

## ***Interactive comment on “Analysis of projected hydrological behavior of catchments based on signature indices” by M. C. Casper et al.***

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The paper has received three review reports and a common theme has emerged from these reports: the paper will benefit from a major revision that aims to improve the presentation of the study’s objectives, hypotheses and research questions, methods and materials, a detailed discussion of the results in light of the objectives, and a conclusion section. After reading the paper I agree with the reviewers’ comments that the authors fail to explain what the objectives of the study are and what major results emerge from the analysis.

The authors define 9 hydrologic signatures derived from the flow duration curve and the event runoff coefficients. They argue that these 9 signatures are useful to detect

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hydrologic change, in the sense that any catchment response change resulting from changes in climate forcing can be quantified by comparing the signatures from a reference period and from future climate scenario runs. That may well be the case but I don't think the authors have sufficiently demonstrated this. I did not see a statistical analysis that reveals the power of detection of hydrologic change, merely a comparison of signatures as bar plots and a very qualitative assessment of the observed differences. Why are these signatures sensitive to change and why are these the best set? I'm not saying they are not, but at least I need to be convinced with scientific arguments that these signatures are what people should be using in order to perform, for instance, climate change studies. The authors should justify why the 5 signatures derived from the FDC as proposed by Yilmaz will serve the purpose of hydrologic change detection, rather than model diagnostics (as the case in Yilmaz et al.) There may be good reasons for using these signatures, and they should be discussed. Same holds for the signatures derived from the event RC distributions. Also an analysis of co-variations between the signatures is required. I can imagine that some are redundant as they provide basically the same type of information. This would result in fewer signatures to be compared. More importantly, are these 9 (or less) signatures sufficient to capture all hydrologic change? What about extreme response? timing of runoff generation? Shifts in runoff generation? All that is promised in the abstract of the paper but very little discussion of results is given in the Results section. This needs to be improved.

There is a confusing and unnecessary discussion on bias correction, but this paper is not about bias correction, so I don't see the point. I understand that when using simulation data from regional climate models, precipitation and temperature need bias correction (and so do probably all other simulation variables) before they can be used as forcing to hydrological models that represent the response of catchments for which one wants to assess hydrologic change, but since the paper doesn't aim to present new findings regarding bias correction and the paper is not about assessing hydrologic change (authors' own words), I suggest to remove that discussion and reserve it for a follow-up paper.

In short, the authors need to make up their minds what this paper is about, clearly state those objectives and apply appropriate methods to investigate hypotheses and research questions. Without such major revisions I don't think this paper is acceptable for publication.

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