Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-613-RC3, 2017

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Technical note: Long-term memory loss of urban streams as a metric for catchment classification" *by* Dusan Jovanovic et al.

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Received and published: 30 November 2017

General comments

The paper further investigates the effect of urbanization on stream flows through the long-term (i.e. in large scales or lags) change in the process second-order dependence structure (e.g. autocorrelation function). The review of previous studies is presented in the introduction [section 1] but I believe some additional literature review is required (please see in the minor comments). The above quantification is accomplished for (a) the urbanization through the measurement of the catchment imperviousness (in particular, they suggest 3 ranges of urbanization, i.e. natural, peri-urban and urban,

that correspond to less than 5% imperviousness, in between 5% and 15%, and larger than 15%, respectively) [sections 1 and 3], and (b) the long-term alteration through the estimation of the Hurst parameter (applying two statistical methods, i.e. R/S and MF-DFA) [sections 2.1 and 3]. The proposed methodology is applied to 38 catchments (22 in USA and 16 in Australia, some with no missing data and highlighting the results from the ones with missing data) [sections 2.2 and 3]. Furthermore, the Authors calculate the statistical significance between catchment size, annual rainfall and specific mean discharge (i.e. discharge over catchment size), with no clear significance found (the Authors mention that a weak significance is found between H and annual rainfall as well as specific mean discharge attributing it to the small catchment sizes used) [section 3]. The main conclusion is that a correlation is evident between catchment imperviousness and the Hurst parameter and so, the Authors suggest using the latter as an index/metric of the former [sections 3 and 4].

In my opinion, the idea (of adding H to the several urbanization metrics) looks promising and certainly is worth of attention. The paper is well written and well structured, and previous studies are well documented. The generalization of the results is justified since the proposed methodology has been applied to several catchments in different climatic conditions and continents. However, I believe that the analysis still needs to be improved. Below, I have numbered several suggestions and comments that I hope the Authors will find useful for their analysis and worth of discussing.

Major comments:

1) The basic idea of linking catchment urbanization to the long-term alteration of stream flows is based on the assumption that both are well (and independently) quantified. However, the urbanization is quantified through the catchment imperviousness which is certainly an effect of urbanization but not exclusively, and thus, it may be useful to include additional metrics for the classification of the catchments (e.g. land-use). If (after including other metrics) the classification presented by the Authors still stands then this could be an additional finding of the paper, i.e. that urbanization can be well

classified just by using the imperviousness metric.

An additional possible limitation of just using the imperviousness metric for the urbanization classification, is that there may be catchments that have a small imperviousness metric (and thus, they must have been classified here as natural or peri-urban) but may include upstream civil works (such as dams) which certainly have an effect on the Hurst parameter and in particular, these upstream constructions are expected to cause a large drop of H close to 0.5 (since the release of water in the river would be no longer entirely dependent to the rainfall-runoff natural process). It may be useful for the Authors to check whether such large-scale civil works exist upstream of the stream flow stations, and if this is the case, consider adding a discussion (or even introduce a new classification for them).

Finally, since both imperviousness and streamflow H are affected by urbanization in various ways (and so, a direct comparison between them may not be always illustrative of the urbanization level), the Authors may consider to additionally estimate the H of precipitation at the examined locations of the streamflow stations or to nearby stations within the catchment. If the decreasing level of streamflow H to imperviousness is repeated for the precipitation H (as shown in Figure 1 of the paper), then this will strengthen the robustness of the analysis (based also on the results of Jovanovic et al., 2016 mentioned in the paper). An investigation of the precipitation over the examined catchments is also suggested by a Referee of this paper.

2) The H parameter is one of the key factors of the analysis, and although the Authors have used two methods to estimate it, both suggested methods do not take into account the bias effect (e.g. Tyralis and Koutsoyiannis, 2011) which can be very large for long-term persistent processes and especially when estimated from short-length time series (e.g. Koutsoyiannis, 2013).

Also, Dimitriadis and Koutsoyiannis (2015a) have shown (through Monte-Carlo analysis of a wide range of long-term persistent processes) that one requires at least the

10% of a time series length to estimate the Hurst parameter adjusted for bias, which corresponds to at least 20 years of measurements (the Authors mention this in Ln. 263 but with no references and so, they may consider using this reference to justify their statement), so as to have at least 2 values to estimate the log-log slope of the dependence structure and thus, the H parameter adjusted for bias. Note that the adjustment for bias usually increases the H (< 1) estimation and thus, it could be also mentioned in the paper that.

The above comment on bias is also mentioned from another Referee of this paper. Also, this Referee suggests using the lag-1 autocorrelation coefficient instead of the H parameter. This could be easily done by the Authors if they decide to follow this suggestion and then, they could see how the lag-1 coefficient is linked to the Hurst parameter (normally higher coefficients will correspond to higher H) they have already estimated. I believe this could strengthen the robustness of the analysis even more (but again I believe the effect of bias to the lag-1 coefficient should again be mentioned).

An additional issue worth of discussing is the estimations of H > 1. The Hurst parameter corresponds to the large lag (or scale) behaviour of an ergodic (and thus, stationary) process (Koutsoyiannis and Montanari, 2015) like for example the fractional Gaussian noise, and can be easily quantified (without adjusting for bias) through the log-log slope of e.g. the autocorrelation function. Therefore, an H > 1, corresponds to an increasing autocorrelation function with lag, which comes into contradiction with the originally assumption of ergodicity. As already mentioned by another Referee of this paper, I also believe the H > 1 estimated values in the paper are due to sampling errors and not to non-stationarity (which as explained above the latter conclusion leads to a contradiction).

3) In the Abstract, it is mentioned that '...the relationship between this exponent and level of urbanisation needs to be further examined and verified on catchments with different levels of imperviousness and from different climatic regions' [Ln. 14-15]. However, I could not find in the analysis the effect of the (properly defined) climatic con-

ditions of the catchments to the H parameter. If the Authors would like to add this to the analysis, they could easily do so. Since both temperature and precipitation is already included in the analysis [section 2.2] as well as comments on some climatic impacts [end of section 3], I think it would be useful for the Readers and in favor of the generalization of the analysis, to assign a climatic regime metric (e.g. just the five basic classifications of Koppen-Geiger; http://koeppen-geiger.vu-wien.ac.at/) to each catchment and add a discussion about how (or whether) this has an effect on the H of streamflows and precipitations (it is my belief that different Koppen-Geiger classifications will have different H, .e.g. Markonis et al., 2016; Tyralis et al., 2017).

Minor comments:

1) In my opinion that additional references should be included in the Introduction. Some of previous works that the Authors may find interesting are O'Driscoll et al. (2010), Miller et al. (2014), and references therein.

2) In my opinion, the equations in section 2.1 describing the two methods should be placed in an Appendix.

3) In Ln. 83-84 the Authors mention that the "Seasonal cycles are removed from the original series by subtracting the calendar day mean and dividing by the calendar day standard deviation". However, this is true only for a Gaussian process (streamflows are not-Gaussian distributed). In my opinion the word 'removed' should be replaced with 'approximately removed' or apply a more robust method for de-seasonalization (e.g. Dimitriadis and Koutsoyiannis, 2015b), where each cycle is modeled through a time-varying parameter of the distribution function (which may not be necessarily Gaussian).

4) Please, consider adding extra information on the data used in the analysis, as for example climatic regime (e.g., through the Koppen-Geiger classification), latitude and longitude, percentage of zero values (if any) as well as some statistical characteristics such as mean, standard deviation etc.

5) Ln. 176: how is imperviousness U parameter mathematically defined (for example does it take into account the land-use). I think that it would be easier for the Readers to understand the proposed analysis if a mathematical expression of this parameter is included.

6) In Figure 1 please add somewhere in the legend the process name, i.e. 'streamflows'.

7) Ln. 211-213. The Authors mention that "Between 5% and 15% the values of H appear more scattered. Therefore, the three levels of imperviousness defined from Figure 1 can be classified as natural (U<=5%), peri-urban (5%<U<=15%), and urban (U>15%) catchments based on the corresponding range of values of H.". In my opinion, it is not clear how the natural and peri-urban classification is justified. Perhaps the Authors could add some comments on this and make use of other rather simple statistical metrics to define the limit of 5% between natural and peri-urban catchments. An example could be to justify this value (of 5%) of imperviousness where H is estimated larger than 0.75 (for the R/S method), which is the mean between 0.5 (white noise behaviour at large scales) and $\sim$1 (highly persistent).

8) Ln. 196-197: "This suggests that the increase in impervious cover might cause more precipitation to bypass the groundwater storage", and Ln. 232-233: "Therefore, small catchments may not have sufficient water storage to influence the long-term dependence in flow time series.".

The above sentences seem somehow contradictory, in the sense that if there is no clear relationship between H and catchment size because the catchment sizes are small compared to literature, then how come H is decreasing due to the increase of the imperviousness which has caused more precipitation to bypass the storage capacity? I believe the Authors try to explain this in Ln. 237-239 by mentioning that "Generally, catchments with lower rainfall totals and lower specific mean streamflow are found to have higher long-term dependence due to the longer dry weather periods and consequently longer low flow periods.". However, only minor information is given for the rainfall totals and aridity of the examined catchments. Maybe some further explanation here (and more information provided on the climatic conditions of the examined catchments) could help the Readers to better understand this point.

Spelling and grammar comments:

1) Ln. 187: "15% imperviousness". Please replace this with '15% of imperviousness'.
2) Ln. 192: ". . . due the high precipitation...". Please replace 'due the high' with 'due to high'.

I hope the Authors would find some of the above comments useful to their analysis.

Thank you for the invitation to review this paper.

Panayiotis Dimitriadis

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