Hydrol. Earth Syst. Sci. Discuss., 11, C5020–C5026, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C5020/2014/

Hydrology and Earth System Sciences
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Discussions

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

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Received and published: 11 November 2014

Thank you for your review. We were happy to address all the concerns. Our response to each comment is written below and we incorporated them in the manuscript as track changes, please check the supplement.

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 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.

[Reply] We have re-written the abstract to reflect the major science questions addressed within the paper. Regarding “trends and periodicity”, we analyzed trends to determine “heterogeneity” in patterns in natural low flow across the basin i.e. how variable the trend patterns are if location-specific data are selected; also “periodicity” indicates “sub-decadal to multi-decadal” variability. However, to make these points clearer, we now have written and addressed our questions to better reflect the analyses done in the manuscript.

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.

[Reply] We previously conducted the trend analyses for variable length of data to ensure statistical power for the stations having longer records. We found an east/west difference but regional clustering of trends was perfect (i.e. nearby east stations were yielding similar trending patterns). However, the question of the reviewer indicating "Is it possible that the different trend results are a consequence of the different periods of record used?" - is a valid point. Therefore, we re-run the analyses only for those stations, which have more than 30-years data availability (for ensuring the statistical power), and of equal length (1976-2011), and show the results in Figure S3. Figure 3 (trend results using variable data length) and Figure S3 (equal length data from 1976-2011) indicate similar spatial variability in trends i.e. a clear east and west difference in trends in low flow magnitude as well as similar patterns and significance.

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.

[Reply] Good question. "Periodicities that seem to come and go over time" is feasible in wavelet plots (see for example: Wang and Wang, 1996; Torrence and Compo, 1998; Torrence and Webster, 1998), possibly due to the sparseness of the data (Folland et al., 1994). This is why we conducted an additional analysis to re-test whether the periodicity we found from wavelet analysis from the individual station data does in fact show relationships with the "known" climatic pattern of similar periodicity; and not only so, whether that finding is consistent with the previous discoveries on droughts. We conducted a Principal Component Analysis on the low flow indices data, and the first principal component (PC1) gave us variance explained of nearly 40-60% for annual/seasonal indices (Figure S4). PC1 also yielded significant correlation patterns with the northern Pacific (and tropical Pacific) surface temperature, which is popularly known as Interdecadal Pacific Oscillation (IPO by Dai et al. 2013) and related to Pacific Decadal Oscillation (PDO). McCabe et al (2004) indicated that southwestern droughts are associated with different phases of PDO. Our results conclude that low flow magnitudes at individual locations have a periodicity and that is also evident in first principal component correlation maps.

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?

[Reply] To address this concern we removed Figure 5. Instead we added Principal Component Analysis to detect common variance in the data across UCRB and then run the correlation analysis between PC1 and large-scale climate. Please check new Figure 5 and S5.

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.

[Reply] Please find answers to the comments above and below.

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."

[Reply] This sentence is removed from the manuscript.

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.

[Reply] We incorporated "agricultural" in the manuscript. Please check pages 3 and 4, lines 65-66, 68.

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 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).

[Reply] This is a good point. We now incorporated "snow" and melting snow induced runoff shifts. Please check page 3, lines 73-75 and line 84.

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.

[Reply] This is why we cross-checked correlation patterns using non-parametric Spearman method. The results yielded the same patterns. 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.

[Reply] The unit is in cms/day/year. We have now included this in the figure caption.

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?

[Reply] Torrence and Compo (1998) first determined significance levels for wavelet spectra. For that one first needs to choose appropriate background spectra, which in this case was red noise (increasing power with decreasing frequency). According to Torrence and Compo (1998), the null hypothesis is defined for the wavelet power spectrum. A mean power spectrum for the time series is possibly given by a formula by Gilman et al. (1963): where α is the assumed to be lag-1 autocorrelation, N is the number of points in the time series and k = 0 ... N/2 is the frequency index. If in the wavelet power spectrum a peak is significantly above this background spectrum, it can be assumed to be a true feature with the 95% confidence level (significant at 5%).

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

[Reply] This meant that precipitation played a greater role than regional temperature.

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?

[Reply] This was a great suggestion; please check a revised Figure 2 that follows this suggestion.

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

[Reply] That is the “cone of influence”. We have added that now in the figure caption.

References

Dai, A.: The influence of the Inter-decadal Pacific Oscillation on U.S. precipitation during 1923-2010, Climate Dynamics, 41, 633-646, doi: 10.1007/s00382-012-1446-5, 2013. Folland, C. K., Parker, D. E. and Kates, F. E.: Worldwide marine temperature fluctuations 1856–1981, Nature, 310, 670–673, 1984. Gilman, D. L., Fuglister, F. J. and Mitchell Jr., J. M.: On the power spectrum of “red noise,” J. Atmos. Sci., 20, 182–184, 1963. McCabe, G.J., Palecki, M.A., and Betancourt, J.L.: Pacific and Atlantic Ocean influences on multidecadal drought frequency in the United States, Proceedings of the National Academy of Sciences, 101, 4136-4141, 2004. Torrence, C., and Compo, G. P.: A Practical Guide to Wavelet Analysis. Bulletin of the American Meteorological Society. 79 (1), pp. 61–78. 1998. Torrence, C., and Webster, P. J.: The annual cycle of persistence in the El Niño–Southern Oscillation, Quart. J. Roy. Meteor. Soc., 124(550), 1985-2004, 1998. Wang, B., and Wang, Y.: Temporal structure of the Southern Oscillation as revealed by waveform and wavelet analysis, J. Climate, 9, 1586–1598, 1996.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C5020/2014/hessd-11-C5020-2014-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 8779, 2014.