Hydrology and Earth System Sciences Discussions

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Comment on hess-2020-613

Anonymous Referee #3

Referee 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-RC3, 2021

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 a
spects. 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 de
dicated paragraphs in the supplementary information.

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 highlight
ed more.
- l.38: "The overlap between hydrological ..." not clear what you mean here.
- l.180: for clarity I would state explicitely that p=0.05 and 5% can be considered linked, as the p-value is a guess on the probability of the null hypothesis.
- l.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.
- 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 lable
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.
- 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.
- l.239: ".. unable to group hydrographs when they are not aligned in time" depends on the metric you use in k-means?

- l.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 in
itial guess of the centroids, the seeds. How did you select them?
- l.270: I would rephrasing stating more clearly that a trend was estimated for each of the 5-days time preiods.
- l.283: Also here the metric used in the k-means and the seed method should be explained.
- l.289: these figures are in the SI, why pinning them here?
- l.306: capable -> useful
- l.318 on: landsat composites for NDVI, NDWI, NDSI should already be already available in gee. Did the authors recompute them?
- section 3: this section is very long, I believe it would be possible to greately reduce and summarize it, putting the details (e.g. the detailed description of each streamflow regime and its
spatial distribution) in the SI.
- l.357: the dashed lines are plotted below the other lines and you cannot see them for many clusters.
- Figure 8: isn't this figue a repetition of figure 6 (if figure 6 was adjusted to show the centroids, now the dashed line is often hidden).
- l.397: this hydrograph, what hydrograph?
- l.414: the closing parenthesis at the end of line has no opening match.
- l.446: bsin -> basin
- l.477: remind here briefly that the expectation "by chance alone" raises from a choice of p=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 t
his has been proved). I believe the discussion comparing with the expected probablistic 5% threshold is acceptable, but this limitation should be mentioned.
- section 4: also the discussion is a bit overextended. Furthermore, some concepts that were already mentioned in the results section are repeated here.
- l.713: remove one of the 2 "that"
- 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 result
s, of their relevance for the scientific community and possible future development.