

## ***Interactive comment on “Using paired catchments to quantify the human influence on hydrological droughts” by Sally Rangecroft et al.***

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The undesired consequences of extreme events (e.g., floods and droughts) in water sciences have forced many funding agencies and researchers to seek innovative methodologies to evaluate extreme events. In this manuscript, using the concept of paired catchments, the authors present an approach to evaluate drought conditions by means of drought metrics on drought frequency, drought duration, and deficit volume. The approach is demonstrated for a few selected catchments in UK and Australia.

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

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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?

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?

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?

4) In Table 3, for Dun (natural catchment), how did the authors compute the total num-

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ber of months in drought and the frequency? Since the authors have analyzed from 1973 to 2013, there are  $12 \times 41 (=492)$  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?

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?

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 be possible for the authors to state the reason for citing Zégre et al., 2010 to support the statement (see P-3 LN-14)?

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.

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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.

9) From the reader's 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 end 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?

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"?

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. b) In Table 1, the title of the second column (i.e., "assessment for similarity") needs to be changed to reflect the cell values. c) In Table 2, the widths of the columns (e.g., column-7) need to be adjusted 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). e) In Figure 3, the label for the x-axis should be "year"? f) In Figure 4, the label for the x-axis should be "year"?

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