

# ***Interactive comment on “Variability of low flow magnitudes in the Upper Colorado River Basin: identifying trends and relative role of large-scale climate dynamics” by M. Pournasiri Poshtiri and I. Pal***

## **Anonymous Referee #2**

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Comments on “Variability of low flow magnitudes in the Upper Colorado River Basin: identifying trends and relative role of large-scale climate dynamics” by Poshtiri and Pal.

The authors are not clear on the objectives of their study. In the abstract they state their research question as follows: “Is there systematic variability in water availability during the driest time of a year or season? How does that vary across locations and is there a link between large-scale climate and low flow variations?” Then on page 8782 they say their “science questions” which are: “How heterogeneous are the variability of low

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flow conditions in the headwater basin of Colorado River? How are season specific low flows linked with synoptic ocean-atmospheric conditions? Where is that signal evidently strong and where is it weak? How different those linkages are for diverse locations in the headwater basin?” My first concern is that these two statements are rather different. This is confusing. But, more importantly, having stated these questions it is unclear why the paper proceeds to conduct trend analysis and wavelet analysis of the data sets. Trends and apparent periodicities at individual streamgages do not appear to be a part of their question and yet these two topics occupy a large portion of what is presented in the paper.

The trend analysis they conducted appears to cover different periods of record at different sites. They state that the records range in length from 25 to 61 years. Is it possible that the different trend results are a consequence of the different periods of record used? Perhaps they used the same period of record at all sites but they do not seem to indicate what that period is. It is well known that the results of trend tests of hydrologic variables are very much influenced by the period of record that is used, due to long term, regionally extensive, quasi-periodic climate oscillations. Trend tests should only be conducted on records that cover approximately the same periods. The records analyzed here appear to range from 25 years to as much as 61 years.

The wavelet analysis shows periodicities that seem to come and go over time. If that is the case, are these really periodicities at all. Again, data sets with long-term persistence (which is common in hydrologic records) will tend to show behaviors that might be suggestive of either periodic fluctuation or trend, but with the passage of time those behaviors go away or change. How does wavelet analysis contribute to answering the stated “research questions” which have to do with relationships to larger climate system variations.

Figure 5 shows 51 correlation coefficient maps. It is very complex and confusing and is also so small as to be difficult to read. For each station there seems to be a different time period selected for the variable to which the Q7 values are correlated. Thus, it is

impossible to look at the graphics together to see how similar or different the correlation patterns are. For example, the top two graphs on the left edge of the figure show the following: The first is the Black Station Q7 correlation with US ONDJFM precipitation but the next one is the Bobtail station Q7 correlation with US DJFMAM precipitation. These kinds of differences in graphs make them of very limited utility for seeing if there is heterogeneity in the variability of low flows. What exactly is the point of figure 5? They state: “we picked the best monthly combinations for climate conditions, as indicated in Fig. 5.” What was the metric of “best” used to pick these? If the goal is to understand the heterogeneous nature of flows across the basin, why not consider a single climate indicator across all stations?

Overall, the connection between the paper’s stated goals, and the particular techniques used is not at all clear. It is also problematic that it doesn’t appear to make any headway in advancing the science with respect to long-term trends in low flows in this basin or to the ability to forecast at a seasonal time scale the low flows for a given year.

There are many problems with the writing in this paper and with some details of methods and graphics. Here are a few of them.

The sentence on lines 11-13 of page 8781 is very complex and obscure. What is meant by “integral impacts of persistent low flow conditions in a river costing more than ever thought in history.”

On line 21 of page 8781 there is reference to water use of the Colorado river which is characterized by: “especially for high municipal and industrial demands”. The largest use of water in this basin is certainly for irrigation, which isn’t even mentioned. What is meant by “high. . .demands”? Is this high per capita or high in aggregate? Subsequent lines continue to ignore irrigation among the water use categories.

The material on pages 8781 lines 25-29 related to climate warming neglects to even mention the change in precipitation mix (decreasing ratio of snow to total precipitation) or the influence of earlier snowmelt in summer-time low flows, a topic about which there

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are many papers in the literature. Similarly around line 10 of page 8782 in a long list of factors there is no reference to snow-pack dynamics even though there is mention of melting glaciers (the former is much more significant over the region than is the latter).

Regarding normality of the data. The sample skewness and kurtosis are very poor indicators of normality (there is a great deal of literature on this topic). Much better methods include the probability plot correlation coefficient.

Figure 3 and associated text, there is no definition given for what units are used to quantify the trends. The legend and caption for figure 3 simply say “magnitude” but never state the units.

Line 12 of page 8787 the wavelet analysis results in figure 4 are referred to as “test results”. Is there a statistical hypothesis being conducted here? If so, what is it?

Line 15 page 8788: “precipitation played a greater role in the drought increase.” What is meant by this statement? Greater than what?

Figure 2, this is an overly complex graphic. Perhaps boxplots by year would better convey the information. The dotted lines between the points convey no information they just clutter the graphic. There is something odd here as well. There seem to be a group of red x’s that line up on a perfectly horizontal line at a standardized value of about -0.5. This suggests a period of no variability at all at one of the stations. What is happening here?

Figure 4, there is no explanation of the cross-hatching on figure 4.

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