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Discussions

Interactive comment on "Signatures of human intervention – or not? Downstream intensification of hydrological drought along a large Central Asian River: the individual roles of climate variability and land use change" *by* Artemis Roodari et al.

Anonymous Referee #2

Received and published: 8 October 2020

Dear authors, I have read the manuscript "Signatures of human intervention – or not? (...)" with great interest, as I think the attribution of droughts to human or natural phenomena can be very interesting with regards to drought risk reduction policies. The study clearly states its hypotheses and is able to – with limited amount of info available-model droughts in the Helmand River Basin, including both hydrological and human components. The manuscript is well written, and I particularly do like the creative graphics and the nuance at the end of the paper. While I am satisfied with the general

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setup and idea behind the analysis, I am left with a few concerns regarding the method, therefore I would like to recommend a major revision for this paper.

About the SETUP: The use of 10year periods to conclude about large trends is questionable. As droughts are (supposed to be) an extreme event, it is possible that some decades have more droughts than others without pointing to any climate- or human-related trend. Why not dividing it only in two periods? Or only looking at average trends?

About the RESERVOIR: A lot of your analysis of droughts is dependent on the assumption you make regarding the reservoir routing (line164+, in particular on line 175). The routing through reservoirs during low flow, in this case the most interesting one, has a rather low R2 (0.57). Have you done a sensitivity analysis to see how this affects your results? I think this should be more prominent in the discussion. Besides, there is the assumption of human reservoir operations that are absent, assuming the outflow is not adjusted by humans, but using the empirical link with the total storage distinguishing low and high flow, but make no distinction between drought and more-than-average-Q years. The conclusions about the influence of the reservoirs on the propagation of droughts should reflect this uncertainty.

About the MODEL: I feel I do not understand the additional parameters (such as deep infiltration losses) well: how are they parameterised? How do you know for sure this water is lost due to percolation? What is the importance and sensitivity of Snowmelt in the model? Since humans are not effective in applying irrigation water, there must be an underestimation of the water used for irrigation? I agree the end results of the hydrological bucket model are not bad (although the intra-annual variability is not very good), but with so many parameters, how sure are you that you model the correct processes? I suggest to add this to the discussion.

About the INDICES: I would like to see the goodness of fit of the gamma and GEV distributions for the SPI SPEI and SDI. They can matter a lot, a bad distribution (for

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some months) could potentially affect the rest of your analysis, hence other distributions could be a solution. I was wondering if you used the Stagge et al 2015 approach to deal with zero values for the Gamma? Besides, I wonder why you would use an accumulation time of 12 months – in a very vulnerable environment as you work in, I would think an accumulation time of three months is more relevant, as the 12months can balance out dry and wet periods in the different seasons. I strongly suggest to try the same analysis with an accumulation time of 3months and see if your results still hold. Then you can indeed say you balance short and long term effects. Moreover, I do not understand why you would use a standardised value of 0 to determine a drought. Often -1 is used. Further, the whole analysis now only investigates below average conditions, that maybe not lead to any impacts. I would add the same analysis but for a threshold of -1 or -1.5, to see how real extremes change over time and through space. Again, this could really affect your conclusions. Finally, I also do not really understand how you include the lag time that usually exist between meteorological and hydrological droughts: it is logical that the SPI12Dec1987 is not consistent with the SDI12DEC1987 because droughts travel through the hydrological cycle with a certain lag time. Did you account for this?

About the DISCUSSION In the introduction (line50+), you cite a few authors who have started to analyse droughts in Afghanistan and the HRB, but you fail to explain what your approach will add to this. Also, did they find similar results ? It does not come back in your discussion sections (you cite a lot of numbers: how do they compare with other studies?).

Small comments related to the text: 1. In the abstract, I would specify human influence better. When you state "however the downstream parts of the HRB moderated the further propagation. . .." (l30) I would explain that this is because of the dams/reservoirs and/or land use – then it is easier to reflect on what caused the shift in this effect. Moreover, I would clearly state that you assume reservoirs without any human management. 2. In the introduction (line70+), you refer to Mishra and Singh, but this sentence is very unclear. Please clarify that is the takeaway from this sentence. 3. I cannot find tis table S1 with al relevant model equations. . . (Only model variables) 4. Why would you show the actual instead of the potential evaporation in figure 10? SPEI uses potential, so that would reflect your drought analysis better.

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