of R (previously GR). We found no evidence of water loss from depths greater than 30cm (Or et al., 2013). We added this reference to the revised text*

Specific comment 13: Line 85: I take a look at SI. The unit measure of each parameter is missing. Please add it.

Reply to specific comment 13: *We thank the reviewer for the comment.*
The units were added to the datasets now available in the following link:
http://www.hydroshare.org/resource/5a28b74c55c74a7e9ad2b59a0a5d9ab3.

Specific comment 14: Lines 96-97: what does it mean observed GR/Rain ratio. Do you measure GR?

Reply to specific comment 14: *For clarity, we changed the wording from observed to reported. As mentioned in the manuscript, all reported values were estimated using ground-based methods such as chloride mass balance and water isotopes. See the beginning of section 2.1*

Specific comment 15: Lines 114-120: I think all this procedure is important and should be described better. Some sentences are misleading. Authors can also take into account explaining the procedures for the mean and st.dev modification with a flowchart to be added in this part of the text (i.e. a figure).

Reply to specific comment 15: *We thank the reviewer for the comment.*
As suggested, a flowchart was created and added to the supporting information.

Specific comment 16: caption of Figure 1: I still don't understand properly what you mean with measured GR. Is the measured value coming from the water content/flux detected by satellite observation at 5 m depth? Moreover, you previously affirmed (pag 3) that for the water flow simulation you consider only three layers of the topsoil up to 30 cm. Please try to clearly define limits and boundaries for both measured and calculated GR (what about a table?)

Reply to specific comment 16: *See reply to specific comment 14, we have changed the wording from 'measured' to 'reported'.*

Specific comment 17: Line 134: This second part is clearer because of the presence of different formulas defining MTS. Anyway I still suggest to consider a flowchart to let the reader better understand the procedure.

Reply to specific comment 17: *See our reply to specific comment 15.*

Specific comment 18: Line 161-162: Really? How much does the average annual temperature decrease to have a 33% decrease in evapotranspiration?

Reply to specific comment 18: *See the response to general comment 2. Common climate projections suggest that changes to the potential ET are mostly limited to the range of 10% Basso et al. (2021); Guo et al. (2023). Changes to the precipitations are much larger. To illustrate the latter we present in the figure below the predicted changes for a location in Australia. The different models suggest different changes spanning the range we considered. From a theoretical point of view, it is interesting to study the effects of small and large changes in climate conditions as we did.*

Specific comment 19: Lines 189-190: what about the rainfall intensity? If you consider extreme events you should take into account it, keeping in mind that an increase in intensity also means a lower capacity of the soil to let water infiltrate

Reply to specific comment 19: *In this work, we considered daily values of rain and E_p; obviously, when the daily rain amount changes, so does the intensity. Characteristics of the daily distribution were not considered in the current study.*

Specific comment 20: Lines 212-223:I don't appreciate this part. I find these considerations extremely forced. Where are the soil properties in here? Moreover, this is an analytical guise which, however, is not adherent to the reality of measured data in the field. And this is because the initial definition is, in itself, incorrect. Groundwater recharge is not what you define in (6), where with P-ET you are representing the net or effective rainfall, i.e. the quantity of water available for infiltration, net of evaporation and transpiration processes that occur. After this, soil and rock mass properties have key role in defining runoff and infiltration. I understand and appreciate the effort and will to try to give a global picture of such an important process for water management, but at this point in the manuscript, I am convinced that there are major gaps from a conceptual point of view in this study. The hydrogeological characteristics (larger orders of magnitude, not only the topsoil in the first meter) of the outcropping formations in the locations are not considered. Rainfall and evapotranspiration are considered to have potentially similar variability in your simulations. However, average rainfall, within its seasonality, clearly has greater variability on a monthly and annual scale when intense drought or storm events occur. On the contrary, evapotranspiration can certainly have a growing role considering the average increase in global temperatures, but it always depends on a parameter (T) that has a lower variability and this can be found in the historical series of any air thermometer in the world.

Reply to specific comment 20: *See reply to general comment 5. We found that in many locations, there is no linear relationship between the actual evapotranspiration (E_a) and the potential evapotranspiration (E_p). In the revised text, we explain that under the assumption of $E_a = f(E_p/P)$, with f being any function, the effects of changes in E_p or in P are expected to be the same as long as the ratio, E_p/P, remains constant. We believe that the observed difference in the responses to changes in E_p and changes in P is due to the fact that the E_a depends on the water content of the top soil layer. The water content of the top soil layer is a function of the specific temporal fluctuations of the precipitation and the potential evapotranspiration as well as of the soil hydraulic parameters.*

Specific comment 21: Lines 222-223: In the last sentence, assuming that ET is proportional to ETref is misleading too, considering what you state after. The subsequent implication is then referred to rainfall (weaker sensitivity of GR to R changes). Actually ET depends both on ETref and R! If ETref > R, the ET = R. So, at the time scale considered, the R acts as a cutoff for ET if ETref is higher than R (nothing can evapotranspirate if there is no rainfall). For this reason I cannot accept this analytical part and I think it's better to delete it. Because actually ET is a function of R too (ET=ET(R)) and not a indipendent variable.

Just to give an exemple: annual estimation of ET by Turc empirical formula (the easiest way to find it), needs the cumulative annual rainfall to be considered as input.

Reply to specific comment 21: *See the response to the comment above and to general comment 5.*

Specific comment 22: Lines 236-238: This distinction is very important and probably should have been stated much earlier, not at the end of the manuscript. Karst settings are charcterized by focused GR and host some of the most important groundwater resources in the world.

Reply to specific comment 22: *We accept the comment and added a sentence emphasizing this distinction in the last part of the introduction.*
In areas where R occurs predominantly through focused processes, additional factors dominate the overall recharge and are beyond the scope of this study (lines 49-50).

Specific comment 23: Line 242: I would like to receive your datasets, if it is possible. Thank you.

Reply to specific comment 23: *See reply to general comment 7. All the data related to this work is uploaded to the HydroShare and is available at the following link:http://www.hydroshare.org/resource/5a28b74c55c74a7e9ad2b59a0a5d9ab3.*

References

Basso, B., Martinez-Feria, R. A., Rill, L., and Ritchie, J. T.: Contrasting long-term temperature trends reveal minor changes in projected potential evapotranspiration in the US Midwest, Nature Communications, 12, 1476, 2021.

Guo, D., Olesen, J. E., Manevski, K., Pullens, J. W. M., Li, A., and Liu, E.: Change Trend and Attribution Analysis of Reference Evapotranspiration under Climate Change in the Northern China, Agronomy, 13, https://doi.org/10.3390/agronomy13123036, 2023.

Healy, R. W. and Scanlon, B. R.: Estimating Groundwater Recharge, Cambridge University Press, 2010.

Or, D., Lehmann, P., Shahraeeni, E., and Shokri, N.: Advances in soil evaporation physics—A review, Vadose Zone Journal, 12, 1–16, 2013.