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Interactive comment on "Watershed classification for the Canadian prairie" *by* Jared D. Wolfe et al.

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General comments

1. This study uses clustering analysis to define regions of hydrologic similarity in the Canadian Prairies. The authors distinguish seven classes, using information on the region's climate, wetland traits, topography, land use, and streamflow. The authors use a range of techniques to reduce dimensionality of the data and create an attractive map of hydrologic/ecologic similarity in the Prairies. I appreciate the enormous effort that must have gone into this work, but I'm also left with several concerns that will need to be addressed, focusing on the validity of the clusters and clarification of the methods.

2. Discussion of the data preparation and initial analysis is extensive, whereas the assessment of how well the clustering has worked is comparatively small. For any practical application of their results, a measure of the validity and usefulness of these

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clusters is needed, beyond the discussion given in section 3.5 (which mainly lists which attributes were most informative in creating the clusters). A quantification of usefulness through independent data, or an example application of these clusters as a management tool would improve this manuscript by a large margin. I have outlined a possible option in the specific comments.

3. The methods section is also partly used to introduce data, and to define the metrics used for classification. Readability of the manuscript might be improved if these two are separated (i.e. a data section followed by methods) or if the authors provide a table that lists their classification metrics, the data the metrics are based on, and the hydrologic relevance of each metric. Currently, it is not entirely obvious to me why certain metrics are included in the analysis and why others, that seem obvious candidates to me, are not (detailed in the specific comments). I assume this is obvious to experts on prairie hydrology, but a little more background would make the manuscript accessible to a wider audience.

4. The authors present a lot of information in section 2, and not all readers will be equally familiar with all concepts. A bit more background might help those without extensive knowledge about the specific methods used in this work. Additionally, the results section could benefit from a brief introduction (or figure) that outlines what is coming. E.g. "First, in section 3.1 we use CCA to . . . Next, we reduce the dimensionality of our problem through PCA (section 3.2). . . .".

Specific comments

1. International readers might not be able to place the Canadian Prairie on a map (line 55). A brief statement about the geographical extent of the Prairies would help.

2. "Hydrological characteristics" (line 71) is unclear. Do the authors mean catchment attributes (e.g. topography, soils), climatic conditions, statistical properties of the streamflow regime or something else?

3. It would be helpful for the reader to briefly summarize how well earlier classification attempts have worked (line 74-78) and where the authors see current challenges.

4. The HydroSHEDS webpage (https://www.hydrosheds.org/page/development) lists a few regions where the data set is prone to errors, including areas with low or not well-defined relief. Is this of concern in the Canadian Prairies?

5. Approximately how many meters are 15 arc-seconds (line 140) in this area?

6. What motivated the choice for these specific area (line 142) and urbanization (line 143, Table S1) thresholds?

7. The spatial resolution of climate data (line 157) seems large compared to the resolution of the watershed boundaries. Can climate data on this resolution still be considered representative for the smaller catchments?

8. What is the rationale for choosing the Thornthwaite method (line 161)?

9. Snow formation and melt can strongly influence the seasonal water distribution and accounting for the fraction precipitation that occurs as snowfall has recently proved valuable in hydrologic similarity research (Knoben et al, WRR, 2018; https://doi.org/10.1029/2018WR022913). Is there any particular reason why the authors use only mean P and ET in their clustering?

10. What is meant with a wet cycle (line 176-177)?

11. Please include a (short) definition of potholes (line 177).

12. Why is the Lw/Lo metric (line 184) relevant? What does this metric tell us about watershed behaviour?

13. The climate data (line 156), land cover data (line 230 and further) and hydrological data (line 252 and further) cover different periods in time (1970-2000 for climate, 2011/2016 for agriculture land use, 1990-2014 for hydrologic data). For a general classification of similar regions, overlapping time periods for the data sources would be

more appropriate. What is the rationale for not doing this?

14. Estimation of mean flow Q2 and flood Q100 (line 252) for 4175 watersheds using only 11 stations (line 255) seems ambitious to me. Spence and Saso (2005) show a significant uncertainty in their predictions. Can the authors provide a statement about their confidence in the Q2 and Q100 estimates?

15. What is the reasoning behind the 80% threshold for PCA components (line 279)? Perhaps the authors can include a plot or table that shows the importance of each PC to support this choice.

16. Were variables standardized to a fixed interval (e.g. [0,1]) in addition to the log-transform (line 282)?

17. Line 286-287 needs clarification. Which variables are the "complete suite of variables"? The previous section gives the impression that all variables were converted to PCs, of which only those above 80% would be used. A table with a summary of all variables used, their data source(s) and their hydrologic relevance could help clarify what is going on.

18. Retaining PCs above 50% (line 291) seems to contradict retaining PCs above 80% (line 279).

19. A short description of Ward's criterion (line 295) would be helpful.

20. I suggest replacing "and thus did not explicitly affect the clustering analysis" (line 303) with "and are not included in the clustering procedure" (assuming that I correctly interpreted this sentence).

21. Not all readers will be equally familiar with canonical regression analysis. I find it difficult to interpret the results in section 3.1. A (very) brief description of CCA might help. Some questions I'm stuck with: are those lambda values high or low? What would either tell us? What does it mean that hydrologic variables are associated with W2?

22. I would say these regressions are not particularly convincing (line 314). It looks as if the one high value could be inflating the correlation value. Did the authors use Pearson or Spearman correlations? Predicting streamflow characteristics in ungauged basins (i.e. regionalization) is an active field of study but achieving robust results has proven very difficult. How does this impact the extrapolation of this information to the 4100+ watersheds and what are the consequences for the subsequent analysis?

23. Section 3.2 (PCA results) lacks a logical conclusion (or perhaps an introduction). How did the authors choose how many PCAs to discuss and which PCAs are selected to be used in subsequent steps?

24. The difference between active and supplementary variables needs to be defined (line 348).

25. Section 3.3 lacks a logical conclusion. Which PCAs are carried over to the clustering analysis?

26. What do the authors mean with “definition of clusters” (line 370)?

27. Section 3.4 is very brief. One of the main aspects of clustering analysis is assessment of how good the resulting clusters are. Currently the authors extensively list the differences between the clusters (section 3.5) by summarising which inputs were most influential in determining the clusters. However, this only tells us something about the patterns in the data and not much about the usefulness of these clusters. The authors suggest in the discussion that these clusters can be helpful to inform management decisions, by showing which regions are expected to behave similarly and which regions are not. This statement should be backed up by proof with independent data that these cluster indeed show that. The GSIM archive (Do et al, HESSD, 2018; https://doi.org/10.5194/essd-10-765-2018) is a recent contribution of global streamflow indices which might provide the authors with independent hydrologic information that they can use to quantify how well their clusters group hydrologically similar regions. See e.g. Knoben et al, WRR, 2018 (linked above) for possible ideas.

28. The subsections of section 3.5 are hard work for an international audience. Perhaps figure 5 can be expanded to include a map which shows the various names used in these sections (see e.g. Addor et al, HESS, 2017; figure 1e; https://doi.org/10.5194/hess-21-5293-2017)

29. Line 435-437 ("Being river valleys . . . Q2 values (Tabl 1)) repeats line 428-429.

30. I'm unsure how section 3.6 relates to the previous clustering results. I was under the impression that wetland density is one of the variables used during clustering. Should section 3.6 perhaps be moved to before the clustering results? Also, if this is part of the clustering analysis (as e.g. table 3 and 4 seem to suggest), why does this specific attribute deserve its own section? Edit: reading back, it seems to me that wetland distributions were estimated (line 186 and further). In that case, are the observations referred to in line 480 from the 11 stations? This seems a small sample of observations to compare results for 4100+ watersheds to. How confident can we be in these estimates?

31. The authors stress the importance of accounting for human influences (Section 4.1) in classification procedures. Can they comment on the extent to which this was done in their work and do they have any recommendations for future efforts? For example, should artificial drainage density be considered as a variable?

32. The authors mention that certain variables can dominate the clustering approach (line 579 and further). This is why it is not uncommon to standardize clustering variables to a fixed interval, because this reduces the effect of a variable's variability. Log-transforms lessen, but do not prevent this. Can the authors comment on which variables had the widest (log-transformed) range and whether this correlates with the variables that are most important during clustering?

Technical corrections

1. "van der Kamp" (line 48 and others) should be "Van der Kamp ".

2. Figure S1, c: text in the centre overlaps and is unreadable.

3. “described” (line 342) should be “describe”.

4. Figure 6b: the number of points make this plot difficult to read. x-axis should be changed to cover the width of the page. Possibly cut of the y-axis at 10 for additional clarity.

5. “Bering” (line 435) should be “being”.

6. Figure 7. Readability would be improved if the numbering of classes is placed in front of the class name (like was done for subplots a, b, c).

7. Figure 8. Suggest changing “solid” to “dark” and “transparent” to “light”.

8. Figure 8c. A boxplot seems more appropriate than these bar plots given the information presented.

9. Line 540. Remove “a”.

10. Line 603. Change to “. . . may inadvertently result in the presence of smaller wetlands being perceived as . . .”

11. Line 662-663. Some words need to be removed to make the sentence make sense.

12. Line 696-697. This sentence needs rewriting.

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