

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2  
<https://doi.org/10.5194/hess-2021-5-RC2>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.



## Comment on hess-2021-5

Anonymous Referee #2

---

Referee comment on "Drought-induced non-stationarity in the rainfall-runoff relationship invalidates the role of control catchment at the Red Hill paired-catchment experimental site" by Yunfan Zhang et al., Hydrol. Earth Syst. Sci. Discuss.,  
<https://doi.org/10.5194/hess-2021-5-RC2>, 2021

---

This manuscript investigates the discrepancy of estimated vegetation influence on catchment run-off based on three established methods for one paired catchments site in Australia. The particular pair had been the only one showing strong discrepancies in an earlier study (Zhao et al., 2010, cited in the manuscript). The three methods are: (a) „paired-catchment method“ comparing control and treatment catchment runoff (b) „time-trend analysis method“, comparing rainfall-runoff dynamics before and after the treatment only in the treated catchment (c) „sensitivity-based method“, comparing observed and modeled „climate affected only“ runoff in the treated catchment. The modeled runoff is derived from un-calibrated prediction of precipitation partitioning into runoff and evapotranspiration based on the aridity index and is supposed to mimic the variation in catchment runoff related to climate variation alone. Only method (a) uses both catchments, while (b) and (c) only rely on the treated catchment (although awkwardly almost no data are presented on that one, see below). The main attention of the manuscript lies in investigation of the suitability of the paired catchment approach. The authors re-visit the catchments using a longer time series and also applying a lumped hydrological model. They identify changes in the overall rainfall-runoff-relation that coincide with a prolonged drought period in Australia, and conclude that the main assumption of the paired catchment approach (e.g. „stationarity“) is violated. When using modeled runoff for the control catchment based on the pre-drought parameters (e.g. imposing stationarity) instead of the real measurements, the results match better with two other alternative approaches. The authors conclude that non-stationarity was the main reason for the discrepancy between the three methods used to evaluate the effect of land cover change on catchment runoff.

The question how land-use change affects catchment run-off is certainly an important one given the ongoing anthropogenic changes of landscapes. Contributions to improving methods for detecting those effects are of interest for the readership of HESS. Overall the

paper is well written and mostly well-organized. I have some serious concerns about the conclusions drawn from the analysis as I think they are flawed. Unfortunately, I also have major concerns about the motivation and presented material. My concerns are severe and would require a substantial revision of the basic approach, such that I recommend rejection at this point. I believe investigation could gain in merit, if argumentation and analysis were rendered consistent between the two catchments, methods were compared in a balanced fashion and more focus was placed on underlying processes.

### **Serious concerns**

- I find that the study's main conclusion is based on a flawed assumption that the rainfall-runoff dynamics in the control catchment was affected by the drought, while the one in the treatment catchment was not. This assumption is arbitrary, not stated and its implications not discussed. All potential causes mentioned in the discussion for the changing rainfall-runoff relation in the control catchment (change of catchment capacity due to lower groundwater levels, increases in potential evaporation), would equally apply to the directly neighboring treatment catchment. The assumption that this is not the case is without basis. Indeed, the results of the three applied methods matching after the „correction“ may be by accident.
- The study states that „stationarity“ is a main assumption for method (a). Indeed, the basic assumption is that catchments would respond very similarly, if they had the same vegetation. Strictly speaking, this does not imply that their rainfall-runoff behaviors cannot change over time.
- Following up on (1) the study only investigates the violation of assumptions in one of the three methods, while the others are taken as approximately correct. Alternative hypotheses are not discussed. Method (a) has great potential to covering the effect of climate variation properly. Its main assumption is that the catchments would respond similarly, if they had the same vegetation. Any differences in catchment behavior (e.g. catchment storage capacity) that are the result of the treatment are arguably still a vegetation effect. However, it would be interesting to investigate whether the catchments generally behaved differently at drier conditions which were not observed during the „pre-treatment“ period. Method (b) assumes that the parameters of the treated catchment do not change over time. This assumption was clearly violated in the control catchment as the authors show at length, while the treated catchment was not investigated for it. From their own investigation, the authors should be highly skeptical that the assumption can be made. Method (c) ignores catchment processes, and assumes that rainfall partitioning only depends on aridity. This approach assumes that changes in catchment storage can be ignored (stationarity), which contradicts the conclusions made for the control catchment, e.g. changed catchment capacity due to lower groundwater tables during the drought. In other words, to me method (a) appears the most robust compared to the other two, given the presented results. The comparison of the methods requires more balanced assessment and more scrutiny. Currently the manuscript is inconsistent.

### **Major concerns**

- The motivation is very vague and so is the conclusion. It states very generally that the community can learn from assessing the discrepancies in this one catchment pair, but the derived conclusions are not novel. Already the study of Zhao et al., (2010, cited in the manuscript) came to the same conclusion. So it is unclear how this analysis adds more knowledge.
- The entire investigation is awkwardly focussed only on the control catchment. For example, of the eight figures dealing with catchment runoff only one also shows the treated catchment, the others only the control catchment. As such, the study boils down to investigating the effect of the drought on catchment runoff - but if this was the focus of the study, the methods much more than now should try to look into the catchment processes.

I am not including more detailed comments, since I believe the manuscript requires substantial changes and further comments would become obsolete in the next version of the manuscript.