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Discussions

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

Anonymous Referee #2

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General comments

The manuscript presents an analysis of low flows in Eastern U.S. that is based streamflow 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 autocorrelation 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 assump-

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tions (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.

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

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).

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

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 ..”.

point-5 Page 2767 Lines 6-8:
“we develop an alternative approach ”. I find this statement somewhat misleading, it seems to suggests that the routine approach is to rely on site notes, and I don’t believe it is the case.
“ that assumes that the impact of human activities can be detected in the streamflow 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?

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.].

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

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.

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).

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?

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)?

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.

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?

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.

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".

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?

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.

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.

point-19 Page 2774/L20-2775/L10:
"From table 1 we observe that:". See point-2.

point-20 Page 2775 Line 11:
See point-6 on causes of abrupt changes neglected in this study.

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.

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.

point-23 Page 2778 Lines 11-:
"Out of the remaining 335 sites", should numbers add up in e.g. Fig. 9A (17+13+1)?

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.]

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.

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