

Comment on hess-2021-646

Anonymous Referee #2

Referee comment on "Response of active catchment water storage capacity to a prolonged meteorological drought and asymptotic climate variation" by Jing Tian et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-646-RC2>, 2022

In this manuscript, the authors try to identify the temporal changes in the water storage capacity of the catchments in Australia due to prolonged meteorological droughts and highlight the factors responsible for causing such changes.

Based on my initial assessment of just the title, abstract and the research questions proposed in the Introduction, I found this research quite relevant for the larger hydrological and ecohydrological community exploring ecosystem response to droughts, changes to above- and below-ground water resources and predicting such changes under future climate change. However, after reading the manuscript, I had several major concerns.

Major comments:

- I was confused by the term 'catchment water storage capacity'. However, after reading the manuscript, I thought the concept explored in this study was similar to 'root zone storage capacity'. It would make sense to use terms already well established and accepted in the scientific community rather than introducing new terms. Authors should cite appropriate literature in the manuscript related to earlier and recent work about root zone storage capacity exploring their response under changing hydroclimate.
- The authors have used the word 'asymptotic' (i.e., the influence of droughts/climate change on catchment's water storage capacity seasonally) quite frequently (every 3-4 lines in the Introduction) as a central research gap that is addressed in this manuscript. However, after reading the whole manuscript, I am still unsure how it was addressed. This is because authors haven't clarified how the trends observed in catchments due to prolonged meteorological droughts have permeated to show changes in seasonal hydrological trends of the catchments.
- The authors had used the change in root zone storage capacity (i.e., catchment water storage capacity) before and after the prolonged droughts and correlated it with catchment and climate characteristics to infer relevant factors influencing the

catchments. But does a high correlation mean causation as well? The authors have neither provided a concrete justification about probable catchment dynamics in response to the droughts nor cited a single literature in the 'Results and discussion', which makes it difficult to understand their reasoning. Furthermore, the characteristics of soil and forest cover are rarely discussed. Although these factors play a major role in influencing/partitioning storage and runoff of the catchments.

- The manuscript's language needs to be improved considerably for it to be considered for acceptance in HESS. My main concerns are related to improper paragraph structure, grammatical inconsistencies (e.g., use of was, is and has been in the first paragraph of Introduction) and repetitions throughout the manuscript (e.g., Line 292-296 already mentioned in Methods). Although I have not included all inconsistencies that I found in the manuscript in this comment, the authors should check for them carefully.

Specific comments:

- The catchment's response to prolonged droughts would have already covered any seasonal response. Do authors think that using the word "asymptotic" adds any value to the analysis presented?
- Line 55-62: Authors briefly discuss the strengths and weaknesses of statistical techniques but don't discuss the limitation of hydrological models. Is there none in the context of modeling, parameterization, etc.)?
- Line 69-73: Is climate change not considered under changing environment? Are the authors claiming the hydrological models do not consider climate variability on catchments? Please provide appropriate citations to this statement.
- Authors have referred to a publication 'Pan et al. (2020)' as 'our previous study', and highlighted this study as the extension of the study mentioned above, addressing previous studies' time-based research gaps. I would recommend authors to dedicate one paragraph to 'Pan et al. (2020)' to briefly discuss the necessary context, rather than discussing it in bits and pieces (Line 77-82, 103-105, etc.)
- Study area and Section 4.1 can be combined as 'Study area and catchment demographic' as it adds no novelty to the research gaps.
- Line 129-130: '...which had a significant impact on the stability of local ecosystems, and the development of society, economy and politics.' Add references which highlight this.
- Remove 116-119. Since the sections and subsections are already there, this paragraph is unnecessary.
- Check the equation in section 3.2.2 Criteria (2).
- Line 253: NSE is abbreviated before it is defined. Authors can probably keep the unabbreviated words for sub-headings.
- Line 261: I still do not understand Criteria (3): Robustness requirements of the results. Clarify.
- Line 321: Avoid starting a sentence with a number.
- Is the word 'significant' used to refer to statistical significance, i.e., $p < 0.05$? If so, make this clear in the manuscript and caption of the figures.
- Line 334-337: Cite the appropriate table. The authors have also not adequately cited tables and figures in appropriate places. Check.
- Line 407-408: '.....while those of catchments with significantly downward changes in $\delta Q_{1/4}$ are 391.9 and 422 days, respectively.' What does 'upward/downward change' mean? And why is the response time less than the upward change?
- 'Results and discussion' section needs to be structured properly. For example: Line 461-463: 'Since no strong correlation between the amplitude and a single factor is

found, therefore we speculate that the potential change of the variation range of the CWSC is the result of the combination of various catchment features and climate factors.' Discuss what those feedbacks could have been. So far the 'Results and discussion' sections seem like just 'Results' without any citation justifying the claims made by the authors.

- Line 439-445: 'On the whole, we can get the conclusion that: catchments with small area\ low elevation\ small slope range\ large forest coverage and AWHC soil may change more significantly than catchments with opposite characteristics. It is likely that the resilience of catchments with small area\ low elevation\ small slope range\ large forest coverage and high AWHC soil is poor, and which result in an easy change in CWSC of these catchments after the interference of meteorological drought.' Why would the resilience of a catchment with a low elevation and high forest cover be poor? What is the reasoning here? Cite appropriately as well.
- Line 487-493: 'In general, soil and forest percentage are the most related variables to the mean value. The water holding capacity of various soil types is different as the dissimilarity of void and adhesion in different soil types, which directly affects the ability of the catchment to absorb and store water, and then affects the CWSC of the catchment. Furthermore, the coverage of multiple forest percentage would affect the water holding capacity and water assumption ability, resulting the potential changes in the CWSC.' I would have preferred more soil and forest cover discussion on the catchment's water storage capacity.
- I had concerns about some of the words that are used in the manuscript: '....different climate-changing patterns' (Line 34), 'stronger robustness' (Line 170), 'lumped conceptual' (Line 178), 'differentiated soil composition' (Line 273), 'remarkable increasing trend' (Line 404), 'remarkable convergence patterns' (Line 355), and many more.