Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-54-RC3, 2020

Hydrology and Earth System Sciences Discussions
Open Access

Interactive comment on "Future streamflow regime changes in the United States: assessment using functional classification" *by* Manuela I. Brunner et al.

Genevieve Ali (Referee)

gali@uoguelph.ca

Received and published: 5 May 2020

GENERAL COMMENTS

In this manuscript entitled "Future streamflow regime changes in the United States: assessment using functional classification", two main goals are pursued: (1) develop a catchment classification scheme for streamflow regimes, and (2) use this scheme to evaluate changes in future flow regimes. Contrary to the majority of previously published catchment classification efforts, here the authors decided not to rely on streamflow indices. Instead, they are using a functional approach via which the shapes of mean annual hydrographs are classified, this in order to retain temporal autocorre-

Printer-friendly version

Discussion paper

lation information. Overall, the manuscript is of appropriate length, well written and with good-quality figures and tables. The dual focus of the manuscript on catchment classification and climate change impact assessment is very interesting, and I agree with the authors about their description of the advantages of functional classification. I did find that a few statements made in the manuscript warranted clarification, and that some details regarding the datasets, process interpretations, or linkages with existing literature were lacking (see specific comments below). With revisions, I believe that this manuscript will be interesting to the HESS readership, and a great addition to our growing body of literature on catchment classification.

**

SPECIFIC COMMENTS

N.B.: page and line numbers are noted as PX (page X) and LX (line X).

Section 2.1: Given the international readership of HESS, I think that more detailed information is needed about the catchment selection criteria. For people not familiar with the CAMELS dataset, it is quite unclear what is meant by "minimum human impact": is the human impact assessed in terms of catchment-wide land use (that would mean no agricultural or urban catchment), or river regulation? And how may the answer to that question affect the generalization potential of the manuscript conclusions? In other words, the authors should discuss the limitations associated with not considering human-impacted catchments in the present study... Also, how was the 1981-2018 data period chosen for the analysis?

P5 L120: There is a reference to characteristics with missing values. Which characteristics (or types of characteristics) are the ones with missing values? Did omitting them lead to biased results?

Section 2.5, specifically L180-182: How was the comparison made, exactly, from a quantitative or statistical standpoint? Using contingency tables or crosstabs? Or something else? This is a bit unclear to me. . .. Maybe because I was expecting a statistical comparison when in fact, it is not what was done. . .

Figure 3: The different (graphical) features of the boxplots should probably be described in the figure caption. I assume that the horizontal black lines refer to the medians.... what do the whiskers represent, though: 1 interquartile range (IQR), 1.5 IQR, min and max values, or something else? Are there no statistical outliers associated with each cluster, i.e., each individual box?

P9 L203-204: That should not be a surprise, given that the flood and drought definitions are hydrograph-based.... or am I missing something?

P9-10, L207-210: The text description, here, does not underline that strong of a contrast between the weak winter regime and the strong winter regime. Maybe it can be rephrased for the contrast to be expressed more strongly?

P10 L217-218: That would explain why there is such a large degree of spatial contiguity/spatial autocorrelation within each cluster. However, it is a bit unclear to me, from the text, whether a RF classification using climatological variables only performs equally as well as – or better than – a RF classification that used both climatological and physiographic variables.

P10 227: The authors stated that "However, our clustering scheme avoids the formation of very small clusters seen in Jehn et al. (2019)." First, what might explain this? Second, the authors seem to imply that having very small clusters is an inconvenient, and I am not sure I agree – very small clusters could represent very local conditions or hotspots, which are real. The authors should either rephrase or at least nuance their statement to clarify what they mean.

P10 L230-234: The authors wrote that "The strong link between regime classes and meteorological and physiographical catchment characteristics allows for the attribution of ungauged catchments, where streamflow data are not available, to one of the regime classes, which is potentially very useful for the prediction of streamflow characteristics in ungauged basins”. I am not sure where that statement is coming from, as ungauged catchments were not examined in the present study. I agree that the present study might have interesting implications for predictions in ungauged catchments, but this statement, as written, reads as a result when in fact it is an interpretation.

In the same line of thought, I wonder whether it would be possible to have separate Results and Discussion sections in the manuscript. There are a few instances, in the text, where it can be tricky to distinguish whether a plain result/fact is being stated, or whether a hypothesis/interpretation is being put forward.

Figure 4: This figure is quite interesting but the comparison of "climate sensitivity" between observations and simulations appears quite qualitative. I wonder: 1) How were the five example catchments showcased in this figure chosen (or, are those sites representative of median cluster conditions)?; and 2) Was a quantitative method of comparison between observations and simulations used for all catchments?

P11 L240: The authors refer to a “visual analysis”; were all plots for all 605 catchments visually analyzed?

P11 L243-244: The Methods section should explicitly state what the Kolmogorov-Smirnov test was used for, the assumptions being it, and the null and alternate hypotheses (so that readers know what the test results mean). Also, a test cannot be rejected: we can only reject or fail to reject a null hypothesis, so that sentence should be reworded.

Figure 5: Lines are a bit difficult to distinguish on this figure; making it larger and changing the symbology might help.

P12 L258-259: The authors wrote “In contrast, regimes with a strong seasonality such as strong winter and New Year’s regimes are well simulated”. What about the melt regime, which is also highly seasonal?

Figure 7: If the black circles mean no regime change, the legend should state so.

**

COMMENTS SPECIFIC TO DISCUSSION ELEMENTS WORTH INCLUDING IN THE MANUSCRIPT

Discussion comment #1: In the present study, regime clusters appear equivalent to clusters derived based on physiographic similarity and clusters derived based on climatological similarity... this is contrary to studies published by Ali et al. (2012) and Oudin et al. (2010) – in a comforting way, I might add – and this should probably be discussed. The "overlap" or agreement between the different classifications bodes well for using climatic and physiographic information as a proxy for streamflow regime types. The fact that an agreement was found in the present study and not in others may be due to the fact that here, functional data were used instead of select streamflow indices.

Discussion comment #2: It is not a study limitation per se, but the authors may want to discuss the rationale for using functional streamflow data classification (to preserve temporal information) while NOT using climate timeseries (e.g., mean annual hyetograph) for classification purposes. When I started reading the manuscript, I was puzzled by the fact that a classification based on temporally autocorrelated data (i.e., whole annual hydrographs) was going to be compared to a classification based on climate indices. In other words, I wondered how the analyses would turn out given that different regions may have similar values of mean annual precipitation, even though the temporal distribution of that precipitation may be skewed in some places but not elsewhere. In the end, the authors found that they could neglect the temporal information included in climate timeseries and still manage to use that climate information (i.e., the climate index class) as a good proxy for streamflow regime class (which, itself, is based on temporally autocorrelated data). That warrants discussion, I think, as it is a bit counter-intuitive (to me, anyway. . .)

Discussion comment #3: The authors may want to use the concepts of resistance, resilience and synchronicity discussed by Carey et al. (2010): those concepts partly echo what the authors are referring to as "climate sensitivity".

**

EDITORIAL SUGGESTIONS

P2 L30: “illustrate the hydrological functioning” seems more appropriate than “govern the hydrological functioning”, since the authors are referring to streamflow regimes

P2 L31: I think that the phrase “influencing streamflow variability” should be changed.... Otherwise the whole sentence read as “The characteristics of streamflow regimes [influence] streamflow variability and seasonality”, which reads as a circular statement.

P10 L217: “shows that the the most important variables for” SHOULD BE CHANGED FOR “shows that the most important variables for”

P11 L243: “Klomogorov–Smirnov” SHOULD BE CHANGED FOR “Kolmogorov-Smirnov”

P13 L274: “In contract” SHOULD BE CHANGED FOR “In contrast”

**

REFERENCES CITED IN THIS REVIEW

Ali, G., Tetzlaff, D., Soulsby, C., McDonnell, J. J., and Capell, R. (2012), A comparison of similarity indices for catchment classification using a cross-regional dataset. Advances in Water Resources, 40, 11-22. doi:10.1016/j.advwatres.2012.01.008

Carey, S.K., Tetzlaff, D., Seibert, J., Soulsby, C., Buttle, J., Laudon, H., McDonnell, J., McGuire, K., Caissie, D., Shanley, J., Kennedy, M., Devito, K. and Pomeroy, J.W. (2010), InterâĂŘcomparison of hydroâĂŘclimatic regimes across northern catchments: synchronicity, resistance and resilience. Hydrological Processes, 24: 3591-

3602. doi:10.1002/hyp.7880

Oudin, L., Kay, A., Andréassian, V., and Perrin, C. (2010), Are seemingly physically similar catchments truly hydrologically similar? Water Resources Research, 46, W11558, doi:10.1029/2009WR008887

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-54, 2020.

Printer-friendly version

Discussion paper