Reply to the comments from Referee #2

Comment:

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 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.

Response:

We appreciate Referee #2 for the very constructive feedback and detailed comments provided, which helped us to improve the manuscript. Please find the detailed responses to the individual comments below. We will incorporate these comments in the revised version of the manuscript. We hope that our responses to the reviewer comments clarify all issues raised.

Comment:

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?

Response:

We would like to thank the Referee for her/his comment. We strongly agree with the reviewer that droughts are extreme and thus by definition rare events that may extend over longer time scales. Individual decades may thus indeed exhibit varying occurrences of drought and to better understand climate- or human-induced effects on droughts longer time periods need to be considered. To do this and to limit the potential of misinterpretations, we therefore first analysed the long-term or "average" trends over the entire study period, as suggested by the reviewer and as shown in Figures 7b,d,f, 8c and 10 in the original manuscript. To provide the reader with more detail and to get a closer understanding of the underlying processes we then, in addition, zoomed in to the individual decades. This then allowed us to better analyse the overall trends and to see if there was indeed a systematic shift over time in the observed pattern or if the overall trends were merely artifacts of extreme events in the first or last decade of the study period. While the decadal considerations for the temporal analysis in section 5.2 and Figure 7 do not add substantially more information but merely support the overall trends, they provide very relevant and interesting insights in the analysis of the spatial drought pattern in section 5.3 of the original manuscript. This can, in particular, be seen in Figure 8b, where the decadal discretization reveals a clear gradual and systematic shift between upstream and downstream drought characteristics over the study period. We would therefore strongly prefer to keep the original decadal discretization as an addition to the long-term trends. In addition, we will add a Figure in the Supplementary Material showing the same analysis with a discretization of the study period into 2 periods only, as suggested by the reviewer, to illustrate also the longer-term validity of our interpretation.

Comment:

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.*

Response:

We completely agree with the reviewer that a major source of uncertainty in the analysis is the lack of more detailed data on the operation of the two reservoirs in the Helmand River Basin. Although the storage-discharge relationship is statistically significant ($p<0.001$), the effect size remains modest. It is plausible to assume that reservoir operation is more careful during drier years. However, this should, at least to some degree, be implicit in the data we used to develop the reservoir routing scheme: during dry seasons water storage in the reservoirs is lower during drought periods than during wetter periods. Of course, this may not always apply. Although it may be a potentially very valuable idea to develop an individual reservoir operation scheme for drought periods only, we unfortunately do not have sufficient data to develop such relationships that are also sufficiently robust. We thus prefer to use all available data to inform a generally applicable scheme. Please also note that the estimated water release from the reservoirs results in overall model outputs in all downstream basins that are quite consistent with the observed daily river flow, which at station SISP (ID8) is even true for the entire 35-year study period. In spite of all other sources of uncertainty throughout the modelling process, this in itself can already be seen as an indication of the plausibility of the modelled reservoir outflow. However, we fully agree with the reviewer that a more detailed sensitivity analysis of the effects of the uncertainties in the reservoir routing scheme on downstream droughts will be very informative to better understand the limitations of or results. We therefore will add a detailed discussion on the effects of such a sensitivity analysis in the revised manuscript.

Comment:

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.

Response:

We would like to thank the Referee for raising our attention to this point. Addressing this comment we would like to clarify that, in absence of further quantitative information, we parameterized these losses as a constant daily loss between stations LHRB (ID7) and SISP (ID8), as specified by the additional calibration parameter (K_L). We agree with the reviewer that we too confidently attributed these losses exclusively to deep groundwater export. Although this of course may play a role as in many other basins worldwide (e.g. Schaller and Fan, 2009; Bouaziz et al., 2018; Condon et al., 2020), we overlooked another even much more plausible source of these losses: when the Helmand River reaches Iran, it bifurcates just upstream the gauge at SISP (ID8) into the Sistan river (SISP, ID8) which then drains into the Hamun wetlands and into the completely ungauged Common Parian River, which follows the border between Iran and Afghanistan. Therefore the lumped loss factor (K_L) combines the effects of deep infiltration, soil evaporation and particularly the proportion of water which is diverted into the Common Parian River. We will correct the definition of this parameter in the revised manuscript. Unfortunately there is not sufficient additional data available in the study area to directly estimate the losses.

Snowmelt in the Helmand River Basin, mostly originating from the headwaters in the Upper Helmand (UHRB; ID1) and the Upper Arghandad River Basins (UARB; ID4), accounts for ~80% of river flow in the downstream sub-basins. As such, snowmelt is, by far, the most dominant source of river water in the region. The importance of the meltwater contribution then also leads to a well-constrained snowmelt model component as shown by rather narrow posterior distributions of the associated model parameters T_T and F_{dd} in Table 2 of the original manuscript.
We completely agree that the rather inefficient irrigation schemes in the study region may lead to an underestimation of the actual irrigation water use. With the available data, this is problematic to quantify in more detail, though. However, please note that modelled river flow reproduces the observed daily river flow at SISP (ID8) rather well over the entire 35-year study period, whereof the 30-year period 1976-2006 was a validation period without further model recalibration. Such a rather good model performance over a 30-year validation period is in itself an indicator that the hydrological dynamics of the system are reproduced by the model in a plausible way. In addition, if the modelled irrigation is an underestimation, it would nevertheless be a conservative estimate: higher real-world irrigation water demand would even further strengthen the conclusion of our analysis, that the shift form of downstream moderation to the intensification of hydrological drought over the study period is largely an effect of human intervention. We will clarify this with a more detailed discussion in the revised manuscript.

Comment:
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 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?

Response:
We would like to thank the Referee for her/his suggestion. We will add the goodness of fit of the gamma and GEV distributions for the drought indices in the revised manuscript.
Interesting point. As mentioned in section 4.2 of the original manuscript and also reiterated by the reviewer in the comment above, we used 12 months accumulation periods to estimate SPI and SDI. Thus we, fortunately, did not come into the situation that we had to deal with 0-values, as the 12-months precipitation and discharge were always non-zero. The choice of the accumulation period is indeed always delicate in drought studies. For that reason, we did a preliminary analysis on multiple accumulation periods, including 3, 6, 9, 12 and 24 months. From that analysis we believe that 12-months accumulation periods can strike a good balance between the shorter and longer-term effects. Of course, no accumulation period can fully describe all drought-related processes that occur across multiple temporal (and spatial) scales. Yet, we

chose the 12-months period here as we think that it can best deal with the time lag introduced by the snow accumulation and melt dynamics which are the dominant control on river flow in the study region. Overall the differences between the accumulation periods are minor and do largely not affect the relevant features of the observed pattern, as shown in the heatmaps of the 3 drought indices for the 3 and 12 months periods here below in Figures R1 and R2.

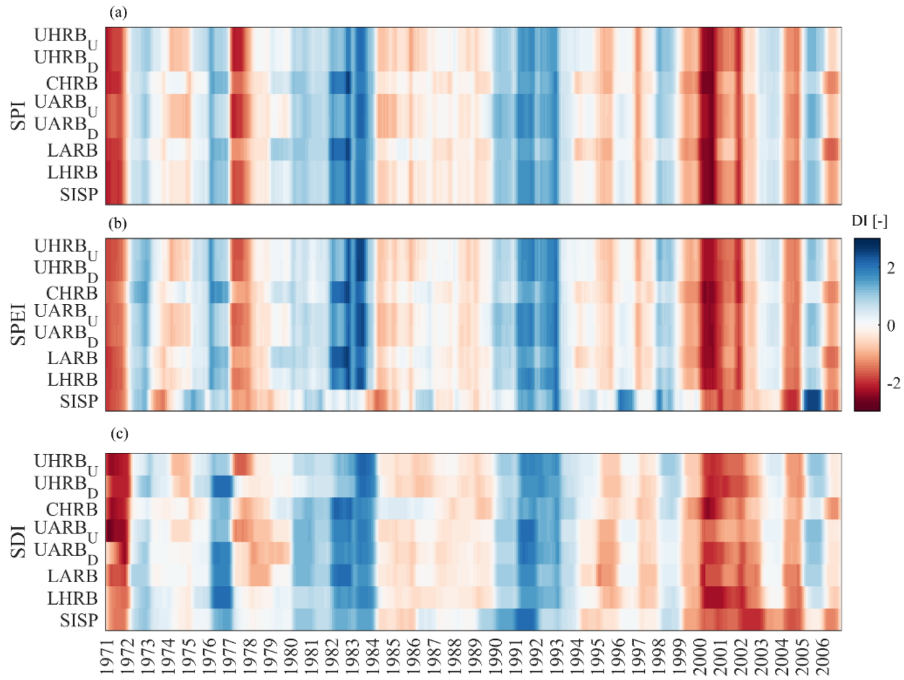

Figure R1. Time series of monthly drought indices (based on 3 months accumulation time) SPI, SPEI and SDI for the subbasins ID1 – ID8 for the 1970 – 2006 study period

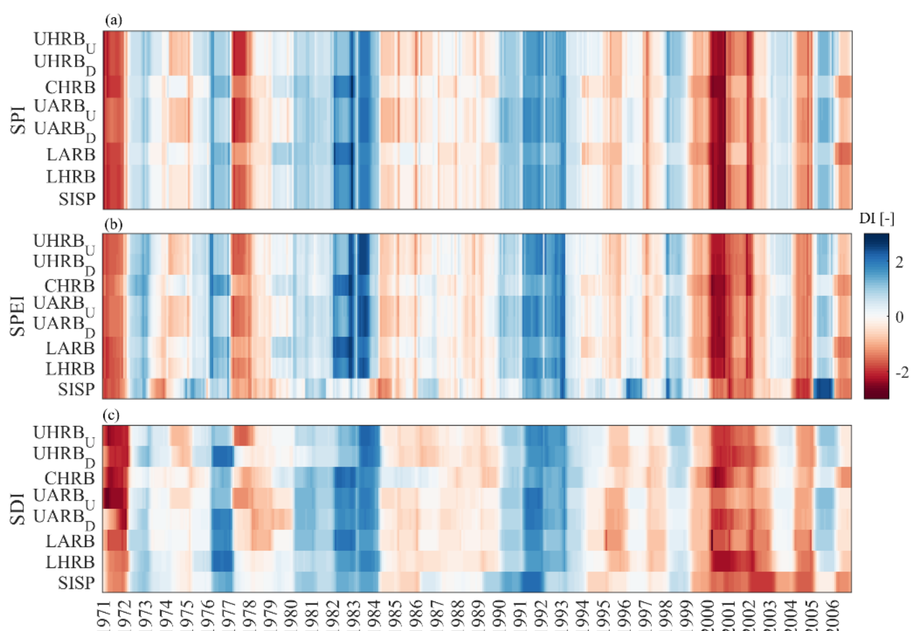

Figure R2. Time series of monthly drought indices (based on 12 months accumulation time) SPI, SPEI and SDI for the subbasins ID1 – ID8 for the 1970 – 2006 study period

Similarly, the choice of a drought index threshold to define droughts remains rather subjective. We agree that many studies use a threshold of -1. We here chose a threshold of 0 to provide a more comprehensive picture, including all below-average conditions. However, please note that most and the most important part of our analysis (Figures 7 – 10) do not require the definition of any threshold. Instead, we directly analyse the distributions of the individual drought indices. By doing so we avoid the need to define a binary and somewhat arbitrary decision drought yes/no. Using the full distributions has several advantages: it does not only provide a more complete picture as it includes all observed conditions, but also eliminates any further subjectivity in the analysis. However, to bring our analysis closer to what is typically done, we will adjust our drought threshold to -1 and provide re-calculated estimates of drought frequency, duration, intensity and severity in section 5.2.
We also completely agree with the last point of the reviewer here. There can be time lags of several months between meteorological and hydrological droughts. We have previously analyzed this in a preliminary cross-correlation anaylsis, which suggested a typical time lag of 3-6 months between SPI and SDI droughts (see Figure R3 here below for LHRB, ID7). However, as in this manuscript we do not compare the propagation from SPI drought to SDI drought, but rather the downstream propagation of both types of drought individually, we decided not to show the analysis on the lag times between the two as it would divert the reader from the main analysis in our manuscript without actually adding value for the interpretation.

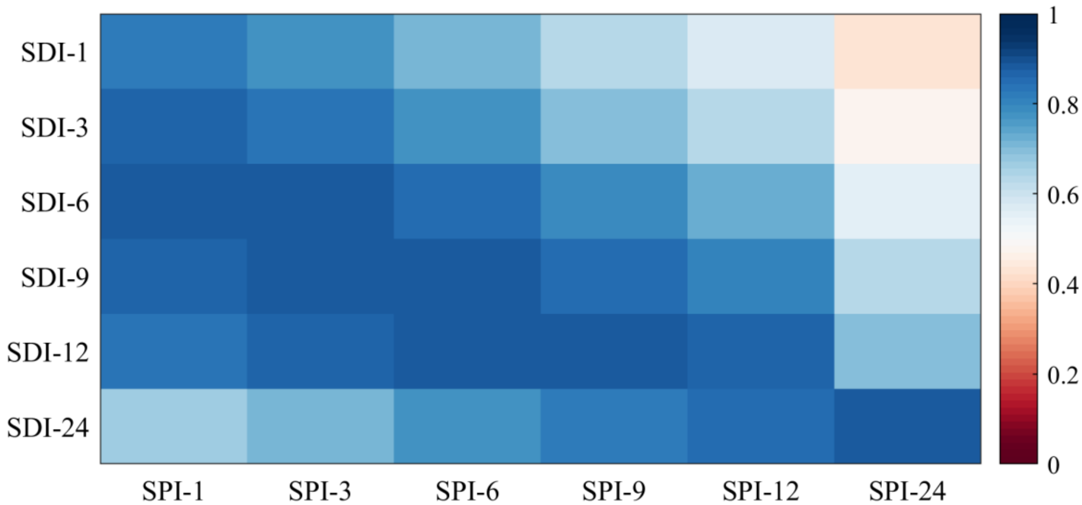

Figure R3. Heatmap of correlation coefficents from cross-correlation analysis between SPI and SDI for LHRB (ID7)

Comment:

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?).

Response:

Thank you for highlighting this important point. Addressing this comment we will provide a more detailed description of the novelty of our study and a more detailed discussion of our results in the context of previous studies which were conducted in the Helmand River Basin or similar environments. Briefly, most studies in the Helmand River Basin have so far remained limited to mere documentation and/or general assessments of mostly meteorological drought characteristics. We here extended this scope also to SPEI and SDI droughts. We particularly focus on changes in drought characteristics over time and space and attempted to explicitly and quantitatively attribute these changes to climate and human activity.

Comment:
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.
Response:
We thank the referee for these suggestions and we will adapt the text in the abstract accordingly in the revised version of the manuscript.

Comment:
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.
Response:
Thank you for raising our attention to this point. We will clarify the sentence and adjust it to: "As pointed out, amongst others, by Mishra and Singh (2010) the processes underlying droughts are complex because they are dependent on many interacting processes in terrestrial hydrological systems, such as the interaction between the atmosphere and the hydrological processes which feed moisture to the atmosphere.

Comment:
I cannot find table S1 with al relevant model equations: (Only model variables)
Response:
Table S1 (Water balance and constitutive equations used in FLEX model) is in the supplementary materials of the paper.

Comment:
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.
Response:
It is true that SPEI is typically based on potential evaporation E_P. However, we believe that in arid and thus water-limited regions fluctuations in E_P are rather irrelevant compared to fluctuations in P or E_A. In other words, there will be little difference in the partitioning of water fluxes if under the same annual precipitation of e.g. 500 mm yr^{-1}, E_P is 1000 or 1500 mm yr^{-1}, as in both cases actual evaporation E_A will be close to (or even exceed) 400 mm yr^{-1} and as therefore most of the available water will be evaporated. In contrast, more water will be evaporated as E_A (even if E_P remains stable) in years when more water is available and thus P is higher. By extension, the effects of evaporation on droughts in arid regions can only be meaningfully assessed by changes in E_A. That is also why we did not develop a strong link between SPEI and the E_A anomaly in the text as analysed in section 5.4 and figure 10, where we tried to quantify the individual role of each component of the water balance for the development of hydrological drought in the HRB. We will clarify this in the revised manuscript.

References:

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