Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
https://doi.org/10.5194/hess-2021-186-RC1, 2021

Comment on hess-2021-186

Anonymous Referee #1

Referee comment on "Assessing hydrological sensitivity of grassland basins in the Canadian Prairies to climate using a basin classification–based virtual modelling approach" by Christopher Spence et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-186-RC1, 2021

Summary

This study aims to demonstrate the utility of a hybrid classification and virtual modelling approach for assessing the sensitivity of a portion of Canadian Prairie catchments to climate. They first developed a class-based virtual basin model for a portion of the Canadian Prairie and then used the virtual basin to explore the sensitivity of hydrological response to climate.

Assessment

I enjoyed reading this manuscript. Class-based virtual basin modeling approach is an innovative tool to roughly identify regional-scale landscape vulnerability. I find the manuscript provides some interesting material for readers of HESS. This seems an appropriate topic to further generalize and extrapolate basins hydrologic functions. However, I think this paper needs major revisions and clarification on the rational of the study, and manuscript presentation and methodology. After these issues are resolved, I believe this paper could be a nice addition to HESS.

Major Points:

- This paper uses only one class of basin developed in the classification approach published by the authors before. This class includes a portion of Canadian PPR. Almost up to the middle of the paper this point is not very clear.

As an example, in line 109-111 the objective of the paper was explained as: "This paper aims to demonstrate the utility of a basin classification–based virtual modelling approach for assessing the sensitivity of Canadian Prairie catchments to climate". But they only explored one class out of 7 classes of catchments within Canadian PPR. I suggest that they explain only one class throughout the paper and focus the text to this class only.

- The point that the classification approach they developed in the previous published work is insensitive to inclusion of climatic features could show the inability of classification approach in categorizing basins based in their functional similarities. The timing, rate, frequency and type of precipitation could be the most dominant factors controlling how a landscape functions irrespective of their physical characteristics.

- The major assumption of the paper is that the authors previously published classification approach can classify landscapes based on (dis)similarity in their hydrologic functions. However, *Oudin et al.* [2010] clearly demonstrated that physically similar catchments, with similar physical features, are not truly functionally similar within the context of basin classification. As they used physical features to classify catchments, it is not clear if their classification is functional.

- If one of the objective of the paper is to demonstrate the utility of a new regionalization approach (e.g., Class-based virtual basin modeling approach) below points should be clarified and discussed in the introduction, method, and discussion sections:

- The rational for needing such approach: In which way the approach works better than other regionalization approaches in the literature (see *Blöschl et al.* [2014] for details of different regionalization approach).
- How computationally efficient is the method? Is it faster than, for example, the approach suggested in *Knoben et al.* [2018] that only used three functional indices to globally regionalize basins streamflow signatures. If not, why we have to use the proposed method? Does it respect functional behaviour of basins stronger than other methods? How accurate is their method compared to the other regionalization schemes? The paper shows some graphs related to the accuracy of the method. But there is no quantitative and/or qualitative comparison with other methods.

- There is a dearth of literature on the impacts of climate change and wetland drainage on streamflows of PPR watersheds in both USA and Canada. The authors mostly referred to their previous papers in this regard. In both introduction and discussion, other groups' works must be acknowledged. It should be clarified, how their findings are (in)consistent with other works.

Minor Points:

Line 44: How long does it take to run one model. How long would it have taken to run a

model for every catchment in the database or every catchment in the cluster? No talk of efficacy of virtual experiments in discussion, yet it seems like one of the central purposes of your paper in the introduction.

Line 139: Why would there be bias

Lines 209-210: Why is the maximum threshold less than minimum

Line 193: (Shook et al., 2013) lots of other places where comma is missing as well

Line 283: Why don't you look at cluster center (compare median catchment with virtual model)

Line 434: To me advance in max snow depth date implies further in time, i.e., closer to summer. Rephrase.

Line 517: should it be "temperatures , but these patterns"?

Blöschl, G., M. Sivapalan, M. Wagener, A. Viglione, and H. Savenije (2014), *Runoff Prediction in Ungauged Basins*, Cambridge University Press.

Knoben, W. J. M., R. A. Woods, and J. E. Freer (2018), A Quantitative Hydrological Climate Classification Evaluated With Independent Streamflow Data, *Water Resources Research*, *54*(7), 5088-5109.

Oudin, L., A. Kay, V. Andréassian, and C. Perrin (2010), Are seemingly physically similar catchments truly hydrologically similar?, *Water Resources Research*, *46*(11).