

## ***Interactive comment on “The Spatial Extent of Hydrological and Landscape Changes across the Mountains and Prairies of the Saskatchewan and Mackenzie Basins” by Paul H. Whitfield et al.***

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Interactive comment on “The Spatial Extent of Hydrological and Landscape Changes across the Mountains and Prairies of the Saskatchewan and Mackenzie Basins” by Paul H. Whitfield et al.

Anonymous Referee #1 Received and published: 27 February 2020 General comments: This is a really interesting, innovative, and valuable study that deserves to be published in HESS. The existing database of streamflow measurements in western Canada east of the continental divide is expanded, perhaps greatly, compared to earlier statistical studies of hydrologic change in the region by adding datasets from

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streamgages that are only operated seasonally (i.e. not in winter). Some intriguing new analytical methods are introduced, in particular dynamic time warping. Further, Landsat imagery is interpreted alongside the statistical hydrology results, giving additional insights. The region itself also warrants close study in light of its size and, especially in its northern reaches, relative lack of study compared to many other places. Unfortunately, the manuscript doesn't seem to do a great job of communicating the work, and it has the feeling of being only half-finished, as explained in some detail in the specific comments below. I therefore recommended publication of this (potentially excellent) article pending major revisions that entail among other things an extensive ground-up rewrite.

**Authors' response:** We appreciate these comments and the reviewer's concerns. We have endeavoured to make substantive corrections and changes to address these concerns in the revised manuscript. The manuscript has been extensively rewritten, reorganized, figures have been modified and some additional figures and tables added to address this reviewer's comments and suggestions. —

**Specific comments:** The abstract feels clunky. There are unnecessary details (e.g. clarifying that the Landsat imagery analysis covers a different timeframe from the streamflow data analysis) while at the same time some of the big punch lines from the study seem to be missing.

**Authors' response:** We think that it is important to make the reader aware that the satellite imagery is for different periods of time than the streamflow. We have rewritten the abstract so that it is no longer 'clunky', at least in our opinion. As to the "Missing big punch lines", we chose to be careful to not overstate the results, and in revision have made several more forceful. —

There are odd writing choices (such as using both quotation marks and capitalization for "Streamflow Regime Types" and "Trend Patterns" when neither of these terms/concepts is new in any way). And some of it is just plain confusing.

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Authors' response: We disagree with this comment. It is important that the reader understand when we are referring to members or either one of these two lists. In the revised abstract we have removed the quotation marks and capitalization but in the balance of the paper we use these to specifically refer to cluster/group membership. When the terms are introduced, quotes and capitals to highlight that we are using these terms in a very specific manner that is different from common practice. When used un-capitalized they are used with their common meaning. —

The use of data from rivers that are intermittent or ephemeral, because for example they're frozen completely or unmonitored in winter, is one of the more interesting and new aspects of this study. However, this dimension of the paper also feels like it hasn't been explored as much as it could and maybe should have. On the one hand, there may be a bit of missed opportunity here. The study doesn't seem to give much information on the main advantage of this approach, specifically, that it could hugely increase the sample size by pooling results across both perennial and intermittent/seasonal rivers; I don't see any numbers in the article about how much the available dataset is expanded by using this philosophy?

Authors' response: We agree that this is a key aspect of the paper since most studies only use stations with perennial flow and hence temporary streams are often overlooked. A classic trend study would have a few stations with perennial flow, and a common time window. The present study uses data from a large number of stations and includes both perennial and temporary streams, and does not use a common period of years. The revised manuscript provide details that address these points. Additional plots were added to the supplementary material to demonstrate missingness and dataset size for both continuous and seasonal stations. Text has been added to provide both more detail and more emphasis on the importance of this. —

And one of the most interesting questions that could be addressed in a study applying statistical change analysis to streamflow data from seasonal cold-regions rivers would be whether there have been changes in the annual no-flow duration and/or its dates of

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onset, etc; that hasn't been tackled here at all as far as I can see.

Authors' response: While we agree that this would be interesting, investigating no flow duration and its onset is far more complicated than the reviewer may expect. Data suitable for a no flow duration associated with winter is rare as most seasonal stations cease operation at the end of October and do not start until the following April. No flow also occurs in the both the cold and warm season in many of the seasonal stations, but addressing it is beyond the scope of the present paper. —

At the same time, the approach used (if I understand it correctly) has the disadvantage of looking only at the common seasonal period between each streamgage time series, which means that wintertime flow measurements are not considered; that's the tradeoff.

Authors' response: This is the case. The common element used to compare data from these stations is a seasonal time window that is common to all stations, the "annual common time window". Where data exists outside the window as at some stations, analysis is reported at the station level but all comparisons use the same seasonal window. In several places we comment that members of a Trend Pattern with continuous data share the same trend pattern outside the common window. The use of a common seasonal time window not a disadvantage as it provide the opportunity to compare data from a much large number of locations. —

While some of the rivers in the study don't flow in winter, many do. Given that some of the stronger changes in climate and hydrology at high latitudes and cold regions generally are being experienced in winter, certain important phenomena could be missed using this approach. That fact doesn't necessarily reduce the "publishability" of the paper, but it does imply that a clearer statement of scope and purpose ought to be made. Maybe I've just missed it (a real possibility given the clunky writing and organization of the paper) but I don't see anywhere in the title or abstract that the study is limited to the warm season and it doesn't look like there's much discussion in the paper as a whole of what this limitation means.

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Authors' response: In the revision we highlight the numbers of station and years included in this approach vs a common time period. We make the point in the abstract that the seasonal data is collected in the warm season. Could important phenomena be missed using this approach? This is probably the case for every trend study, but we clearly explain and limit what we are examining and how and others may wish to hypothesize further and conduct additional analysis. The increase in winter streamflow in the north is noted in the paper, and the fact that detecting trends in only the warm season created clusters where the winter streamflow was also increasing. Some text has been added to emphasize this point. —

Lines 100-103, phrases like “by integrating different forms of data, which previously had only been treated separately and individually” and “By linking continuous and partial year data from a large number of hydrometric stations using only warm season data three important questions are addressed. . .” I worry that many readers might feel that simply using data from summer, or looking at Landsat data alongside the streamflow data analysis, doesn't really count as “data integration,” which might be taken to mean something more ambitious than what was done here. It might be safer to simply say that an unusually large and diverse dataset was considered.

Authors' response: We have made wording changes, where appropriate, to make the data availability clear. We more carefully describe the nature of the ‘integration’ that is achieved. We added a figure to the main text identifying the large basins and the names of important rivers and the locations of continuous and seasonally operated stations. Figures showing missingness and data availability, for continuous and seasonally operated stations were to the supplementary material so that the reader will be able to see the complete data picture; basically a common time window approach would be limited to a ~25 year period for only ~130 sites. —

Not clear what lines 134-135 are intended to mean; typographical error?

Authors' response: Indeed a typo. Should read: A threshold of  $p < 0.05$  is used in tests

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of significance, and 5% is used as an indicator that the number of trends exceeds the number that would be expected by chance alone. —

The idea of not using a common period of record across all the streamgages runs contrary to almost all work in this field. That doesn't necessarily mean it's wrong, but it does seem likely to be viewed as a problem by some readers, and the subject probably requires more attention than it's been given in the article. For example, the authors write on lines 138-144 that "Because the periods of record, rather than a common period, are used it is not possible to compare the magnitudes of trends among the stations. Instead, the analyses are restricted to determining the existence of significant trends in individual five-day periods in (five-day) periods 23 to 61 (of 73), as shown in Figure 1 and 2." In addition to being a clunky and hard-to-read sentence, its logic seems doubtful. If the magnitudes of trends are not comparable between stations, why would the existences of trends be? One problem is statistical. Determining the existence of trend using a statistical significance test (as done here) simply amounts to a measure of the magnitude of the observed trend relative to what you'd expect to see purely by chance; the concepts of magnitude and existence can't be completely separated as the manuscript seems to suggest they can. Another problem is physical. There's a reason why statistical hydrology and climate studies of this sort normally focus on a common period of record, to ensure apples-to-apples comparisons. Especially in light of the fact that the minimum period of record seems to be a reasonable but not great 30 years, the observed trends could simply represent decadal (e.g. PDO) climate regime shifts, and because there's (apparently) different 30-year periods between the records, figuring out what these different trends at different stations actually means seems messy. There have been some studies that have gotten away with using (slightly) mixed periods of records, but these have been very geographically tightly focused studies of a dozen streamgages or less and evaluated those data in depth for very particular phenomena of interest. In such a large-scale pattern-recognition study as that presented here, though, it's not clear this works. A stronger case should be made for it, or at least a discussion giving better clarity on the pros and cons.

Authors' response: The main response to this comment is that this is indeed the novelty of this manuscript. We totally agree that if the interest was in determining trend magnitudes that a common time window and complete annual records would be used. Even if this is the case, however, the trends that are detected may be inappropriately interpreted as trends over time result from complicated changes within the climate system and include shifts in climate indices [AMO, NAO, PDO, and ENSO] that confound attribution. In the present study we simply focus on whether or not the five day periods on the common time window demonstrate a coherent trend pattern. Stations may not show a pattern for many reasons, the available record may be too short in some cases, but the trends reported are those present in each dataset. The reviewer should understand that this approach is also an apples to apples comparison just different to how many studies are done. We accept that our approach may be perceived as contrary to much of the work in the past. While we agree that it would be nice to have the most common apples to apples comparison even a common time window does not guarantee that that is achieved. Selecting a particular block of years does not guarantee that any trends that are detected are attributable to warming climate since the climate system is complex and variable. In our approach we focus on changes in the seasonal pattern of streamflow and whether those patterns have a spatial pattern. We disagree with the reviewer with respect to their point on separating trend magnitude and direction. These are not completely coupled. It is reasonable to separate direction from magnitude; in fact asking the question "Is there a trend?" does not imply a need to know how large. The nonparametric Mann-Kendall is used to analyze whether a series is consistently [monotonically] increasing or decreasing but itself does not resolve magnitude. To be able to compare magnitudes it would indeed need to have a different apples to apples basis. This has been noted in the text. —

Lines 184-187, motivating the use of dynamic time warping: "The timing of inflections does not affect the clustering, hence the effects of latitude and elevation that often result in misclassification of hydrographs because of timing differences are avoided, which is important in a spatial domain of the size being considered here." That's fine

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as far as it goes, but the apparent down sides of such an approach should also be clarified for readers. Differences in timing and magnitude are basic identifying characteristics of a watershed's hydrologic regime and represent real differences in climate and watershed hydrology.

Authors' response: The text has been expanded to and clarified. The point is that a common process, for example snowmelt, has different timing with latitude and elevation, and therefore classifications need to not confuse generating processes because of timing differences. —

Lines 195-197, speaking of statistical trend analysis: "Since these were comparing periods separated by 360 days, autocorrelation was not expected and therefore prewhitening was not applied." Careful here – there's a significant body of literature on the question of whether pre-whitening is or isn't needed in statistical analysis of streamflow for climate trends. It seems like the verdict is still out, but some would view the

Authors' assertion are being a basic technical error, at least without further analysis to quantify autocorrelation across years. Again, it looks like some more detailed analysis and more careful wordsmithing are needed.

Authors' response: There is indeed a large body of literature on prewhitening. It is very clear that when data are closely spaced in time such as daily and hourly observations, or when the time series is assembled from large numbers of samples [annual mean streamflow] the data may be expected to be autocorrelated. It is much less clear of how to apply it in datasets where autocorrelation is low and many cases not significant. We have examined this for many cases over the past decades; in most rivers only about 15% of the cases show autocorrelation when more than 100 years of data are available. With 60 years of data less than 10% of the cases have significant autocorrelation. Just for clarity, we took rivers with long records and sampled them every five days and calculated the autocorrelation for each of the 73 'slices' and compared those to the

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level of significance determining the proportion that exceed the significance threshold ( $\sim 0.19$ ). The autocorrelations that do exceed the thresholds are small ( $\sim 0.2$  to  $0.25$ ). So, in our opinion it is better to treat all the datasets in the same fashion and not introduce additional complications either by prewhitening all the cases because a few cases have significant autocorrelation or prewhitening some and not the majority. A few lines have been added to the manuscript to reflect this. —

The explanation of dynamic time warping is inadequate (lines 122-133). This is a really interesting idea that has not been seen much if any application in hydrology and is presented in the paper as one of its main elements. Much more detailed description of it would be useful for readers. Right now, there's just one sentence of explanation and a reference to the R package used to run it. It's also not clear how the cluster analysis the authors perform using dynamic time warping relates to the cluster analysis they perform using conventional k-means analysis. Are both used in the same way, as checks on each other? Or for slightly different purposes?

Authors' response: We have added some additional text and an additional component to the Figure to better explain the method. —

More broadly, the overall descriptions of the methods and their rationales feel a little murky, meandering, and internally inconsistent. The numerous typographical mistakes, grammatical errors, and poorly written sentences don't help, making the manuscript unnecessarily difficult to read and understand at points. It also feels like the paper is longer than it needs to be for its content, or maybe it is merely a matter of organization. Reorganizing the material more clearly into finer subsections with clearly worded and specific headings might help. The starting point would be to have separate "data" and "methods" sections. I suggest the authors rewrite the methodological explanation and justification from scratch and maybe consider a ground-up rewrite of the entire article focusing on making a clear case for the chief outcomes they want to communicate.

Authors' response: We revised the methods to provide the reader with more information

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about the data and analysis. —

Lines 578-581, “Many studies that seek to explore change perform many analyses simply to see what falls out. The approach used here is targeted to assess which aspects are changing and why, the basic element being the hydrological response that depends upon the key streamflow-generating processes in each basin.” Perhaps it was not the Authors’ intention, but this passage seems to come across like a somewhat ungracious and perhaps inaccurate description of earlier work in the hydrology and climate communities. There are many change studies that have performed extremely well-designed and very tightly focused analyses. The authors could also be setting the bar rather high for themselves by framing their study relative to previous work in such elevated terms; it may be in their own best interest to reconsider that choice.

Authors’ response: While it might be ungracious, this is a classic problem in the literature despite there being many well designed studies. We have rewritten the text to make the point in a more gracious manner. —

There are a couple of assertions in the paper, about a claimed resilience of mountain rivers to climate change, that seem likely to be controversial and may undermine its credibility as a whole with readers. Lines 812-817 say, “Using a modelling approach, (Bennett et al 2012; Schnorbus et al 2014) demonstrated that detecting climate driven changes in basins in the British Columbia Rocky Mountains were difficult because of interannual variability. Despite the ongoing deglaciation in the mountains of the west (Clarke et al 2015) basins in the Canadian Rockies can be resilient to change (Harder et al 2015, Whitfield and Pomeroy 2016).” Then again around line 923 or so we have, “Mountain basins appear to be resilient to change.” These statements seem inconsistent with a lot of work in the region. A few examples are Jost et al, HESS, 2012; St. Jacques et al, Canadian Water Resour. J., 2014; St. Jacques et al, Geophys. Res. Letters, 2010; Fleming and Weber, J. Hydrol., 2013; Fleming and Dahlke, Canadian Water Resour. J., 2014; Najafi et al, Geophys. Res. Letters, 2017; and Clarke et al, Nature Geoscience, 2015. Additionally, I don’t think Bennett et al

2012 and Schnorbus et al 2014 said quite what the authors of this submission seem to be ascribing to them, because both those papers clearly identified expected changes in hydrology from climate change.

Authors' response: We modified wordings to specifically relate this to our results. While we disagree with the reviewer's comment about our interpretation of the papers by Bennett et al and Schnorbus et al. we did change 'interannual variability' to "uncertainty". Bennett et al (2012) state that based on their model projections "For the 2050s time period, runoff anomalies examined for winter had the largest range in GCMs over other seasons, emissions scenarios, and the hydrologic parameterizations, in that order, with an average range for all basins of 84%, 58%, and 31%, respectively." Schnorbus et al 2014 state in their conclusions that "There is a high degree of consensus for a decrease in summer precipitation in southern BC (CR and UCR). Summer precipitation results for north-eastern BC (the PR) are more ambiguous." Versus. "Runoff changes in the three areas vary spatially and seasonally and generally reflect snow and precipitation changes and topography based temperature gradients." And "The degree to which the MD site transitions from a glacial–nival regime to strictly nival regime by the mid-21st century is potentially underestimated in the current study." Additional text has been provided to specifically indicate that the changes we observe in the mountains are different from other relevant published work. —

Plus, these assertions seem contrary to the general understanding in the hydrology and climate change communities that mountain regions are particularly susceptible to climate change, though of course such a general truism isn't necessarily applicable everywhere. At least part of the problem may be what seems to be a logical error in interpretation. On lines 923-927 we have, "These basins demonstrate several hydrograph types but generally lack structure in trend patterns. Individually, these basin do show periods with increases or decreases in streamflow consistent with freshet timing changes, as has been reported elsewhere, but there is sufficient inconsistency among the basins to define a specific pattern." I guess the idea here is that if there isn't clear

spatial consistency in trends, then there are no real trends, and the mountain region considered here is (therefore, in this view) resilient to change. But watershed properties influence hydrologic trends resulting from climate change, producing major trend variability between basins. Glaciers in alpine watersheds are a really well known example, and a relevant one in light of the fact that huge ice fields are at the headwaters of many rivers draining eastward off the Canadian Rocky Mountains and that are presumably considered in this article. For background and examples see Jansson et al, J. Hydrol., 2003; Fleming and Clarke, Can. Water Resour. J., 2003; Dahlke et al, HESS, 2012; Jost et al, HESS, 2012; Baraer et al, J. Glaciology, 2012; Fleming and Dahlke, Can. Water Resour. J., 2014; Moore et al, Hydrol. Proc., 2009; Stahl and Moore, Water Resour. Res., 2006; Stahl et al, Water Resour. Res., 2008; Fleming et al, Advances Water Resour., 2016; Casassa et al, Hydrol. Proc., 2009; Li et al, Hydrol. Proc., 2010; O'Neel et al, Climatic Change, 2014; O'Neel et al, Bioscience, 2015. Maybe the mountains of western Canada are, as the authors of this study suggest, more resilient to change than had previously been thought, but if so, it feels like a much more convincing case for it has to be made with complete and accurate referencing of the relevant literature.

Authors' response: In our opinion, there is no logical error. Warming in mountainous areas has the potential to result in large hydrologic change and that has been widely reported as the reviewer indicates. We make the case that when comparing stations that are closely located in space, with either similar or dissimilar streamflow generating mechanisms, one should expect that the seasonal pattern of changes would be similar. This is a different perspective to many of the papers the reviewer lists. There is no question that glaciers are melting, but this does not always result in changes in seasonal patterns of streamflow. The fact that there is inconsistency is what is important; however, that does not mean that there are no trends. We added some text to emphasize these differences. —

The discussion of interannual/interdecadal climate variability like ENSO and the PDO

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on lines 829-837 is inadequate. The reference to these phenomena as “climate signals” is vague (climate change and other climate processes produce climate signals too), the passage is under-referenced, and the view it gives of these effects is not sufficiently incomplete.

Authors’ response: We disagree. We acknowledge in the discussion that these signals were not considered in this study but that some authors had reported effects of PDO and AO.

It feels like the referencing around climatic changes that might be causing the hydrologic and landscape change inferred in this study might be improved a little. The most notable omissions include Vincent et al., J. Climate, 2015; and Vincent et al., Atmos.-Ocean, 2018.

Authors’ response: Vincent et al (2015) studied the trends in temperature and precipitation associated with low frequency modes which we acknowledged we did not investigate. We have included text to reflect the changes in snowfall days reported in Vincent et al 2018. —

“Section 4: Code Availability” is empty, and insufficient detail is provided for data sources in “Section 5: Data Availability.”

Authors’ response: These headers were required by HESS even though they are not used. The access to the data through ECDataExplorer is described in the main text. This should be sufficient for others to access the data in the same manner. —

While using sequent 5-day periods of the year for analysis makes sense, plotting them up in this way does not. Nobody intuitively thinks of seasons this way – what time of year is, say, the 23rd sequent 5-day interval? It makes interpretation of many of the figures and discussions in the manuscript unnecessarily opaque (Figure 8 is one example).

Authors’ response: We think that the reviewer is simply asking for information about

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the months of the year to compliment the series of 5-day periods. We have added secondary axes to all the plots in the main paper that show the months of the year, and a table to show the starting dates of each 5-day period, but the series of 5 day periods upon which the analysis is based are retained and used as the point of reference. —

A clearer case for using high-frequency data in long term change analysis could also be helpful. A starting point would be to provide more references to studies using variations of this approach like Hatcher and Jones, *Atmos.-Ocean*, 2013, Fleming et al, *Advances Water Resour.*, 2016, and Vincent et al, *Atmos.-Ocean*, 2018.

Authors' response: We do not understand what the reviewer intends by "A clearer case for using high-frequency data in long term change analysis could also be helpful." References to Hatcher and Jones (2013) regarding resilience in Columbia headwaters and Vincent et al (2018) regarding changes in days with snow were added into the text. Fleming et al (2016) deals with teleconnections which, as noted above, we acknowledge we did not address. —

The figures need work. Obvious examples are the confusing use of numbered sequent 5-day periods instead of day or month of year in many of the figures (see above comment), the lack of any axis labels on Figures 8 and 11, and the lack of even the most basic geographic details on Figure 6 for readers unfamiliar with Canada.

Authors' response: A secondary axis indicating the months of the year was added to figures in the revised main and supplementary texts. A summary of all the changes to figures is provided at the end of this document. —

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