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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Positive and negative human-modified droughts: a quantitative approach illustrated with two Iranian catchments" *by* Elham Kakaei et al.

Anonymous Referee #2

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This paper proposes a new methodology to identify positive and negative human modified droughts and tests the methodology on two Iranian catchments. There certainly should be more studies investigating human-modified droughts and it is refreshing to see a case study application with data from catchments in Iran.

However, there are a number of key issues with the study that affect the robustness of the results and conclusions. While I agree with the authors that the methodology could enable quantification of positive and negative human modified drought in the Anthropocene - currently (1) the methods are not very clear which makes it difficult to understand exactly what was done in the study and (2) the methodology only works and

attributes droughts correctly if the modelling simulations are robust. The model performance and large differences during the naturalized period do not provide confidence in the validity of the key results of the paper and instead I believe that the human-modified droughts identified are more due to hydrologic model uncertainty rather than human activities. The presentation of the figures also needs to be significantly improved. I encourage the authors to re-address the hydrological modeling they have done, to significantly revise the methods section and to improve the quality of the figures to support their key results.

Main Comments

Subdividing the time series into natural and disturbed

Better justification is needed to prove that these statistics are identifying change points. One way could be to gain qualitative data about human activity that is occurring in the catchment (i.e. was a dam built in the change-point year, or a significant new water abstraction implemented?). Figure 1 also needs improving- currently it is really difficult to see any evidence of a distinct change point. As low flows are of interest then discharge could be plotted on a log scale, you should remove the blue dots and I would also add precipitation data. I would also consider plotting a much shorter timeseries (perhaps years that have similar climatic characteristics for both the natural and disturbed period).

HBV Hydrological Modelling Set Up

a) Objective Function – It isn't clear from the paper whether you used logNSE or NSE. There also needs to be better justification for this use of objective function – there are lots of different metrics that evaluate low flows and you need to better justify your choice here and discuss how it might impact the results.

b) Calibration – The calibration section needs to be much better explained. On Page7, L6 it states that 'The calibration procedure was done more than fifty times' and then

on L9 it states ‘each calibration was repeated 100’. It isn’t particularly clear what the calibration procedure is (simply optimizing logNSE but then R2 is introduced?), how the parameters ranges were chosen for each of the 14 parameters, how many samples were implemented (50 or 100?) etc. On L9 it also states that this ‘resulted in 100 different parameterizations’ but it isn’t clear how these 100 different parameterizations were used in the rest of the study – I assume you just took the best one to use in the rest of the study but given the uncertainty in the hydrological model, your results would be much more robust if you calculated natural-human drought characteristics for all 100 parameterizations.

c) Calibration Results – It is very difficult to see from Figure 4 the comparison between the observed and simulated discharge. It would be useful to plot a comparison of the flow duration curves (on log scale)

HBV Model Results

The attribution of positive and negative human-modified droughts rests entirely on the performance of the model and it’s ability to represent the naturalised flows. From Figure 5, the observed flows are plotted against the modelled naturalised flows for the period 1987-1988 for Eskandari and 1988 – 1989 for Kiakola. As I understand it from Section 2.2.2 both these periods lie in the ‘natural’ period (undisturbed period) for each catchment and so you would expect the naturalised model flow to (as much as possible) represent the observed flow and so climate-induced droughts to be identified at the same point in the time series. In this case, there are quite large discrepancies between the observed and modelled naturalised flows and climate-induced droughts are identified at completely different points. This is because the threshold is derived from the observed flows (which are very different to the naturalised flows produced by the model) and so casts doubts on the modelling results as they should be similar for the naturalised period. This has knock-on impacts for the attribution of positive and negative human modified droughts, which again I think are due to model simulation uncertainty rather than human influences. Consequently, it is difficult to have confidence

in the results and key conclusions of the paper.

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