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Discussions

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

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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 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 north-

ern 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.

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. 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.

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

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.

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

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 seems 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.

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 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.

Lines 195-197, speaking of statistical trend analysis: "Since these were comparing periods separated by 360 days, autocorrelation was not expected and therefore pre-whitening 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.

The explanation of dynamic time warping is inadequate (lines 122-133). This is a really interesting idea that has not 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?

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.

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.

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. 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.

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

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.

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

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). 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.

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.

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