

Hydrol. Earth Syst. Sci. Discuss., author comment AC3
<https://doi.org/10.5194/hess-2020-613-AC3>, 2021
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Reply on RC3

Paul H. Whitfield et al.

Author comment on "The spatial extent of hydrological and landscape changes across the mountains and prairies of Canada in the Mackenzie and Nelson River basins based on data from a warm-season time window" by Paul H. Whitfield et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-613-AC3>, 2021

HESD 2019-613 R3

The authors of this studied the data from several stream gauges in western Canada, analyzing several aspects, like regime types, trends and their relation with trends in vegetation, water and snow availability derived from satellite.

The paper is of clear scientific relevance, the content is interesting and the methods are generally well explained (with maybe a few exceptions, see below).

The main issue that I found is the extent of the manuscript, which is too long (~15000 words) and detailed, in a way that a reader can get lost in many small details and miss the interesting aspects. Starting from the abstract, which is almost 500 words long, and IMO fails to summarize the message of the paper and its scientific relevance. My suggestion is a major reshuffle of the text, making it more compact and streamlined, and concentrating a bit more on the important aspects of the research, possibly moving the details to dedicated paragraphs in the supplementary information.

[Authors response] We appreciate the reviewer having taken the time to provide comments our manuscript.

More detailed comments below-

The abstract is very long, with too many methodological details. A much shorter account

of the used methods should be given, while the output and its scientific relevance should be highlighted more.

- I.138: "The overlap between hydrological ..." not clear what you mean here.

[Authors response] "The overlap between hydrological and satellite index trends were not consistent across the study area."

Replace with:

"Hydrological trends and satellite index trends were not consistent across the study area, or sometimes within watersheds."

- I.180: for clarity I would state explicitly that $p=0.05$ and 5% can be considered linked, as the p-value is a guess on the probability of the null hypothesis.

[Authors response] Replace: "A threshold of 0.05 was used in tests of significance, and 5% was also used as an indicator that the number of trends exceeds the number expected by chance alone. "

With:

"A threshold of 0.05 was used in tests of significance, and accordingly, 5% was also used as an indicator that the number of trends exceeds the number expected by chance alone."

- I.185: table 1 is rather ugly and not of real scientific content, IMO does not show well in the main manuscript, I would move it to the SI.

[Authors response] "Table 2 provides the starting dates of the five-day period corresponding to each 5-day period number from April through November."

A previous HESSD reviewer asked for such a table to be added so the reader could connect periods and actual dates. Similar tables have been provided in other papers (e.g. Rickenbach et al. 2020). It needs to be in the main paper as a reader should not have to go to Supplementary Material for this table.

Rickenbach, T. M., Ferreira, R. N., and Wells, H.: Springtime onset of isolated convection precipitation across the southeastern United States: Framework and regional evolution, Monthly Weather Review, 148, 891-906, 2020.

- 190: I find a bit difficult to follow this numbering of the 5-days period, also because sometimes the numbering is shifted (e.g. period 23 becomes period 1). Would it be meaningful to label the 5-day periods with the starting date? E.g. the period that starts with the 1st of March would be 03-01. This would make the discussion and the figures clearer.

[Authors response] While we partially agree with this comment, the shift that is made (where period 23 of the year become point 1 in a time sequence) is due to software package and the manner in which it frames the input data. The dates associated with periods are shown in all plots so referring to them in the text in the manner suggested seems to make things more, not less, complicated.

- Figure 2 and others (also in the SI): the authors should label the panels with ids, e.g. a, b, c, and in the text reference the panels instead of referring to the "bottom left" panel.

[Authors response] Figure 2, and others that are similar, are single figures with coupled panels, not separate panels. While we understand the convention we have chosen to leave the description as originally stated.

- I.239: ".. unable to group hydrographs when they are not aligned in time" depends on the metric you use in k-means?

[Authors response] "Statistical methods, such as k-means (Likas et al. 2003; Steinley 2006) or self-organized maps (Kohonen & Somervuo 1998; Hewitson & Crane 2002; Kalteh et al. 2008; Céréghino & Park 2009; van Hulle 2012), are unable to group hydrographs when they are not aligned in time (Halverson & Fleming 2015)."

The reviewer is incorrect, the k-means metric is not the issue as k-means performs Euclidean distance from point to point and we have used dtw to cluster the median hydrograph, not k-means.

- I.261: It is not entirely clear what metric was used in the k-means (and subsequently, the mathematical reason of why you do need a DTW). Furthermore, the results of k-means depend on the initial guess of the centroids, the seeds. How did you select them?

[Authors response] We did not use k-means in this section and have stated that in lines 244 to 248:

"The clustering of annual median streamflow time series was done using dynamic time warping, DTW, (Berndt & Clifford 1994; Wang & Gasser 1997; Keogh & Ratanamahatana 2005) which measures similarity between time series that may vary in magnitude and timing by aligning the two standardized (zero mean, unit variance) curves in time, essentially matching the shape of inflections to create clusters (Sarda-Espinosa 2017; Whitfield et al. 2020)."

- I.270: I would rephrasing stating more clearly that a trend was estimated for each of the 5-days time periods.

[Authors response] "Trends in the five-day periods for the annual common time window were determined for the period of record of each time series, using Mann-Kendall tests as described above, following the approach of Déry et al. (2009) for examining trend magnitude for a fixed endpoint in time."

Changed to:

"Trends in each of the five-day periods for the annual common time window were determined for the period of record of each time series, using Mann-Kendall tests as described above, following the approach of Déry et al. (2009) for examining trend magnitude for a fixed endpoint in time."

- I.283: Also here the metric used in the k-means and the seed method should be explained.

[Authors response] "The individual annual trend scores for the annual common time window for the 395 stations were clustered using the method of k-means, which partitions observations into clusters having similar means and which is well suited to clustering of features such as patterns of significant differences (Likas et al. 2003; Steinley 2006; Agarwal et al. 2016). The number of clusters chosen (six) was based upon the elbow method (Ketchen & Shook 1996; Kodinariya & Makwana 2013); using more than six

clusters did not improve the modelling (not shown). "

- I.289: these figures are in the SI, why pinning them here?

[Authors response] Sorry if this was confusing. This was simply tracking in the manuscript where each figure and supplementary figure was used. These will be removed in the final version.

- I.306: capable -> useful

[Authors response] "Google Earth Engine (GEE) allows for cloud-based planetary scale analysis while it serves as a database for petabytes of open access satellite imagery such as the Landsat archive (Google Earth Engine Team 2017; Gorelick et al. 2017), and is particularly capable for this study. "

Change to:

Google Earth Engine (GEE) allows for cloud-based planetary scale analysis while it serves as a database for petabytes of open access satellite imagery such as the Landsat archive (Google Earth Engine Team 2017; Gorelick et al. 2017), and is particularly useful for this study.

- I.318 on: landsat composites for NDVI, NDWI, NDSI should already be already available in gee. Did the authors recompute them?

[Authors response] "For each basin, three time series of spectral index averages were derived from the 16-day mosaics of Landsat 5 TM data."

We are unsure what the reviewer's point is as this text describes that we simply created time series of basin averages of the indices that are available for each pixel using mosaics we created from Landsat imagery.

- section 3: this section is very long, I believe it would be possible to greatly reduce and summarize it, putting the details (e.g. the detailed description of each streamflow regime and its spatial distribution) in the SI.

[Authors response] We agree that this paper is long, but we disagree with this suggestion. We believe such a move would make the presentation even more difficult to

follow, since the reader would have to switch back and forth between the main paper and the supplementary material and that, in our opinion, would be detrimental.

- I.357: the dashed lines are plotted below the other lines and you cannot see them for many clusters.

[Authors response] Figure 6 "Each of the twelve plots contains a line for each gauged basin in that Type and the heavy dashed line is the centroid of all members; the colour of the lines is based upon stationID. "

This is an issue with the originating software that uses ggplot where the centroid is plotted first and over-plotted with the individual lines. We have changed the description to be more precise:

"Each of the twelve plots contains a line for each gauged basin in that Type and the heavy dashed line, where visible is the centroid of all members; the colour of the lines is based upon stationID."

- Figure 8: isn't this figure a repetition of figure 6 (if figure 6 was adjusted to show the centroids, now the dashed line is often hidden).

[Authors response] Yes, Figure 8 shows only the centroid that is not always visible in Figure 6. This is stated in lines 416:

"The standardized streamflows plotted in Figure 6 make it difficult to compare the Streamflow Regime Types; plotting the z-score centroids of each (Figure 8) makes the comparisons simpler."

- I.397: this hydrograph, what hydrograph?

[Authors response] "Streamflow Regime Type 5 basins were also common in the Prairie ecozone (n=49), in the Boreal Plains (n=81) as well as along the Mackenzie River to below Great Slave Lake; this hydrograph shows an earlier and briefer peak than Type 1 with a rapid recession (Figure 6)."

Changed to:

"Streamflow Regime Type 5 basins were also common in the Prairie ecozone (n=49), in the Boreal Plains (n=81) as well as along the Mackenzie River to below Great Slave Lake; the hydrograph for this Type shows an earlier and briefer peak than Type 1 with a rapid recession (Figure 6)."

- l.414: the closing parenthesis at the end of line has no opening match.

[Authors response] "While these descriptions are explicit, there was overlapping of types in space (particularly Streamflow Regime Types 2 & 5), and cases where individual basins of a Type occur quite separately from each other (Types 9 & 12) as is evident in Figure 7."

Changed to:

While these descriptions are explicit, there was overlapping of types in space (particularly Streamflow Regime Types 2 & 5), and cases where individual basins of a Type occur quite separately from each other (Types 9 & 12) as is evident in Figure 7.

- l.446: bsin -> basin

[Authors response] Changed.

- l.477: remind here briefly that the expectation "by chance alone" raises from a choice of $p=0.05$

[Authors response] "For a period of 10 years, 5% of the cases show significant trends, as would be expected by chance alone."

That choice was stated earlier in the paper, but could be repeated here.

For a period of 10 years, 5% of the cases show significant trends, as would be expected by chance alone (based on a p value of ≤ 0.05).

- paragraph 3.3: the trends in sats indices look weak, furthermore, you did not prove that the trends may be enhanced or hindered by multi decadal oscillation (while for the streamflow trend this has been proved). I believe the discussion comparing with the expected probabilistic 5% threshold is acceptable, but this limitation should be mentioned.

[Authors response] "The spatial patterns of trends in the mean values of the three normalized difference indices are presented in Figure 13. The spatial patterns of the trends in the maximum, mean, and minimum of NDVI, NDWI, and NDSI are provided in Figures S22-S24 and are also summarized in comparison with Streamflow Regime Type (Table 7), Trend Pattern (Table 8), and Ecozone (Table 9). The tables show the fractions of stations grouped by trends that were significant at $p \leq 0.05$. In the figures significant trends ($p \leq 0.05$) are shown as red (decreasing) or blue (increasing) triangles, trends whose significance was ≤ 0.10 are shown as red or blue dots, and those with no trend are plotted in black. There was a stronger association of the trends in the three indices with

spatial location and with ecozones than with Streamflow Regime type or trend pattern. Frequently, the trends in vegetation, water, and snow satellite indices occur in a spatial domain that follows the margin between two or more ecozones (Figures 13 & S22-S24)."

This is Results section 3.3 and the reviewer's comment is really related to the discussion where a section on PDO and AO is included at lines 969-979.

- section 4: also the discussion is a bit overextended. Furthermore, some concepts that were already mentioned in the results section are repeated here.

[Authors response] Gosh, if we need to remind readers that the 5% is related to a $p \leq 0.05$, then we need to remind the reader of these details.

- l.713: remove one of the 2 "that"

[Authors response] Removed

- conclusions: I would suggest to shorten also the conclusions, providing a more summarized account of what has been done and the originality of the methodology, of the most interesting results, of their relevance for the scientific community and possible future development.

[Authors response] We disagree. The conclusion is a summary of a study of a large spatial domain with many watersheds and multiple facets being considered and several novel techniques. Each of these is summarized as briefly as is reasonable.