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Discussions

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

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This is a conceptual paper which aims to set out a methodology for characterising the human influence on streamflow drought via paired catchment analysis. Quite a substantial part of the paper reads as more of an extended perspective/opinion piece, but articulates a new method and applies it to two example catchments in UK and Australia.

This is a generally well written paper on an important topic and the paper has significant potential to make a very worthwhile contribution to the 'drought in the Anthropocene' debate. Paired catchment analysis is a staple of experimental hydrology, but is much harder to do in 'real world' examples when it is not possible to control all variables

except the main intervention of interest, and even the latter may be poorly understood. This paper tries to establish a method to allow this to be done even when there are no pre-disturbance periods, and without recourse to models. In this regard, it is potentially a very useful advance arising from a simple yet potentially very effective idea.

However, at present I don't think it can deliver on this promise as the methodology seems to have a flaw which I have outlined below in some detail. The method is predicated on the similarity of the donor natural catchment/target influenced catchment, but in the UK example at least, the catchments are not similar enough, very likely leading to over-estimation of the anthropogenic effect. I feel major revisions are needed to convincingly demonstrate the method, through modifying it to allow for some tolerance in the donor/target relationship, verifying the method using independent abstraction data, or benchmarking it against other methods.

Major Comment: impact of catchment (dis)similarity on the proposed method

There is a reason that paired catchment analysis using existing, gauged catchments is hard, and is rarely published (for drought or other topics): it is difficult to find suitable pairs. Even when catchments are in principle very similar (geology, rainfall etc), the concept of 'uniqueness of place' (as discussed at length in various papers by Keith Beven) is a major obstacle.

Despite this obstacle, data transfer is still possible as evident from the abundance of regionalisation methodologies available (a prior PUB decade, indeed!). However, transferring a threshold directly from one catchment (reading off the Q80 flow value from the natural catchment and applying it to the influenced one) to another seems like a potentially dangerous business.

Regionalisation methods use FDC statistics (e.g. transferring Q95 from donor to target catchments) for scaling purposes, but they tend to have lots of mechanisms built in to accommodate the fact that catchment similarity is imperfect: e.g. they are supported by multivariate regression based on catchment characteristics, and/or benefit from pooling groups which place less emphasis on the relationship between any one donor and the target site. Finally, the uncertainties in regionalisation techniques are widely acknowledged. This might not be a problem if one is just trying to estimate flows at an ungauged site, and can report uncertainties; but in the present method any biases arising from the data transfer could be very misleading.

Put simply, the method applied in this paper can only work if the donor's natural flow regimes is near-identical to the 'theoretical natural' flow regime of the target site (i.e. what the regime would be in the 'world that might have been' with no human interventions). Any deviation between these regimes will be interpreted as anthropogenic; when it could just be due to variations between two catchments that appear quite similar but are in fact different.

This becomes problematic when one looks in detail at a catchment pair. I have investigated this for the UK pair as I am more familiar with UK hydrology, and have not commented on the Australian example. The Dun and Kennet are very similar indeed in terms of rainfall and geology, and make a good starting choice of study catchments. However, we can still see that the flow regimes are quite different. See the attached graph showing the two series scaled as runoff in mm, to account for the different catchment areas, as in the paper. The Kennet has a greater range in flows, with higher high flows and lower low flows (notwithstanding the abstraction effect). The Dun is more muted. The catchments are different in terms of runoff response/catchment function, despite their similar rainfall.

I'm not entirely sure of why the two catchments differ in terms of response, but it is likely that hydrogeology is a major factor. As acknowledged by the authors, in such chalk dominated catchments, the size/nature of the contributing catchment can be very different to the topographic catchment. Moreover, the geology of the chalk is heterogeneous and very complex.

The different regimes will have a significant bearing on the derived Q80. The net result

is (likely) that the natural Q80 transferred from the Dun is a biased estimator of the 'theoretical natural' flow of the Kennett. Given the more limited range of the Dun flows, my guess is the Q80 of the Dun will be somewhat higher, leading to inflated values for the deviations that are used to infer aggravated drought due to human effects.

As a result, I do not think the authors can claim 'attribution', and the claims of the paper need to be reconsidered. Even if the catchments were very close matches hydrologically, this would be 'weak' attribution. The method can be useful as a screening approach, but there remains a need to seek information on the influences in order to fully attribute. Note that this is an important difference in the urbanisation paper (Prosdocimi et al. 2015) or in the classic experimental catchments, which all incorporate some data on the intervention in question into the analysis. (e.g. the land cover data used by Prosdocimi et al.). The Kennett is very well known to experience major abstractions, which have been non-stationary over the series. But more could be done to follow this study up – there is anecdotal information on abstractions in various grey literature sources I found online (below).

In terms of results, the figures quoted in Table 3 seem very large. Given the nature of the debate around abstraction impacts, these figures could be quite contentious, as the Kennet is something of a poster child in the debate around sustainable abstraction, and the authors should do more to ensure they are meaningful. I'm not sure the method gives me enough confidence to get behind these figures. Other work suggests impacts in summer low flows of 10% - 40% (in major droughts). In addition, this paper uses the Kennet at Marlborough which is upstream of the single biggest abstraction at Axford which I imagine features in the 10 – 40% statistics quoted elsewhere.

(e.g. http://assets.wwf.org.uk/downloads/riverside_tales.pdf) http://assets.wwf.org.uk/downloads/case_study_kennet_final.pdf

Given these concerns, the authors could consider some approaches to bolster the method and provide verification – e.g., how this method performs relative to other approaches or modifications, e.g. detecting deviations based on rainfall (as in Tijdeman et al. 2018) and PE. In general the approach could be strengthened considerably by taking a more water balance approach, as done in the classic paired experimental catchment studies, and also in the study of Prosdocimi which incorporates climate variables to account for any confounding effects. But I'm not sure this would help as I think the differences in catchment function are possibly hydrogeological; this could be explored in more detail.

Finally, to really demonstrate the success of the method, it would be nice to have some independent verification of the suggested impacts. I appreciate access of abstraction data is not straightforward for the UK, but might be possible for one catchment, at least for derived data on impacts rather than particular abstractions. I would suggest some dialogue with the Environment Agency would be worthwhile, as there seem to be naturalised data (by decomposition and/or modelling) available for the Kennett for various past studies. http://www.mariusdroughtproject.org/wp-content/uploads/2017/01/MaRIUS_Kennet_ECG_Report_Jan2017.pdf

Specific Comments

A technical matter: In Figure 3, I'm surprised to see so little of the flows being below the threshold. It does not look like 20% of the flows are below the threshold to me – can the authors please check?

P2, L21. Another approach is using deviations in the P-Q relationship, e.g. Tijdeman et al. 2018.

P2, Intro. The paper would do well to refer to the expansive literature in hydroecology which also tackles a similar problem of estimating 'natural' flows for sites, against which impacted flows can be compared. The classic papers of Brian Richter are a good start, and I'm fairly sure methods have been proposed to transfer natural flow percentiles (but using a whole FDC approach; try the DHRAM work by Andrew Black, Dundee as a start). Another area where this is done routinely is through the LowFlows software

product, a regionalisation product which estimates natural and disturbed FDCs ar any site. It's not drought specific, but definitely has a very similar aim.

P4, Sect. 2.3. Given the concerns raised about the UK catchments, this section needs to be reconsidered.

P6, L2. The 80th percentile is not what is being used here. This paper uses the 20th percentile, or, as is most commonly referred to in hydrology, Q80: the 80% non-exceedance threshold from the flow duration curve.

Discussion: is generally very insightful but definitely needs reconsidering in light of catchment selection issues, and claims about attribution need to be moderated.

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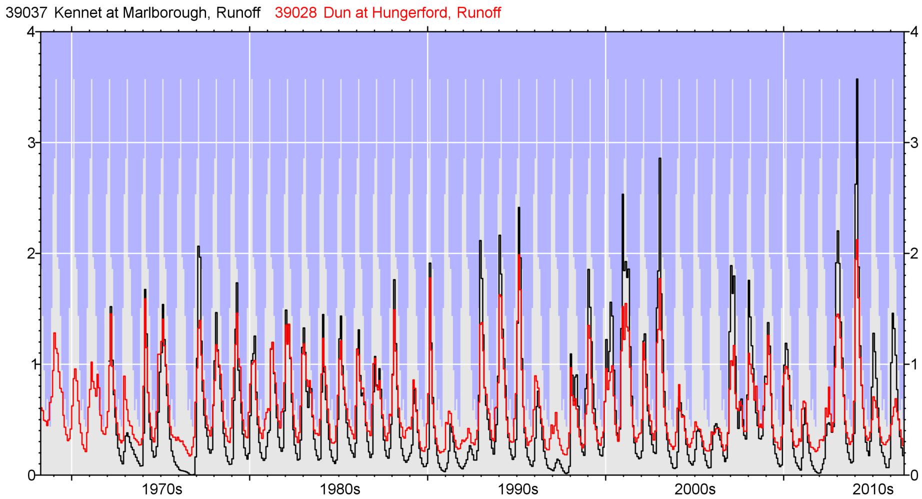
39037 Kennet at Marlborough, Runoff
39028 Dun at Hungerford, Runoff
0
1
2
3
4
1970s
1980s
1990s
2000s
2010s

Fig. 1.