

Part 1. Omitted Points 15-25

From the previous review, there is no trace of the authors' responses to the points 15 to 25. I urge the authors to reply/address them (pasted below).

\noindent{\bf point-15}

Page 2770 Lines 22-24:\\

You introduce the index ``day of the year of low flows'', but there is no indication on how it is obtained. You provide a description at the beginning of section 4.2 – that you could improve (e.g. clarifying how you identify the 4-month periods) and move to this section.

\noindent{\bf point-16}

Page 2771 Lines 13-14:\\

Visual inspection simply provides an indication. I suggest to either delete this phrase or replace ``can be very helpful in determining'' with something like ``can provide indication in the attempt to assess stationarity''.

\noindent{\bf point-16}

Page 2771 Lines 21-24:\\

You should clarify the following: 1) Provided that autocorrelation is an issue for both MK and Pettitt tests, if autocorrelation is present the Pettitt test is applied, but the same is not valid for the MK test, why? Also: for MK there are adaptations of the test proposed by Hamed and Rao (1998) and Yue and Wang (2002,2004) to account for autocorrelation, did you consider this option?

\noindent{\bf point-17}

Page 2771 Line 27:\\

``We assume that the change year corresponds to human intervention'' I find this assumption questionable. As written in point-6, a change point could result from climate variability.

\noindent{\bf point-18}

Page 2772 Lines 3:\\

In light of the previous observations I find this algorithm should be reconsidered. Also, a visual (flow chart) of the algorithm would be useful to guide the reader through the different steps.

\noindent{\bf point-19}

Page 2774/L20-2775/L10:\\

''From table 1 we observe that:``. See point-2.

\noindent{\bf point-20}

Page 2775 Line 11:\\

See point-6 on causes of abrupt changes neglected in this study.

\noindent{\bf point-21}

Page 2776 Line 21:\\

Figure 7b: there must be something wrong with the counting – 55 decreasing trends seems like too much compared to Figure 7a (same number?). Also dots overlap a lot, might be a good idea to reduce the size.

\noindent{\bf point-22}

Page 2778 Lines 6-7:\\

''applied within the 4 month season of Q1 and Q7 low flows``. It is not clear to which series the MK and Pettitt tests have been applied.

\noindent{\bf point-23}

Page 2778 Lines 11-:\\

''Out of the remaining 335 sites``, should numbers add up in e.g. Fig. 9A (17+13+1)?

\noindent{\bf point-24}

Page 2779 Line 4\\

As you write in the Conclusions: ''However, definitive attribution will require detailed analysis of these competing factors and possibly carefully crafted

modeling studies.`` I would not call section 5.1 Attribution., maybe Towards the attribution of trends in low flows, or similar. There should also be mention, either in this section or in the introduction, of the distinction between trend detection and attribution and on the difficulties of performing the latter (e.g. Merz et al. (2012)) [Merz, B., Vorogushyn, S., Uhlemann, S., Delgado, J., Hundecha, Y., 2012. Hess opinions ''more efforts and scientific rigour are needed to attribute trends in flood time series. Hydrol. Earth Syst. Sci. Discuss. 9, 1345–1365, HESSD.]

>> [Added Oct 25 – complement to point-24] >> there should be mention of novel approaches to attribute changes to external drivers in line with the ideas outlined in Merz et al (2012). For example Harrigan et al. (2014) and Prosdocimi et al. (2015), in which specific effort is made to identify whether some anthropogenic variable can be related to some evolving behaviour in hydrological series, keeping the natural climatic variability in control either via the Multiple working hypothesis approach or via a complex statistical model. [Harrigan, S., Murphy, C., Hall, J., Wilby, R. L., and Sweeney, J.: Attribution of detected changes in streamflow using multiple working hypotheses, Hydrol. Earth Syst. Sci., 18, 1935–1952, doi:10.5194/hess-18-1935-2014, 2014; Prosdocimi, I., T. R. Kjeldsen, and J. D. Miller (2015), Detection and attribution of urbanization effect on flood extremes using nonstationary flood-frequency models, Water Resour. Res., 51, 4244–4262, doi:[10.1002/2015WR017065](https://doi.org/10.1002/2015WR017065)]. >>

\noindent{\bf point-25}

Page 2779 Lines 20–28:\\

No reference to antecedent precipitation is found in the results, I think this block belongs to the results section, to be later discussed in this section.

Part 2. Updated manuscript.

In general, I find it inappropriate to talk about attribution under this study's framework: the authors have changed the title of Section 5.1 from "Attribution" to "Potential Drivers", they should also change terminology in the remainder of the paper accordingly: i.e. L. 19, L. 180, L. 433, L. 498, etc.

With regards to the pre-whitening, the authors have cited Kumar (2009) (please add to the references list), but have not specified which method they used of the four proposed by the reference.

Moreover, the hypothesis that step changes are human induced and that slow changes are related to e.g. long range dependence is questionable or, at the very least, a huge approximation. If this stays a disclaimer should be put in the discussions.

Regarding the updated manuscript:

Line 151–154: ``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.'' I think there are too many claims in this paragraph, I also suggest ``attempt to systematically identify [..]'' comes first.

Line 169–170: ``our assumption is based on identifying abrupt and visually obvious step changes''. I don't consider this an assumption that can hold, but a simplification with two inherent shortcomings: how arbitrary is the judgment of an abrupt change? More importantly, natural variability can produce abrupt changes too. This issue was raised in point 17 too.

Line 231–233: Both referee 1 and myself had suggested to check your results on HCDN stations. The authors added that 64 of the sites are in the HCDN-2009 database. I strongly suggest the authors to go beyond listing the number of HCDN stations and actually report on how their method performs on those stations.