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Interactive comment on "A novel algorithmic framework for identifying changing streamflow regimes: Application to Canadian natural streams (1966–2010)" *by* Masoud Zaerpour et al.

Anonymous Referee #2

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The paper presented by Zaerpour and colleagues proposes a new framework for identifying shifts in streamflow regimes with a subsequent application to Canadian streamflow. While the methodical challenge and the chosen case study are clearly intersting, the convoluted nature of both the methodology and the presentation of the result prevent me from recommending the paper for publication in its present form. Below, I summarize my main points that need to be clarified.

A: METHODOLOGY.

A.1 Comment on clustering and change detection: The authors propose to use a fuzzy clustering algorithm to first identify groups of stations (or degree of membership of each station to each group) within a matrix of Indicators of Hydrologic Alterations (IHA) for a given – short – time window. Subsequently the degree of membership if each station to each class is computed for further time-frames. Overall, I can follow this approach and on first reading the description of the methodology makes sense (note a minor issue mentioned below). However, I have several questions regarding the choice of this particular approach and the stability of the analysis:

Question A.1.1: How stable is the estimate? The results of the analysis are crucially dependent on the identification of clusters in the first period. However, the streamflow climatologies (or IHAs) used for estimating these clusters are only computed using a small fraction of the available data, which is likely to yield unstable estimates. As a consequence investigating shifts in these clusters may be confounded by estimation errors. For example, I wonder if the authors would reach the same conclusions if clusters would have been identified using another period. Since the paper does not report on the stability of the estimate, it is hard to evaluate whether the overall conclusions are affected by the arbitrary choice of the first time window for identifying clusters (e.g. why not use the last window or one in the centre). Approaches for combatting this issue could be to (a) use all time windows for identifying the clusters and subsequently assessing how the degree of membership of each station to each cluster changes over time or (b) repeating the analysis with clusters identified for each time window and report the associated spread.

Question A.1.2: What is the benefit over a classical EOF/PCA analysis? Technically the analysis has distinct similarities to applications of dimension reduction methods such as Principal Component Analysis (PCA)/Empirical Orthogonal Function (EOF) analysis or Multidimensional Scaling (MDS) to spatially distributed time series. Of course, these methods do not evolve around the idea of "clusters" but identify modes of similar variability, but the strategy to first identify a membership matrix (analogue to "leading EOF patterns") which are then projected onto individual stations. In the EOF/PCA world, an analogue approach would yield a filtered time series at each station in which then again could be used to assess regime shifts without the need of developing a new (and somewhat convoluted methodology).

A.2 Why use a combination of Kendall's tau and R2 for the attribution work? To me it appears to be a bit convoluted to use two very different metrics (Kendall's tau and R2) for the attribution work. While Kendall's tau operates on ranks and is thus less sensitive to non-linarites or outliers, R2 is in essence a linear metric. As an alternative single metric I could e.g. imagine to rely on Spearmans rank-correlation coefficient together with a simple test of significance thereof.

A.3 Why not just look at trends in time series of monthly means and timing indicators? The analysis revolves around what the authors refer to as Indicators of Hydrologic Alterations (IHA). These are essentially a: The annual mean. b: the mean of each month and c: the timing of low/high flows. While reading I wondered if it would not have been sufficient to simply show maps of the trends of each of these metrics to arrive at the same conclusions?

A.4 Why stratify the analysis along large drainage basins. While I acknowledge the tradition of stratifying the analysis of streamflow data along drainage basins I wonder if this is the ideal choice in this particular instance. The regime classes identified by the authors essentially reflect different climatological regions (e.g. colder, snow dominated vs. warmer, rainfall dominated). Continental-scale drainage basins typically cover large climatic gradients and apart from the case where stations are hydrologically connected (how many of them are?), we would not expect a-priory the drainage basin would have much explanatory power on the climatology. Alternatively, I could imagine an assessment of changes in the underlying climate drivers (e.g. temperature, precipitation) would contribute to a deeper understanding of the associated changes.

B: PRESENTATION

Overall, I found the paper to long, a bit convoluted and therefore cumbersome to read. Some reasons:

1. The key selling point advertised in the title (i.e. the methodology) is featured in about 10% (4 of 39 pages) of the article and the properties of the methodology (e.g. stability or relation to alternative techniques) are neither assessed and nor discussed.

2. The description of the results is very convoluted and I found it difficult to extract the key message upon first reading. For example, I would value if the results description would focus on overarching patterns/conclusions instead of a diligent, but lengthy description of details.

3. I found figures 8,9,10,11 quite hard to assess on first reading. Would it be possible to summarize these results e.g. in sets of 6 maps (one for each cluster).

C: MINOR ISSUES

1. There is a significant number of grammatical mistakes (i.e. missing articles) in the paper that need to be resolved by the authors.

2. Text following equation 2c: V (matrix of centroids) is described twice. This indicates that the methods section might have been written in a sloppy manner, raising the question if everything is correct. I did not have the time to check all the indices etc. in detail.

3. How is the "timing of annual low/high flow" defined? Is it the day of year of the smallest/largest value? If yes: how is the discontinuity between day 365 and day 1 handled?

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