

Hydrol. Earth Syst. Sci. Discuss., referee comment RC3  
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## Comment on hess-2022-166

Anonymous Referee #3

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Referee comment on "Does non-stationarity induced by multiyear drought invalidate the paired-catchment method?" by Yunfan Zhang et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-166-RC3>, 2022

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This is an interesting paper on an important topic. In fact it could be argued the issues discussed in this paper are absolutely fundamental in hydrology. This is particularly true in light of the way "data" is used uncritically in many studies.

The paper is generally well written and structured, although there is a need for a more careful read-through as there is some awkward syntax and grammar (particularly in the Discussion) where the quality of the writing seems to wander a little. My main questions relate to the data set used and the essential paradigm of the PCM. The Redhill site did not have a calibration period. Although the authors suggest that the first period of treatment may be thought of as non-treated in that the trees were very small and not high water using, I feel the implications of this may be important. This then connects to the PCM paradigm; that is, that the length of calibration or the approach developing the calibrations in theory should account for the type of non-stationarity that is discussed (ie. drought). Putting this another way, how can we decide what is non-stationarity and what is variability? This is particularly germane to Australian hydrology where we experience significant variability. I am not suggesting climates are stationary, but disentangling non-stationarity from variability with a relatively short data period (in climate terms) is a question.

The calibration period issue is a vexed one as there is no longer an appetite by funding bodies to set up a paired-catchment experiment and then wait for a lengthy period before anything happens. Bren and Lane (2014, JH 519) explored this issue and proposed a method using daily flows that rather obviously increases the number of data points. Somewhat surprisingly the analysis showed that good calibrations (Nash-Sutcliffe  $E = 0.8$ ) with 100 days of data, and very little improvement after 3 years. Apologies for the treatise, but I wonder if this approach might be useful in thinking about the PCM. That is, if such an analysis was performed and compared with the other analyses it might be very useful. Data could be randomly pulled out of the Kileys Run data. At the very least it should be discussed.

I have some more specific comments as follows:

Line 40 - there are more updated references for the research in Australian catchment behaviour that are relevant (eg Petersen et al, 2021, Science 372)

L46. I think this statement about PCM and non-stationarity requires more justification. How does it not deal with the issue given that is the paradigm of PCM?

L 65+ Both TTM and SBM require lengthy records; is this an issue with these analyses at Redhill? There may be an argument to see the drought as a plus in terms of a record with wet/dry periods.

Lane et al. 2005 (JH) used FDCs that included Redhill – might be worth including these results as a comparison from a different method. This paper also has some estimates of time to equilibrium.

L 69 – syntax not great “issues about this hypothesis” could be improved.

L 107 – should be double mass curves, FDCs etc. There are quite a few examples of this, need a careful read.

L 168- See earlier general discussion. It does trouble me that a site with no calibration is used for this study. In addition, the vegetation effect is dynamic; growing from seedlings to (presumably, given there are no growth data) a closed canopy. I do wonder if this really is the best data set for such a study, or what might be gained from using more data sets.

Figure 3 is a great figure!

Table 2 – total flow changes would be useful. They appear later in the text but I think having totals in the table make it easier to evaluate the methods.

This also brings up another point that I don't think has been discussed properly. Qclim is conceptualised as the climate effect, encompassing wet and dry and mean climate inputs. I am not sure there is adequate discussion of how this does not deal with the climate issue as formulated.

5.2.1 – this paragraph brings up the interesting point (that is the subject of the Saft/Peterson/Fowler etc studies); is it the climate that is non-stationary or is the processes (obviously driven by the climate).

The Paragraph around Line 375 needs some rewriting, the syntax is jarring. For example "the" control..

L 388 "Because Saft.." this is a poor sentence

L 399 "pines" should not be italicised *P.radiata* would be