Hydrology and Earth System Sciences Discussions

Interactive comment on "Exploring the physical controls of regional patterns of flow duration curves – Part 3: A catchment classification system based on seasonality and runoff regime" *by* E. Coopersmith et al.

Anonymous Referee #3

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In this paper, the authors seek to develop a hydrologically based classification system of catchments using data from 428 MOPEX catchments in continental USA. They use four metrics (3 climatic and 1 hydrologic) with an automated clustering technique (iterative dichotomiser algorithm) to produce a hierarchical catchment classification system. The classification approach used by the authors is appropriate and they show some interesting geographic patterns of catchment grouping. However, I have some concerns about the way this study has been motivated. Below are my specific comments.

Specific Comments:

1) I found the rationale for performing classification to be rather weak. For motivation, the authors state that (P7087, Line 6) "...a catchment's regime curve (ensemble mean of the within-year variation of runoff) has a major impact on the shape of the FDC". This rationale might be good enough to motivate why some of your four metrics were chosen, but has nothing to do with why classification is required in first place. Moreover, if the authors consider the shape of FDC to be an important hydrologic property of catchments, why not use FDC directly to perform classification?

2) In the current format, the introduction misses an opportunity to provide an overview of how previous studies have approached classification, and how the particular approach taken by the authors is going to be different/better than what has been done already. The references provided in the manuscript were either opinion commentaries [Dooge, 1986; McDonnell and Woods, 2004] or reviews [Blöschl and Sivapalan, 1995; Olden et al., 2011], and without any critique or thoughts on previous approaches. There is a rich history of hydrologic classification studies that are very relevant to the approach used by the authors (see Mosley [1981], Ogunkoya [1988], Burn [1997], Burn and Goel [2000], Sawicz et al. [2011], and several studies referred in Olden et al. [2011]). I believe that setting up the current study in the context of previous classification efforts will greatly strengthen the paper.

3) The authors state that they seek to develop a precursor to extending the Koppen-Geiger climate classification (basically a hydrologic equivalent). It might be helpful to provide more information about the Koppen-Geiger classification itself in the Introduction. Specifically, what variables did they use to perform classification? and what were the key reasons for their classification effort to be so successful? Was simplicity alone their strong point? This provides a strong motivation to extend this classification approach into hydrology (the primary aim of the authors).

4) P 7089, Line 22: I found the paragraph summarizing other studies in this series to

be a distraction from the classification message of this paper.

5) P 7092, Line 8: "While this image does provide useful information about the within-year (daily) variability of the chosen variables, for the purpose of catchment classification in this paper, a sliding, 30-day moving average is generated". Please provide a rationale for using a 30-day moving average filter.

6) The following are fairly sweeping statements without any back-up or citations from the authors: P 7093, Line 17: "These four variables are chosen not only because they are succinct descriptors of processes that underpin seasonality of runoff, but also because they represent the minimum amount of information that is needed to classify regime behavior within the continental US". P 7097, Line 25: "Our hypothesis in this paper is that a combination of the 4 similarity indices governs the regime behavior and can be the basis of their classification". First, it is not even clear if there is redundancy among the four chosen variables. For example, day of peak precipitation and day of peak runoff could be highly correlated in many, if not all, places. Therefore, minimality of the information content is a big unknown here. The study by Sawicz et al. [2011] offers a good approach into how this issue can be dealt with. Second, 3 of the 4 chosen variables are climatic metrics. This leads to an implicit assumption by the authors that climatic similarity is the primary controller of hydrologic similarity. While this has been shown to occur over large regions by previous studies (e.g., see Patil and Stieglitz [2012]), it would be helpful to explicitly state this assumption if the authors wish to limit the classification to these 4 variables.

7) P 7094, Line 1: "Once the classification system is established, even approximate or fuzzy answers to these questions can help towards a first-order classification of regime behavior, subject to further data collection and analysis". I do not understand the need for such a statement. There are many more questions in hydrology than the four chosen by the authors, and fuzzy answers to any of those questions will lead to a first-order classification. This is precisely the reason why there is no universally accepted catchment classification system.

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8) P 7102, Section 4.1: No rationale is provided for the class divisions of the four variables. For instance, why does aridity index have 5 classes, but seasonality index and day of precipitation peak have only 3 and 4 classes respectively? This has important implications on how the clustering results are interpreted.

Minor Comments:

P 7089, Line 4: Is regime behavior the same as regime curve? Please use consistent terminology.

P 7092, Line 4: "The fast and slow flow components were obtained by the application of a standard baseflow separation procedure". I did not see application of fast and slow runoff components later in the classification. These can be removed from Figure 1.

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