Short comment by S Mylevaganam

We thank you for your review and constructive comments. We hope that you find our responses satisfactory to the points that you make. Suggested response or actions are written in italics.

1) As per the authors, research using the paired catchment approach to assess change in hydrological "droughts" due to land use and other human activities remains limited(see P-3 LN-29). However, as per the authors, research works using the paired catchment approach to assess "low flows" due to land use and other human activities are found in the literature(see P-3 LN-24:29). What are low flows? What are droughts? I think, the distinction between low flows and droughts needs to be explicitly mentioned in the manuscript to assert the authors' statement that the research using the paired catchment approach to assess change in hydrological "droughts" due to land use and other human activities remains limited. Moreover, what is implied by "limited"? Should the authors include/cite the research works using the paired catchment approach to assess change in hydrological "droughts" due to land use and other human activities found in the literature?

We thank the reviewer for pointing out this for clarification. We define droughts within the paper at the end of the first paragraph (P2 LN8-10): "Here we use the definition of drought as a deficit in available water from 'normal' conditions (Wilhite & Glantz, 1985; Tallaksen & Van Lanen, 2004), with a focus on hydrological drought, which considers the deficit in streamflow." However we will include a definition of drought and low flows and add a brief description of how these two terms differ to make it clearer to the reader. We will use the definition by Smakhtin (2001): "minimum flow in a river during the dry periods of the year…. Low flows is a seasonal phenomenon, and an integral component of a flow regime of any river".

By "limited" we mean that there are actually no published articles specifically assessing effects of human activities on hydrological droughts using paired catchments in a rigorous manner. Tijdeman et al. (2018) use the concept of comparing droughts occurring in two similar catchments (e.g. Supplementary figure S4). They do not use a specific framework as we present here, but they explicitly state the potential use for it: "An alternative approach is based on the principles of paired catchment analysis, a concept that has been a foundation of process hydrology. Typically, a paired catchment study compares the flow regimes of nearby catchments with similar physical characteristics. The approach has been applied in numerous iconic experimental studies to investigate land use impacts on river flow (e.g. review of Brown et al., 2005). However, the paired catchment concept can also be used to study human influences on streamflow, using existing gauging station networks, if appropriate "donor" natural catchments with similar flow regimes can be found for "target" catchments with known influences (as conducted in the case of urbanisation effects on floods; Prosdocimi et al., 2015)." (Tijdeman et al., 2018, p.1053). We will change the statement in the manuscript to demonstrate that there are no published articles specifically assessing human activities on hydrological droughts using paired catchments.

2) As per the authors, drought analysis is normally conducted on the daily or monthly time step (see P-5 LN-23). Therefore, the authors use "monthly" data for the paired catchment analysis, even though the selected catchments are provided with data on daily time step? In the current version of the manuscript, the authors fail to state the reason for using "monthly" data for the paired

catchment analysis. Is it the methodology (i.e., paired catchment) that is chosen in the proposed approach forces the authors to use the monthly data instead of daily data? Should the authors show the characteristics of the droughts within a month for the selected catchments? What is the minimum duration (in days) of the drought observed in the selected catchments? What is the minimum frequency (in days) of the drought observed in the selected catchments? Would not these details justify the applicability of selecting the monthly time step to conduct the drought analysis?

As stated in the manuscript (p.5 L23), drought analysis is commonly done on the monthly time step, therefore it is deemed justifiable. Droughts are generally long phenomena with timescales of weeks to years, with drought-generating processes and associated impacts going beyond a daily time step. Therefore common time steps of weeks (few cases, and when used, a minimum of 15 days is often implemented), months, seasons, or multiple years. [P5 L22-25: "Although the paired catchment approach is most commonly used with annual data, it has also been used with monthly data (Bari et al., 1996; Brown et al., 2005). Drought analysis is normally conducted on the daily or monthly time step (e.g. Hisdal et al., 2004; Fleig et al., 2006; Van Loon & Van Lanen, 2012). Therefore, here we use monthly data for the paired catchment analysis."]

Furthermore, monthly data was used to avoid a need for pooling of drought events, and to allow minor droughts of <1 month to be dropped. This is stated in the Methods sub-section 2.5 Drought analysis P6 L6-7: "Drought events of only 1 month have been excluded from the analysis process so that drought events are longer than the time step of the threshold." We will look to state this earlier on in the Methods sub-section 2.4 Data requirements if this is deemed a more appropriate place to highlight this. Ultimately as long as the same time resolution and threshold is used for both catchments then the comparison remains valid.

3) The use of paired catchment in the proposed approach is very much subjective. Is it possible to define hydrologically similar catchments without considering the landuse pattern and spatial orientation of the landuse? Would it be possible to completely define hydrologically similar catchments using precipitation, PET, and geology? The landuse pattern than the spatial orientation of the landuse may dramatically alter the flow pattern even for the same precipitation, PET, and geology?

Land use data is much harder to access and to include because of the usually higher spatial variability than precipitation, PET and geology, therefore it could be a very difficult criterion to stick to. This said, we do consider land use (see reply to comment 2 of reviewer#1), we just do not include it as our fundamentals. We will indeed include this catchment characteristic into Table 1 to show the importance.

With regards to information about the land use of the pairs, for the UK, the Dun is one of the UK benchmark catchments (Harrigan et al., 2018), therefore it is considered as a "natural" catchment by the Centre for Hydrology and Ecology and we are confident in using it as a comparison pair. Land uses are similar for both catchments according to the NRFA.

With the Australian catchments – we find that only the human-influenced catchment, Cox, has agriculture associated to it (Green et al., 2011): "The main land use in this region is a combination of grazing and dryland cropping with agriculture". Whereas, the benchmark catchment used, Cockburn, is described as "the area is mainly used for grazing with some dryland cropping and horticulture" (Green et al., 2011).

This information will be included in the revised manuscript to show more information about land use.

4) In Table 3, for Dun (natural catchment), how did the authors compute the total number of months in drought and the frequency? Since the authors have analyzed from 1973 to 2013, there are 12*41(=492) monthly flow records. In other words, setting 80% as the threshold level yields around 98 months in drought (see Table 3). What is the definition of frequency? Is it meant for the return period of the drought? What is the unit of frequency?

Because minor drought events of <1 month have been dropped, it might result in slightly less drought events being identified as expected. However, our results show that the Dun has 93 months in drought, which is extremely close to this expected 98 months, and the differences seen between the two numbers is purely because of the dropping of minor drought events.

Frequency = number of drought events identified in the time period. This definition will be included in the revised version of the manuscript. It is uncommon to determine return periods and it is not a drought characteristic we choose to focus on. There is no unit for frequency as it is just a value, a number of drought events identified. We used the theory of runs to define events: see manuscript p. 5 Lines 27-28: "For each catchment, drought events and their metrics were identified with the commonly used drought analysis method, the threshold level method (Yevjevich, 1967; Tallaksen & Van Lanen, 2004; Van Loon, 2015)".

5) In the current version of the manuscript, the authors evaluate the groundwater abstraction on the droughts. With groundwater abstraction, it is expected to have a depleted groundwater table. Consequently, as per Darcy's law, since one of the drivers (i.e., hydraulic gradient) is changed due to groundwater abstraction, a possible alteration on the base flow (see the reported BFI values in UK) that defines the low flow conditions in most of the rivers is expected. Would it be possible for the authors to show the rainfall pattern (daily) in the natural catchment and human altered catchment for some of the drought periods in UK?

Indeed groundwater abstraction leads to lower groundwater heads, decreased groundwater gradients, lower groundwater inflow/base flow into the rivers, and hence more extreme drought in streamflow. We could indeed show some of the rainfall patterns for the drought periods if this would add to the paper. It is important to note that we do not take an event-by-event analysis though (as it is a challenge to pair drought events in different catchments), but look at overall averages, maximums and totals of drought duration and deficit over the entire time series to avoid this level of discretion.

6) As per the authors, the paired catchment approach has been a predominant method for detecting the effects of disturbance on catchment scale hydrology (see P-3 LN-14). To support this statement, the authors cite Zégre et al., 2010. Considering the fact that the paired catchment approach has been used for many decades (see P-3 LN-16), starting as early as 1920 (see P-3 LN-19), would it possible for the authors to state the reason for citing Zégre et al., 2010 to support the statement(see P-3 LN-14)

Thanks for this comment. In our revised version we will change this citation to one of an earlier time period, that of Bates (1921), which is the first paired-catchment study.

7) Should the section 2.1 be re-written? The third paragraph of section 2.1 is about the method (i.e., paired catchment approach). In other words, the third paragraph of section 2.1 defines the method. However, the first and the second paragraphs of section 2.1 briefly summarize the crux of some of the previous research works using the paired catchment approach found in the literature. Does it make sense to mention the previous research works using the paired catchment approach at first and then define the method? From the reader's point of view, the third paragraph of section 2.1 should come first and then the previous research works using the paired catchment approach found in the literature.

This restructuring of section 2.1 will be done in the revised version.

8) The section that is devoted for discussion (i.e., section 4.0) is structured using some of the limitations of the approach presented in the manuscript. What is expected in the section (i.e., discussion) by a reader of the manuscript is left missing in the current version of the manuscript.

The focus of the paper is the methodology and the two case study examples are just for illustrations (see L20-27 in Abstract: "Here we outline the methodological approach to quantifying this human influence on hydrological droughts and the requirements in catchment selection, as well as showcase the application using some example results from contrasting case studies in the UK and Australia with catchments heavily influenced by groundwater abstraction. Whilst the selection of the paired catchments must be done with rigorous criteria, this approach overcomes the impacts of climate variability in pre- and post-disturbance studies, and avoids assumptions considered when partly or fully relying on simulation modelling. We discuss important considerations for a successful analysis. This is the first application of this approach to quantify the human influence on hydrological droughts, demonstrating the use of this tool to study hydrology in our human-dominated world.")

We believe that because the aim of the paper is to introduce a simple but novel framework for quantifying the human influence on hydrological droughts, this is the important aspect to be discussed in this section, rather than the results of the two case studies which are used to demonstrate the method. Discussing the limitations and the possible ways forward for future research with this method is useful for future research, given its scalability to be performed in other locations.

9) From the reader' point of view, the numbering of the sections is misleading. In section 2.2, the authors introduce the approach used in the current version of the manuscript to address the intended tasks (i.e., determination of drought metrics). However, in section 2.2, the authors ends the paragraph (see P-4 LN-17: here we outline the important elements for the "approach") to form the subsections that need to be listed under section 2.2. Should the sections 2.3, 2.4, and 2.5 be numbered as section 2.2.1, 2.2.2, and 2.2.3?

We agree that the numbering here could be improved. We propose that section 2.1 Paired catchment analysis becomes its own section after the introduction. That way 2.2 onwards becomes the method and the relevant information for the analysis.

Alternatively, we can indeed change sections 2.3, 2.4 and 2.5 to be 2.2.1, 2.2.2 and 2.2.3.

10) On P-3(see LN-22), what is meant by “see review of Brown et al., 2005”? Is it a review about Brown et al., 2015? On P-3(see LN-24), what is meant by “some studies included low flows”?

We are refereeing to the published review of paired catchments written by Brown et al. (2005) titled ‘A review of paired catchment studies for determining changes in water yield resulting from alterations in vegetation’, however we can change the phrasing of this to be clearer: “see review paper of paired catchments by Brown et al., 2005”

Most paired catchment studies look at annual changes following treatment, and often focus on water yield and high flows, not on low flows or streamflow during droughts. Therefore we specifically point out that some look at low flows, but not the majority of the published literature. P3 L24-30 is an entire paragraph which summarises and points to the work which does use paired catchments to look at low flows: “In the 1990s, some studies included low flows (e.g. Keppeler & Ziemer, 1990; Scott & Smith, 1997). It has been found that clearcut harvesting can lead to an increase in low flows (Keppeler & Ziemer, 1990), while land cover conversion from grasslands, shrublands and croplands to forests can cause a decrease of low flows (Scott & Smith, 1997; Farley et al., 2005). Previous low flow works also suggested that in watersheds located in dry regions streams were likely to completely dry up following afforestation, and that the streamflow regime in those watersheds would change from perennial to intermittent (Farley et al., 2005; Jackson et al., 2009). However research using the paired catchment approach to assess change in hydrological droughts due to land use and other human activities remains limited.”

Minor Comments:

a) The authors’ marriage to some of the words (e.g., “here” we suggest, “here” we outline, we “here” give, “here” we use, “here” the 80% percentile, “here” we have analyzed, “here” we present, “here” we have demonstrated, “here” we focused, “here” the focus was, and “here” we show) is a little off from what is expected in a scientific research paper.

In our revised version we will endeavour to address this repetition.

b) In Table 1, the title of the second column (i.e., “assessment for similarity”) needs to be changed to reflect the cell values.

This will be changed to ‘Metric assessed’.

c) In Table 2, the widths of the columns (e.g., column-7) need to be adjusted to fit the content.

This will be rotated landscape and column widths expanded to fit the content.

d) In Table 2, the gage numbers for the selected catchments in Australia are missing (see Figure 2 and Table 2).

Thank you for pointing this out, these will be added to the revised version.

e) In Figure 3, the label for the x-axis should be “year”?

X axis label will be changed to Year rather than Dates in the revised version.

f) In Figure 4, the label for the x-axis should be “year”?

X axis label will be changed to Year rather than Dates in the revised version.

References

Bates, C. G. 1921. First results in the streamflow experiment, Wagon Wheel Gap, Colorado. Journal of Forestry, 19(4): 402-408.

Green, D., Petrovic, J., Moss, P. & Burrell, M., 2011. Water resources and management overview: Namoi catchment, Sydney: NSW Office of Water.

Smakhtin, V. U. 2001. Low flow hydrology: a review. Journal of Hydrology, 240: 147-186.