Hydrol. Earth Syst. Sci. Discuss., 12, C1082–C1086, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C1082/2015/

Hydrology and Earth System Sciences
Open Access
Discussions

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

Anonymous Referee #1

Received and published: 16 April 2015

Anonymous Referee Review of "Nonstationarity of low flows and their timing in the eastern United States" by S. Sadri, J. Kam, and J. Sheffield

This article is well written and provides valuable insight into changes in low flow magnitude and timing in the eastern U.S. It is a more thorough look at historical low flow changes than I have seen previously. The introduction is very well done and referenced. I have two major concerns about the article that I describe below in detail; one concerns the classification of streamflow gauges and one concerns the decomposition algorithm that was used for the analyses. I have also listed minor concerns below.

Classification of flows: I consulted colleagues at USGS and there are no site notes available for low flows. The codes used for this article are presumably from USGS

peak streamflow metadata which are available online. The codes are relevant to the annual instantaneous peak flows and may or may not be applicable to low flows. For example, low flows are likely more sensitive to streamflow regulation than peak flows. Also, some of the codes used by the authors are not meaningful for determining anthropogenic influence. The "base discharge" is a level set to allow 3-4 peaks per year on average to exceed this level. Instantaneous peaks above this level are then recorded. A "change of base discharge" does not indicate any change in actual flows recorded or any anthropogenic change in watersheds above a gauge. The "change of gauge datum" also does not indicate any change in flows or anthropogenic influence. It indicates only that the arbitrary zero gauge height for the rating curve has been changed, normally because of changes in the gauge control point on a river (the riffle or channel section that controls the relation between river height and flow at a gauge). So for example in the abstract, the statement that "about a third of the sites with a decreasing trend were associated with a change of gauge datum" is not a meaningful statement. Statements throughout the article will need to be evaluated and many changed or removed. With my stated concerns with the classification of flows and the decomposition algorithm (see below), it's currently not clear to me whether the classification system provides meaningful insight into the study results.

Decomposition algorithm: It's not clear to me why this recursive algorithm is used in the way it is for the Ljung-Box, Mann-Kendall, and Pettitt tests. This algorithm needs to be justified and fully referenced. There is currently one reference to the impact of autocorrelation on the Mann-Kendall test. Concerns/questions that I have with the decomposition algorithm:

(1) Why not correct the Mann-Kendall test for autocorrelation rather than not testing sites with autocorrelation. This has been an area of much work. There is the issue of removing trends when removing autocorrelation. Certainly a discussion and justification with references is warranted. (2) Why not test all gauges with the Pettitt test? A significant Mann-Kendall test could actually be due to a step change. This can easily

be demonstrated by generating say 30 random normal values about a mean and another 30 about a different mean. There is no trend in each set, but the Mann-Kendall test will indicate a significant trend (if the means are far enough apart) if these two sets are treated as one time series. Why not specify the direction of the step change in the article as was done for Mann-Kendall? This would add important information. (3) Why use only test sites with significant autocorrelation for the Pettitt test? In addition to the issue in (2), the Pettitt test is sensitive to autocorrelation just like Mann-Kendall (Serinaldi and Kilsby, 2015, The importance of prewhitening in change point analysis under persistence, Stoch Environ Res Risk Assess; Ferguson and Villarini, 2012, Detecting inhomogeneities in the twentieth century reanalysis over the Central United States, J Geophys Research Atmos). (4) More information is needed on how the Ljung-Box test was applied. It's not clear to me how many and which lags were tested. Also, does this test address long term persistence, which is an important issue in time series testing (see for example Cohn and Lins, 2005 and several articles by Koutsoyiannis). (5) The results in Figure 7 don't follow the stated algorithm. All sites are tested for trend in Panel A, regardless of autocorrelation.

Other Comments

(1) "Eastern United States" should be defined when the term is first used in the introduction. (2) Page 2763, line 18, Low flows in the eastern US are controlled by more than just subsurface flows. Precipitation in low flow seasons is important to low flows in the eastern US because of the regular precipitation during low flow seasons. It's not clear whether a true base flow from only groundwater input actually happens in the eastern US. If you believe it does, please provide references. Otherwise, please mention the importance of rainfall to maintaining low flows. (3) Page 2765, line 1, Increased urbanization can also lead to increases in low flows due to water supply and wastewater pipe leakage, direct wastewater discharge, reduced evapotransporation, and importation of water from outside of watersheds that decreases groundwater pumping. See for example Brandes et al. (2005, Base flow trends in urbanizing water-

sheds of the Delaware River basin, JAWRA). (4) Page 2766, line 25, It is misleading to say that none of these studies considered the role of anthropogenic influences. What at least some of the studies did was to remove, to the extent possible, anthropogenic influences by use of minimally altered watersheds. This was done to try to isolate climatic signals rather than direct anthropogenic watershed change signals. (5) Page 2767, line 1, The extensive efforts of the USGS to identify gauges with minimal human influence should be noted in this paragraph. The old HCDN from 1992 and the new HCDN (Lins, 2012) classify minimally influenced gauges. The latest set is the HCDN-2009 (Lins, 2012) which looked at many quantitative factors in watersheds, including urbanization and regulation, and consulted with local USGS offices. It did not target just high flows in it's classification. I think the current study should at least check their classification scheme vs. the HCDN-2009 to see how well it works for minimally disturbed watersheds. It's not reasonable to assume that the author's classification system is better, at least for minimally disturbed watersheds. (6) Page 2767, line 14, I think it's a bad assumption that step changes are indicative of an anthropogenic effect. This certainly happens, but step changes can occur for natural reasons, such as a change in a large scale ocean/atmosphere system (PDO, AMO, etc.). A step change in annual minimum flows at many minimally disturbed watersheds in the eastern United States was found by McCabe and Wolock (2002) which may be explained by North Atlantic sea surface temperatures (McCabe and Wolock, 2008). (7) Page 2768, line 17, Surely some parts of the Pacific Northwest and Alaska are wetter than the eastern United States. (8) Page 2768, line 21, What lakes would cause lake-effect snow in NY City and Philadelphia if not shielded by the Appalachian Mountains? (9) Page 2769, line 4, The water supply in the Northeast comes as least partly from groundwater from limited aquifers and bedrock. As far as I know, water suppliers have shifted from surface water to groundwater sources in recent years because of increased costs from required filtering of surface water. I think this is because of EPA regulations. (10) Page 2769, line 16, "Several tens of stations" is a bit vague and misleading. Based on the HCDN-2009 map in Lins (2012) it looks like there are about 200 or so stations in your study area, though not all of these will have 50 years of data. Also, in the reference on line 15, this should be HCDN-2009, not HCDN. HCDN was the earlier network from 1992. (11) Page 2770, line 2, Why is the median record length 18 years longer than the mean record length? This doesn't seem right. (12) Page 2771, equations 1a-1c, The variables in the equations should be defined. (13) Page 2772, lines 10 and 19, "no trend" should be "no significant trend" as the lack of a significant trend does not prove that there is no trend. Also, the level of significance for the trend tests should be stated. (14) Page 2772, line 16, I disagree with the assumption that the lack of a significant step change combined with significant autocorrelation is necessarily a reflection of management effects. What about multi-year climatic droughts, for example in New England in the 1960s? What about groundwater storage? What is the basis for this assumption of management effects? What management effects would cause autocorrelation of low flows? Are there many reservoirs in the eastern US with the capability for multi-year storage? (15) Page 2776, line 23, I don't see any gray colored sites in Figure 7, are the correct panels in this figure? (16) Section 4.3, Are the tests for changes in low flow timing following the algorithm specified in section 2.3.2? This is confusing, for example in line 10, I thought sites with autocorrelation were removed prior to the MK tests. In line 17, results from the MK and Pettitt tests are compared. Is this for tests at the same sites or different sites? In the algorithm, MK and Pettitt were not done at the same sites. (17) Page 2778, line 21, There is nothing about average month of low flows in my copies of Figure 9. (18) Page 2781, line 10, Abrupt shifts can be climate related and gradual changes can be caused by changes in the management of flows by dams. (19) Page 2781, line 24, It is the case that the USGS flags relate to high flows and not low flows.

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