Review of "**Assessment of streamflow decrease due to climate vs. human influence in a semiarid area**"

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Thanks for the chance to review this paper.

Message and contributions of the manuscript: This study aims to estimate the fractional contribution of climate variation and regional human activities to changes in streamflow in a semi-arid river basin in Iran (Karkheh River Basin, KRB) over the period 1980-2012. In doing so, they have split the study period into two period of pre- and post-change based on streamflow trends. To estimate streamflow changes authors used a Budyko model together with the HBV hydrological model. They have also estimated land use changes in the basin showing that the irrigated farmlands expanded over this period. The study is done across the 5 sub-basins of KRB. This basin has regional importance due to energy and food production for over 4 million people.

My evaluation: I have not identified any major contributions in this study whether in terms of process understanding or methods. I acknowledge that authors said that this is the first time the Budyko hypothesis is used in Iran, which is a positive that helps to improve the geographical bias in the hydroclimatology literature. That said, all the methods (breakpoint detection, Budyko hypothesis, HBV modeling) have been developed previously. Similar studies on fractional contribution of climate variation and human activities to annual runoff changes have been done before, such as (Liu et al., 2017) and (Yang & Yang, 2011), each offering a new contribution either in method or process understanding. Even compared to those studies, the present study is an over simplification when it comes to analyses such as estimation of potential evapotranspiration (PET) based on a just a correlation with temperature, inadequate model calibration with no account for parameter uncertainty, and inadequate sensitivity analysis of the Budyko hypothesis (section 3.5). In addition to improving the analysis, the manuscript itself also requires major improvement: the paper is long with repetition of previous studies. The present discussion (section 4) is just an overview of the study area, a proper discussion of the results of this study is required. In doing so, the results should be discussed within the a broader context of studies in similar regions in Iran or around the globe. The discussion section would also benefit from a new sub-section discussing the limitations of results and suggestions for future studies. There are several typos, hence a careful proof read.

One major conceptual point is that the terms "climate change" and "climate variation" are used interchangeably in the manuscript. Regardless of what term is better suited (maybe climatic changes?), it is important to clearly define in the manuscript (perhaps in the introduction) that in this study climate change/variation – which human activities are compared to – is a lump variable that encompasses both *human-induced climate change* and *natural climatic variability*. Therefore, the contribution of human activities are under-estimated. This is an important point to discuss in the discussion section (see my comment 1 for details).

I am sorry that my evaluation is not as positive as authors would like. I genuinely acknowledge all the efforts that go into doing a research and writing a manuscript, yet in my opinion the current manuscript does not meet up to the requirement of HESS. This manuscript, upon improvement, better suits journals like *Journal of Hydrology: Regional Studies*.

My detailed comments:

1. Use a consistent term in referring to climatic changes throughout the manuscript (including title). And clearly define that what you referred to as climate change/variation is in fact a lump variable that includes both human-induced climate change and natural climatic variabilities. Therefore, there is a contribution of human activities to climatic changes that you are not capturing in this study, and hence human activities, in general, are under-estimated in this framework. What you estimated as "human activities", is in fact "regional human activities/interventions" in terms of regional water resources projects which is distinct from emissions that are also due to human activities. Other factors such as the impact of dam construction on regional evaporation and precipitation is assumed to be zero, while a major dam as such would presumably have impacts on the regional climate. Human activities are also under-estimated within the Budyko model by definition, as the parameter n in the Budyko equation is assumed to be a constant. That is, interannual changes in catchment characteristics are assumed to be zero (Yang & Yang, 2011). That means the pre-change period could have been impacted by regional human activities that are hard to capture and compare with the post-change period within the present method.
    a. On this topic, there been several recent studies on Lake Urmia, not far from KRB (Alizade Govarchin Ghale et al., 2018; Alizade Govarchin Ghale et al., 2019; Chaudhari et al., 2018; Fazel et al., 2017; Khazaei et al., 2019). Fazel et al. (2017) reported an interesting point, relevant to your study, that "*flow regime in river headwaters appeared to be dominated by natural forces, … the reduction in river flow magnitude increased from headwaters to downstream reaches for all rivers*". This is a major point to bear in mind when discussing your results, as you have not differentiated between headwater and downstream changes in flow.
2. The introduction requires further refinement to be sharp and concise.
3. There is no shortage of method for estimating potential evapotranspiration, including plenty of temperature-based methods (McMahon et al., 2013). A simple correlation is an over-simplification. The T-PET is not a linear relationship. Check out the modified Hargreaves method Droogers and Allen (2002) or the temperature-based method in Oudin et al. (2005).
4. I'd take issue with your hydrological modelling approach on two main grounds. (1) Parameter uncertainty is not accounted for. It is well-established that hydrological models require some type of parameter uncertainty analysis, i.e. models should be used as ensembles. In addition, it is not clear whether you have calibrated the model on daily scale or monthly. Calibrating the model on monthly scale would introduce additional uncertainties. It is easier to achieve a higher model performance in monthly calibrations, as daily variations would be ignored. This is particularly important when models are transferred to changing conditions, as in this study. A model that

cannot reproduce daily variations in the calibration period, would do even worse during an evaluation period with considerable changed conditions. (2) The objective functions used to evaluate the model performance are inadequate. R^2, which is essentially NSE (Nash Sutcliffe efficiency), has been demonstrated to be an inadequate metric for evaluating model performance (Gupta et al., 2009; Murphy, 1988). KGE (Kling Gupta Efficiency, updated version by Kling et al. (2012)) has been shown to be a better alternative to NSE, which already includes a bias term. So, if KGE is used, the second metric you used (Equation 3) would be redundant and you can remove it from model calibration/evaluation. So, I'd suggest to redo the hydrological modeling to account for parameter uncertainty using the KGE metric – and on a daily basis.

   a. Details about HBV model particularly the name of the parameters are unnecessary. Simply remove them and refer to original literature of "the version" of the model you used in this study (there are different versions of HBV). Other parts such as Table 7 (range of calibrated parameters) should be in the supplements as they are not contributing to the main story of the paper.
   b. Lines 356-360 "*The HBV model overestimated the streamflow for the post-change period, which suggests that there existed factors other than climate variations affecting the streamflow of the study basins. These factors are believed to be related to human activities*". You are oversimplifying the problem here. The issue that hydrological models perform poorly under changing conditions is a widely recognized issue and in fact one of the unsolved question of hydrological sciences (Question 19 in Blöschl et al., 2019). That said, you cannot simply conclude that those changes are due to human activities. The underlying reason is a combination of model structure adequacy to represent the evolution of catchment processes, model parameterization, climatic changes (whether natural variability or human-induced) that cannot be represented well by model structure and parameters, and changes due to human activities. I agree that human activities has a role in this, but you cannot simply "believe" that it is all due to human activities.

5. Section 2.5 on Budyko method is too long. All the equations have been derived previously. You can simply explain the method conceptually, direct readers to the literature for more details, and present only the equations you used.
   a. Line 241, it is not "believed" but "assumed" to be…
6. Section 2.7, lines 288-289, "*In order to adequately simulate a hydrological response at the basin level, accurate data such as climate variables (precipitation, ET, etc.) and catchment physical characteristics (topography, land coverage, vegetation, etc.) are vital.*" what about the adequacy of the model structure, both the Budyko model and HBV, in representing catchment processes and change?
   a. Lines 293-294, it does not make sense and does not follow the previous paragraph. Climate elasticity does not address the uncertainties…?
   b. The possible sources of errors (line 300) are the break change method, Budyko model and parameterization, HBV model structure and parameterization, and data

quality and length. Each of these factors, individually or combined, may change the results to some degree. Among these factors, you did not look into the dependency of your results upon the choice of breakpoint method. For instance Liu et al. (2017) used 8 different methods to account for this. You have not looked into HBV parameter uncertainty. What you discussed in section 3.5 is actually a sensitivity analysis of the Budyko model, for which you only reported 10% change in parameter n. What if all the inputs and parameters of Budyko are subjected to 5, 10 and 15% change? How the change in Budyko inputs propagate to the final answer? What about the change in model structure, e.g. if another model was used instead of HBV? Etcetera etcetera… A more rigorous sensitivity analysis is required.

7. The discussion of land use changes (section 3.4) can be improved. It takes one page, two figures and two tables to just say that irrigation area has increased significantly while forested area decreased. It is also good to briefly mention that there are three snapshots of an early year, changepoint, and final year to help readers follow the story line.
8. The figures are not properly ordered. After presenting figure 2 on page 5, authors jumped to figure 9 on page 10, and then back to figure 3 (page 10). Figure 11 is not relevant to the discussion session of the paper, it is relevant to introduction or study area. Figure 12 does not offer anything new, Yang and Yang (2011) already presented and discussed this.
    a. Figure 3, present all the 5 sub-basins in one figure.
    b. Figure 6 is not easy to read.
    c. Figure 7 and 8 could be combined, as two subplots.
9. Line 469, "*The Budyko method showed to be a reliable … method to analyze streamflow changes during the study period*". You have not offered any analysis nor discussion on the *reliability* of the Budyko method.
10. There are several typos throughout the text. A carful proof read is required. Here are a few minor corrections and typos (just as examples):
    a. The name of "Seimareh" river has two different spellings on the Figure and in the text.
    b. The symbol for changes due to human activities is not consistent: ΔQ_{HA} (e.g. line 243) and ΔQ_{H} (e.g. equation 11) – fix throughout. Also fix the symbol for ET_0 and E_0 in equation 13. And the denominator in the second term of the equation 15.
    c. Avoid making up abbreviations in the middle of the manuscript such as *Eqn* on line 202. Be consistent either use abbreviation or the full word throughout. Check the journal guideline for this. Same comment applies to Fig. and Figure.
11. Minor comments:
    a. Within the current visualization of Figure 1, it is very hard to see the stream gauges in most sub-basins. Since you didn't mark each discharge station with its name on the map, it is hard for readers to tell where Payepol station is: is it the blue square inside the Upper Karkheh sub-basin, or is it the one outside of it (the final outlet of the entire catchment)? Also show the location of the Karkheh dam on the figure.

b. Study area page 3: Line 93, I think it's better to mention the actual population (i.e. 4+ million people) the river supplies water to. It's good to keep "5% of Iran's population" for context too. Line 103: it is good to point out that more than ~60% of the precipitation received in northern mountainous area is snow.

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