

Response to the reviewer's comments - hess-2019-618

The authors would like to thank the reviewers for their constructive comments that helped to improve the quality of the manuscript. Our point-by-point responses for the reviewers' valuable comments are listed below.

Reviewer 1:

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.

Thank you for the detailed review and pinpointing shortcoming of the manuscript.

Although, we admit that all the methods including the Budyko, HBV, DBEST, and satellite image processing used in this study are separately developed in previous studies, our manuscript introduces a novel combination of these methods such that a new, more robust framework of separating human vs climate variation effects on streamflow of large river basins in the data-scarce area is presented.

As mentioned, this is the first time that Budyko analysis is implemented in the Iranian catchment. The case study is one of the most important catchments in the country and ironically there is the lack of studies investigating separate impacts of human activities and climate change in the area. In any case, the study offers a new approach for data scarce areas to quantify effects of climate change versus anthropogenic influence. This involves a technique to validate the Budyko method with remote sensing analyses. None of the previous studies mentioned by the reviewer has applied satellite remote sensing techniques to quantify land use changes over a long period of time for verification of the Budyko and HBV modelling results. We firmly believe that this is one of the major contributions of this study to the methodological approaches in the field of hydrology.

Also, as a novel approach, we used the newly developed DBEST algorithm for detecting breakpoints. DBEST uses a (novel) segmentation algorithm for detecting and characterizing significant breakpoints, and has a general designation making it suitable for different time series data inputs. It has been applied successfully in several other studies and this paper shows its suitability for streamflow change detection. Not mentioning that the introduction of the DBEST method here is important because it is new in hydrological studies and specifically relevant in Budyko applications for a more reasonable, systematic selection of time periods to be compared.

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).

The sensitivity analysis provided in section 3.5 and Table 11 shows that the model is not very sensitive to changes in evapotranspiration and hence temperature. Moreover, the new PET stations were added to increase the accuracy of the evapotranspiration time series. As the reviewer pointed out, we used a traditional, however, scientifically sound method for estimating ET at the basin level, even though this was not the main focus of the study.

In addition to improving the analysis, the manuscript itself also requires major improvement: the paper is long with repetition of previous studies.

Thanks for the comment, we have done an intensive revision; we have shortened the manuscript and removed the repetitive section highlighted in the revised version of the manuscript.

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.

The authors agree and the discussion section is improved in the revised version (page 12-14 – lines 345-445). Further, a sub-section was added to the discussion part, on page 14- lines 410-415, improving the analyses of the results.

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). 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.

Although it is not uncommon to use the terms, climate change and climate variability, interchangeably, but, the authors preferred to use the climate variability, because the study period is not long, in climate scale.

Definitely, human activities and climate change are tied together and not independent. However, to do the analysis, it is a common assumption to consider them as independent parameters (Zeng, Xia et al. 2014, Zhang, Liu et al. 2016, Wu, Miao et al. 2017).

In this study, as you mentioned, climate variability is a combination of variability of climate, as well as global-scale human-induced effects due to worldwide greenhouse gasses increase. Human activities, on the other hand, is regional impacts of human. Dam construction and agricultural activities, etc. are intimately embedded in human impact.

Many of these relationships, now have been better described and discussed in the revised manuscript.

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).

Similar studies have assumed the parameter n as a constant. We have now discussed this limitation on page 15- line 425.

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.

Definitely upper flow and downstream play important roles in a catchment’s flow regime and we have considered them too, in this work. The basin was divided into five sub-basins and the upper basin streamflow was taken into account while doing analysis for the lower sub-basin.

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 oversimplification. 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.

We have improved the introduction. As mentioned, our results are not that sensitive to changes in evaporation. Moreover, we had several evapotranspiration stations in the catchment. The extra stations are added to improve the accuracy of the estimations.

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.

We have now changed the previous HBV modelling results and applied the most recent version of HBV model (HBV-light) which has the facility to integrate the "uncertainty analysis". Using the embedded Genetic Algorithm in the HBV-light model, we were able to perform an uncertainty analysis using the 10 best parameter sets. The results of the investigated uncertainty is added in the revised version of the manuscript (subsection on page 12).

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.

The HBV model simulation including calibration process and all inputs are on a daily basis. In the figures, they are shown on a monthly time-scale for the sake of presentation. We have now discussed this for clarification on page 9 line 279.

(2) The objective functions used to evaluate the model performance are inadequate. R2, 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.

Thank you for the suggestion. We did the hydrological modelling again with the new version of HBV model and added the mentioned criteria (KGE) in Table 8, based on your suggestion.

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.

Thanks for the comment. We have now revised the manuscript accordingly. The introduction is enhanced and more specific now. Unnecessary and wordy parts have been removed and the table has been moved to Appendix section on page 33.

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.

Definitely, different researchers have different methods and understanding of the problem and concept. However, as mentioned by Dey et al. (2017), the applied procedure is a common method which has been employed by other researchers such as (Hu, Liu et al. 2012, Sun, Tian et al. 2014, Chang, Zhang et al. 2016). However, your valuable point has now been discussed in the limitation section in the revised manuscript.

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...

Thanks for the comment. We revised the manuscript accordingly. ‘Believed’ was also changed to ‘assumed’.

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.

Thanks for the comment. We reviewed and changed these lines and highlighted them in the text, uncertainty analysis subsection.

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.

Definitely, each hydrological model has its own advantages and limitations and Budyko and HBV models have no exceptions.

Liu et al. (2017) studied several catchments across china, using three approaches of AMOC, Ordered Clustering Test, and Pettitt test methods for the detection of change points.

The focus of studies such as Liu et al. (2017) is to analyse, in detail, the driving factors and their exact shares affecting streamflow changes. While, in this study we basically focus on the impact of human activities and climate variation. For future studies, if there is enough data available, the details of human activities can be studied.

The breaking point occurs when the trend of the streamflow changes. In this study, the breakpoints were detected by a newly developed DBEST method and was cross validated with Man-Kendall test.

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?

The suggestion for future work and discussion has been added to the discussion part but implementing all mentioned methods are beyond the scope of this research.

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.

Thank you for your suggestion. It has now been added to the revised manuscript line 312.

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.

Thanks for the precise comments. We amended the text and figures accordingly.

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.

There are several typos throughout the text. A careful 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: ΔQHA (e.g. line 243) and ΔQH (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.

We appreciate your careful reading of the manuscript. These comments are now implemented and highlighted in the revised version of the manuscript.

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 the entire catchment)? Also show the location of the Karkheh dam on the figure.

Thanks for pinpointing the issues. A better map has now been used.

Additional References (added to the revised manuscript):

- Chang, J., H. Zhang, Y. Wang and Y. Zhu (2016). "Assessing the impact of climate variability and human activities on streamflow variation." *Hydrology and Earth System Sciences* **20**(4): 1547-1560.
- Hu, S., C. Liu, H. Zheng, Z. Wang and J. Yu (2012). "Assessing the impacts of climate variability and human activities on streamflow in the water source area of Baiyangdian Lake." *Journal of Geographical Sciences* **22**(5): 895-905.
- Sun, Y., F. Tian, L. Yang and H. Hu (2014). "Exploring the spatial variability of contributions from climate variation and change in catchment properties to streamflow decrease in a mesoscale basin by three different methods." *Journal of Hydrology* **508**: 170-180.

Wu, J., C. Miao, X. Zhang, T. Yang and Q. Duan (2017). "Detecting the quantitative hydrological response to changes in climate and human activities." Science of the Total Environment **586**: 328-337.

Zeng, S., J. Xia and H. Du (2014). "Separating the effects of climate change and human activities on runoff over different time scales in the Zhang River basin." Stochastic environmental research and risk assessment **28**(2): 401-413.

Zhang, Q., J. Liu, V. P. Singh, X. Gu and X. Chen (2016). "Evaluation of impacts of climate change and human activities on streamflow in the Poyang Lake basin, China." Hydrological Processes **30**(14): 2562-2576.