

***Interactive comment on* “Nonstationarity of low flows and their timing in the eastern United States” by S. Sadri et al.**

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Received and published: 20 July 2015

We thank the referee for the very useful comments and suggestions. We have repeated the comments below and our responses are indicated by the word "Response"

Anonymous Referee #2

General Comments

The manuscript presents an analysis of low flows in Eastern U.S. that is based stream-flow annual minima (with different smoothing windows). In order to identify non-stationarity, the authors propose an algorithm (a cascade of 3 statistical tests) for which gauges are sorted into different classes and, depending on the outcome of the autocor-

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relation test, series are tested for trends or split into sub-series to be tested for trends. On the basis of this classification trend results are provided with a discussion on the possible causes identified in the study.

The writing is clear and results are generally well described and presented. While I acknowledge that low flows are analyzed at remarkable spatial and temporal coverage, I have a few concerns with the stationarity analysis methods and assumptions (e.g. presence of autocorrelation invalidates the use of MannKendall but allows the use of Pettitt; change-points are assumed as human induced and are only tested for autocorrelated series). I also find the attribution part weak and think it should be titled differently. These issues are raised in the section below along with edits suggested to the text.

Specific Remarks

point-1 Page 2762 Lines 8-9: You state : “to systematically distinguish the effects of human intervention from those of climate variability”, as if this were the main goal of the paper (is it?), while it seems to me this is just a step. I suggest that framing of the overall scope of the paper should come first, after the initial introduction of Lines 1-5.

Response: Agreed. The main goal of the paper is to examine non-stationarity in low flow generation, with an initial step of identifying sites with time series that are likely not affected by direct anthropogenic influences. We have moved this part of the sentence to earlier in the abstract and edited it to better reflect the main goal: “The goal of this paper is to examine non-stationarity in low flow generation across the eastern U.S. and explore the attribution to anthropogenic influences or climate variability. We use nonparametric tests to identify abrupt and gradual changes in time series of low flows and their timing for 508 USGS streamflow gauging sites in the eastern US with more than 50 years of daily data,.”

point-2 Page 2762 Lines 12-14: Country wide hydrological databases, for as comprehensive as they can be, may not be so accurate on all gauges metadata and on all flow

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types (high/low): some gauges may be well documented, some others not so much. Of course this is valid for the streamflow data itself too, but in general, as there is no way to check the notes validity without data scrutiny and the help of the data providers, I would be cautious throughout the text referring to the USGS notes and using on them as supporting evidence. Finally, I find this phrase on the USGS notes not so relevant in the abstract. “Examination of the USGS notes for each site confirmed that many of the step changes and around half of the sites with an increasing trend were associated with regulation.”

Response: We acknowledge that the notes have uncertainties and may have shortcomings in understanding of changes in low flows, in particular because they are specific to high flows. However, we are interested in whether there is overall consistency between statistically identified changes and documentation of changes that might affect flows and their measurement.

We have changed the parts of the abstract that link the quantified changes with documented changes to be more cautious: “Examination of the USGS notes for each site showed that many of the sites with step changes and around half of the sites with an increasing trend have been documented as having some kind of regulation. Sites with decreasing or no trend are less likely to have documented influences on flows or changes in measurement characteristics.”

point-3 Page 2764 Lines 17-20: The difficulty of low flow analysis with the advent of non-stationarity could be introduced and developed earlier in the introduction, particularly for the important consequences on hydrological analysis (i.e., the limits non-stationarity poses to the application of statistical tools).

Response: Agreed. We have moved this statement to earlier in the introduction (end of second paragraph): “Regulation generally introduces non-stationarity into low flow time series that impedes the development of regional or at-site frequency analysis models. In most instances, such models show a high standard error between modeled and

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[Interactive Discussion](#)

[Discussion Paper](#)



observed quantiles (Kroll et al., 2004).”

point-3 Page 2766 Lines 28-29: “we analyze the temporal and spatial distribution [...] to systematically distinguish the effects of human intervention from those of climate variability and change”. Is this even possible over such a large area, which has been increasingly impacted by anthropogenic influence over the analyzed decades? Maybe use the verb “attempt to”.

Response: Agreed. We have changed the sentence in response to a previous comment but have highlighted that this is an attempt to identify useable time series that are potentially free of influence. “The goal of this paper is to examine non-stationarity in low flow generation across the eastern U.S. and attempt to systematically identify time series that are potentially free of the effects of human intervention and examine these in terms of the impact of climate variability and change.”

As in point-1, is this the overall scope of the paper? Let the reader know why this distinction is relevant for this work.

Response: See response to point-1

point-4 Page 2767 Lines 1-3: “Often the best way to determine ..”. I don’t agree (see point-2), I would replace with “A way to determine ..”.

Response: We changed the sentence: “A way to determine whether a river has been subject to anthropogenic influences, at least in terms of regulation, is to examine the site notes for the gauging station”

point-5 Page 2767 Lines 6-8: “we develop an alternative approach”. I find this statement somewhat misleading, it seems to suggest that the routine approach is to rely on site notes, and I don’t believe it is the case.

Response: Agreed. We have removed the word “alternative”.

“ that assumes that the impact of human activities can be detected in the streamflow

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data in a systematic way”.

While I recognize the value of this approach for its ability to process virtually any number of sufficiently long streamflow series systematically, I remain skeptical with its efficacy and universal application. Isn't that a simplification rather than an assumption?

Response: Agreed. While we suggest that the method presented shows promise, we agree that further work is needed to understand the robustness of the method and its transferability. We have updated this sentence slightly: “that makes the simplification that the impact of human activities can be detected in the streamflow data in a systematic way”

and have edited the sentence at the end of the conclusions that highlights the potential of the method to be applied elsewhere to also note the limitations/work to be done to understand its robustness and broader applicability: “The methods are readily transferable to other parts of the U.S. and globally, given long enough time series of daily streamflow data, although further work is required to understand their universal application.”

point-6 Page 2767 Lines 14-15: “We therefore assume that step changes in the time series are indicative of an anthropogenic effect ”. Not necessarily, considering that step changes could result from climate variability (e.g. located at turning points from positive to negative phases of AMO, NAO, etc). For instance, you just mentioned that McCabe and Wolock (2002) reported 1970 as a step change, and that large-scale teleconnections may play an important role in driving changes in low flows (e.g. Giuntoli et al., 2013). You could add Mauget (2003) too [Mauget, S.A., 2003. Multidecadal regime shifts in US streamflow, precipitation, and temperature at the end of the twentieth century. J. Climate 16, 3905–3916.].

Response: See also our response to referee #1 on this comment. While we agree that the changes reported in these studies identify changes in different indicators (precipitation, temperature, and streamflow) the time scales over which these happen tend to

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[Interactive Discussion](#)

[Discussion Paper](#)



be of the order of a few years, rather than an abrupt change that occurs from one year to another. In response to other referee comments, we have updated the text in various places to make it clear that the identified shifts are abrupt and visually obvious.

We have added the Mauget reference.

point-7 Page 2768 Line 17: “the” before “wettest”

Response: We corrected it.

point-8 Page 2769 Lines 6-7: Probably worth mentioning that Florida’s aquifer may have some inertia on the streamflow regimes and therefore low flows analysis is harder to achieve as a typically slower water response can result in drought events that are not always confined to the same water-year.

Response: Agreed. This is partially accounted for by calculating the autocorrelation, which will identify this type of behavior. We have added a note on this to this section: “In contrast, the southeast, including Florida, lies on active aquifers (USGS, 2009), where low flow anomalies, such as in a drought year, may persist for multiple years.”

point-9 Page 2769 Line 18: Not sure Fig. 1B is much relevant, and it is so crowded with overlapping dots that it’s difficult to distinguish colors. I would just go with the selection of 508 sites used in the analysis (Fig. 1C).

Response: Agreed. We have deleted Figure 1B and updated the related text.

point-11 Page 2770 Line 6: Can gauges belong to more than one category in Fig. 1D, so be affected by urbanization and have undergone a change in gauge datum?

Response: Yes, but only for a few sites. In this case we chose the flag with the highest likelihood of having affected the flows. For example, a “regulated” flag is assumed to have a larger effect than a change in gauge datum. We have noted this in section 2.2: “A few sites have more than one type of flag and we show the flag associated with a higher likelihood of the flows being affected (e.g. regulated).”

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point-12 Page 2770 Lines 11-12: Have you compared your results with the HCDN data set you mentioned above (with the gauges you identified as free from human intervention)?

Response: See response to referee #1.

point-13 Page 2770 Line 13: The title of this section introduces 2 sub-sections about statistical methods, but this section is actually about low flow indices alone. I suggest to rename this section “2.3 Low flow indices”, and maybe go with what is now 2.3.1 and 2.3.2 as 2.4 and 2.5 respectively.

Response: Agreed. We have updated the section numbering to 2.3, 2.4, 2.5, and changed the title of section 2.3 as suggested.

point-14 Page 2770 Lines 22: You state that Q90 is useful for reservoir operations. Considering that indices are extracted yearly, 90 days is a very large smoothing window. Is Q90 really relevant to this study?

Response: We included Q90 because it is relevant to reservoir operations, and shows some interesting differences from the short-term Qn values. However, as the results for Q90 are similar to Q30 we have decided to remove the results for Q90 and only mention that they are similar. The results for Q1 are also now omitted because they are similar to those for Q7.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 2761, 2015.

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