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Comment on hess-2021-230

Anonymous Referee #1

Referee comment on "Evaporation enhancement drives the European water-budget deficit during multi-year droughts" by Christian Massari et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-230-RC1, 2021

Summary

In this study, Massari et al identify a shift in the rainfall-runoff relationship during multi-year droughts compared to normal years. In addition, they attribute this shift to an enhancement in the evaporation, especially in dry, warm, and water limited environments. The manuscript is well structured, and the conclusions are based on sufficient analysis of results. However, I have major concerns about the novelty of the study, the importance of the study's results, and some methodological details. I elaborate on this below, including some minor comments.

Major Comments

- Novelty: In my opinion the results and the conclusions drawn are fairly logical and obvious. For example, precipitation droughts do not occur in isolation. They are generally accompanied by enhanced temperature (not all the time). In that scenario, an increased evaporation is expected, and it follows that runoff would reduce compared to normal years. The same applies for the results presented for energy-limited and water-limited environments. I request the authors to specifically elaborate on the novelty of their findings and explain how these findings move forward catchment hydrology.
- Implications: I am not very sure that the change in the rainfall-runoff relationship (even the maximum of -40%) is very significant in terms of absolute terms. This is because, it is fair to assume that during multi-year droughts, especially in arid watersheds, the amount of rainfall is reduced significantly (upto -185% according to the findings of this study). Therefore, how does this translate to any significant change in runoff? It would be useful if the authors provide an idea of the change in runoff in absolute terms and not only percentages, as I feel that the implication of the change in rainfall-runoff relationship may not be significant at all.
- Conclusions: Likewise, the two main conclusions regarding the need for a) better calibration of rainfall-runoff models and b) better representation of different processes

in the conceptual models, are not very novel. It is well known that conceptual models do not account for non-stationarity very well, and the response of the watersheds during multi-year droughts is a specific case of non-stationarity. Similarly, how feasible is the inclusion of complex coevolution mechanisms in simple rainfall-runoff models. I suggest the authors focus on the implications of the study beyond improvement of conceptual models.

- Methodology: There are several unclear methodological decisions which needs to be clarified
  - Was the effect of increasing temperature trends in these watersheds taken into account. Although it may not have an effect in a 3-year period, it may have an effect on the rainfall-runoff relationship in multi-year droughts in the early years (1980s) compared to the later years (2000s). Was this explored?
  - The authors need to better justify the use of only 3-years for multi-year drought definition. How was this number arrived at? In addition, how was the threshold of SPI > 0.15 selected?
  - Why was the representative annual precipitation estimated as the mean of average and minimum precipitation and not just average annual precipitation?
  - I do not understand why two different precipitation datasets were used. The provided justification does not explain possible discrepancy between drought definition and annual precip which may lead to differing anomalies. Why should they be independent?
  - In Figure 5, was the water/energy limited watersheds classified based on only the drought period? It may be that a generally energy-limited watershed may transition to a water-limited watershed during multi-year droughts.
  - I am not sure if discussing the importance of basin storage sustaining ET during droughts is relevant to the discussion here as it focuses on rainfall-runoff relationship. Is the argument that enhanced ET may actually be due to storage changes and not because more precipitation is being converted to ET? It would be helpful if the authors can elaborate.

Minor Comments

- There are several grammatical errors in the manuscript. I request the authors to correct them. For example Line 10, “less than what expected” should be “less than what is expected”. Line 120, “...it was not used to define the droughts…” should actually be “...it was used to define the droughts…”.
- Line 207. I do not understand how the fact that having only 2 basins showing a positive shift is an indicator of the “high control” of the experiment or high quality. I do not follow the reasoning here.