Reply to Referee #1

Comment:

The manuscript presents an interesting study on the intensification of downstream hydrological drought caused by meteorological and Human influences using a physicallybased hydrological model and drought indices (SPEI, SPI and SDI). The study is suitable for publication in Hydrology Earth System Sciences (HEES) Journal.

Response:

We would like to thank the referee for the time and effort she/he put into this review. The points she/he raises are highly relevant and addressing them will help to improve our manuscript. We hope that with these replies and the associated revision of our manuscript all issues raised can be clarified.

Comment:

The introduction section needs to be rearranged and improved. Specifically, the authors should start the introduction by stating how their research constitute a global problem before highlighting the regional problems identified in the Central Asian River Basin. I suggest that the authors start the introduction with line 66:81 (meaning it should be moved up and edited meaningfully) before discussion on the study basin. In doing so, however, the authors should ensure the literature cited are relevant in the body of work and should reflect in the discussion section.

Response:

We thank the Referee for having drawn our attention to this point and indeed we agree that it will help the logic and flow of the manuscript to start with a description of the general problem before going into regional detail. The text will be revised accordingly. Due to the scarcity of literature in this basin, we tried to cite the relevant studies in the study basin and other basins in similar environmental settings, subject to similar drought phenomena. In any case, we will extend the literature overview to provide some more context, in particular with respect to the global drought problematic.

Comment:

The methods used in this research has a good scientific foundation. Nonetheless, the reasons for selecting the hydrological model need to be highlighted in a few sentences given that other hydrological models can perform this same task. Why use this model and why is it important. The authors mentioned the scarcity of data as a limitation of this research, does this model help to alleviate the problem of data scarcity? Why do the authors choose this model over other hydrological models?

Response:

We agree that some more explanation on the choice of the model will help the reader to understand our decision. Briefly, the distributed implementation of this process-based model from the FLEX-model family was chosen as these models were previously successfully applied in climatically similar regions (e.g. Gao et al., 2014, 2017) but also many other settings worldwide (e.g. Fenicia et al., 2006; Kavetski et al., 2011; Nijzink et al., 2018; Bouaziz et al., 2018; Hulsman et al., 2020). In addition, the FLEX modelling concept is underlain by a philosophy of model customization and rigorous testing to ensure the implementation of suitable model formulations and the associated more reliable model outputs in different environments (e.g. Fenicia et al., 2011). Given the high number of different hydrological models that have been developed over the past decades, it is indeed plausible to assume that alternative model formulations could have been used to perform the same task. Please also note that, following a multi-objective calibration strategy, i.e.

simultaneously using $E_{NS,Q}$ and $E_{NS,log(Q)}$ as calibration objectives to ensure good representation of both, high- and low flows, our model performances with respect to daily flow in all sub-basins (Table 4, Figure 4), exceed those of the studies of Hajihosseini et al. (2016) but also those of Hajihosseini et al. (2019) who assessed the monthly flow with the SWAT model in the Upper and Lower Helamand basins, respectively. We will clarify this in the revised manuscript.

Scarcity of data is a problem that is faced by *any* type of model application in science and engineering. Available observation technology cannot provide us with the necessary observations to meaningfully quantify the natural heterogeneity of the (sub-)surface properties of large scale terrestrial hydrological systems, i.e. anything larger than the hillslope scale (e.g. Beven, 2001, 2006; Hrachowitz et al., 2014; Zehe et al., 2014). To obtain the parameters of hydrological models (which represent these properties) we therefore have to resort back to the calibration of inverse model applications. Given the number of parameters in these models, data scarcity frequently limits our ability to meaningfully constrain model parameters, thereby making the modelling problem ill-posed and resulting in the well-known equifinality problem (e.g. Beven and Binley, 1992; Savenjie, 2001; Beven, 2006; Clark et al., 2011; Hrachowitz and Clark, 2017). From that perspective, our model here does not "alleviate the problem of data scarcity". Rather, we have gone quite some length to extract as much information as possible from the available data to *constrain the parameters of our model* in this data-scarce environment to avoid the adverse effects of equifinality as much as possible.

However, what are our model results finally permitted us to do, within the margins of uncertainty associated to the equifinality problem, is to estimate stream flow in periods when no observations were available at some gauging stations in the study basin. From this alternative perspective, our model can indeed be considered to "alleviate the problem of data scarcity".

Comment:

Results and Discussion: The result was well presented; however, the discussion was not adequately presented. The authors need to discuss the result by comparing or contrasting conclusions made with relevant literature. The underlying physical processes and human activities that were outlined to be responsible for the derived result should have strong theoretical underpinning. Only then can the result have a strong scientific basis and meaning. If this is not done, this section may look like a mere presentation of result. I will suggest the authors separate the result from the discussion. It will help to know where the result presentation ends and where the discussion begins.

Response:

We acknowledge and appreciate this comment. However, given the stepwise nature of the analysis we would strongly prefer to have the results of the individual steps closely associated to the interpretation and discussion thereof. However, we will extend the discussion and provide some more detailed context and comparisons with the results of previous similar studies in the revised version of the manuscript.

Comment:

It is fascinating to see that the authors presented a limitation of this research. Clarity on limitations is beneficial in outlining the extent of possible errors. However, it will be more meaningful, for instance, if the authors specifically suggest a likely better approach to help limit uncertainties inherent in the result. For instance, is there a hydrological model that does better in the data-scarce region and that will capture different processes as highlighted by the model used; otherwise could developing such a model be a way to limit uncertainties? Moreso, will comparing different meteorological dataset to determine which best *captures drought (with reference to SPEI and SPI) within the study site help limit uncertainty for better policy formulation on water resource management? All these specific forward-looking documentation can be made to readers to know about possibilities and solutions since the data may inaccurately depict reality. And in-turn improve the quality of the manuscript.*

Response:

We thank the Referee for the positive feedback on the limitation part of the study. As mentioned in one of the replies above, by customizing our model to the study basin and the data available there, we attempted to find an efficient trade-off between model complexity and model uncertainty. While we can of course not exclude that an alternative model formulation may do a better job, we still believe that we were able to strike a good balance between the process resolution and the resulting uncertainty in our model. This is in particular true as, after calibration for the 1971-1975 period, the daily model outputs for the gauge that recorded flow for the entire 1971-2006 period (ID8) remained rather good for the entire 35-year (!) study period (i.e. calibration period 1971-1975 and validation period 1976-2006; Table 4 and Figure 4). The best way to reduce uncertainties would be to do more observations and generate data, which could then be confronted with the model. Deficiencies in the model to reproduce this additional data could then, in an iterative process allow model improvement (e.g. Fenicia et al., 2008; Hulsman et al., 2020). However, at this stage, we believe that further model improvement will be difficult as the introduction of more complexity in the model will not be warranted by the actually available data and eventully merely lead to increased equifinality (Beven, 2006). We will clarify this in the revised manuscript.

We strongly agree with the reviewer that using alternative data sets, e.g. for precipitation, may lead to different results and interpretations. Although outside the scope of our manuscript, a future comparison of different data sets is therefore highly recommended. We will strongly emphasize this in the revised version of the manuscript.

Comment:

The grammar is sometimes not correct. The authors should allow a native speaker to help improve some sentences.

Response:

We will improve the grammar in the revised version.

Comment:

Line 97: remove "s" from occurs.

Response:

The word *occur* in this sentence refers to *precipitation*. Thus to the "s" is needed.

Comment:

Line 117-118: the sentence on this line ended with "most of the crops located in the traditionally irrigated areas (Wardlaw et al., 2013)". I feel this sentence is incomplete or rather add no additional meaning to the preceding sentence.

Response:

The sentence will be corrected to " … and large areas of opium poppy are grown especially in the traditionally irrigated area (Wardlaw et al., 2013)".

Comment:

Line 151: put "."After Ssn.

Response:

It will be corrected in the revised manuscript.

Comment:

Line 161: delete "that".

Response:

It will be corrected in the revised manuscript.

Comment:

Line 261: remove "the" before give.

Response:

It will be corrected in the revised manuscript.

Comment:

Line 294: Sentence not understandable. The authors placed Iran after requirement. The sentence needs to be rewritten to portray real meaning.

Response:

The sentence will be modified to "to meet irrigation demand in the downstream Helmand Valley and to satisfy the flow requirements of the Sistan River in Iran under the Iranian-Afghan Helmand River Water Treaty (1973)".

Comment:

Line 300-304: more explanation needs to be given about streamflow loses. Since soil evaporation is not enough reason as to why 60% of stream water is lost in the hyperarid region, an explanation should be provided from the literature with regards to the inferences made by the author. Since the authors outlined that deep aquifer recharge and deep infiltration may be responsible for loses, more explanation should be given with references from existing literature.

Response:

We would like to thank the Referee for raising our attention to this point. Addressing this comment we would like to clarify that we considered one additional parameter (K_L) to account for losses between ID7 (LHRB) and ID8 (SISP). It is true that we too confidently attributed these losses almost 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 drains into the Hamun wetlands, and 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. Merely Burger (2005) in a study of the Helmand River of Afghanistan and Iran loosely mentioned some schematization errors in their project including the existence of flow diversions from the Helmand River into the Common Parian River.

Comment:

Line 350: delete "with" after was.

Response:

It will be corrected in the revised manuscript.

Comment:

Line 355-359: there is a need to corroborate the role Ep plays in intensifying or moderating drought. The authors need to discuss with relevant literature.

Response:

Thank you for your suggestion. We will discuss the role of E_P as an effective factor on droughts in the revised manuscript. However, please note that in arid environments such as in the study region, fluctuations in E_P will have a limited effect on E_A and thus on water deficits. As by definition, these regions are limited by water rather than by energy supply, E_A will not significantly change when precipitation remains stable at e.g. 400 mm yr^{-1} while E_P fluctuates from e.g. 1000 mm yr^{-1} in one year to 1300 mm yr^{-1} in the following year. In either case, most of the 400 mm yr^{-1} of available water will be evaporated as E_A. Changes in E_P will therefore be less relevant for the intensification/moderation of drought in such arid regions than changes in precipitation.

Comment:

Line 434-436: The authors should compare or contrast their findings/conclusions with relevant literature.

Response:

We agree. In the revised manuscript we will extend the discussion and better place our results into a broader context of previous studies.

Comment:

Line 437: recast sentence.

Response:

We will adjust this sentence to "Such short-term influences of hydrological drivers deficits, would be likely to manifest themselves in the evolution of ΔSDI characterized by more erratic temporal pattern".

Comment:

Line 441: remove "s" from estimate.

Response:

It will be removed in the revised manuscript.

Comment:

Line 486-487: the sentence starting with demand is not understandable, please recast.

Response:

We will change the sentence in the revised manuscript to: "Irrigation demand in the lower basin accounts with I_D ~ 13 mm y^{-1} for a substantially larger fraction of the water balance (~ 14%) than in the upper basin."

Comment:

Line 500-501: what do the authors mean by "consequence of the extension the….." please recast to portray the intended meaning

Response:
we will recast this sentence to " … is largely a consequence of increases in agriculturally used area which resulted in increases of the related irrigation water demand".

Comment:
Line 508: Do not start the sentence with "this.
Response:
It will be removed in the revised paper.

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