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Hydrology and Earth System Sciences
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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The paper presents an interesting analysis where changes in correlations are used to measure human impact. A large set of catchment is considered to demonstrate the applicability of the above idea. The paper is excellently written and organized. I think it is presenting a relevant contribution.

The idea is very interesting and based on physical considerations. In fact, human impact affects the river flow regime by inducing changes in runoff formation. Urbanisation typically induces changes in travel time, as the flow formation is accelerated. Reduction of the travel time makes the hydrograph more peaky and less extended in time. These

changes imply corresponding variations in the autocorrelation function of the river flow time series, which can therefore be used to indirectly measure the human impact.

The paper uses the Hurst exponent as a measure of correlation. The idea is very interesting, but probably needs to be supported by some considerations on the physics of the underlying process. The use of the Hurst exponent implies the assumption that the underlying process is affected by long-term persistence (or long memory) which is not always present in river flows. Therefore, one may argue that the proposed metric is not efficient if long memory is not present in the original (not human impacted) process. To put the question in other words: the reader may wonder why the assumptions of long term persistent process was introduced for the unimpacted process (therefore using the H exponent as a measure of urbanization) instead of assuming that the original process is Markovian (therefore using, say, the lag-1 autocorrelation coefficient to measure urbanisation).

I am not against using the H exponent as a metric for measuring urbanisation, but I think a discussion should be provided for the its validity if long memory was not present in the underlying process. This discussion should take into account that long memory is an asymptotic behaviour and can therefore reliably measured only if long time series are available. When dealing with short series, estimation of the Hurst exponent is affected by large uncertainty and impacted by the presence of short memory (Markovian memory, which vanishes for increasing lags). Conversely, estimation of the lag-1 autocorrelation is affected by much less uncertainty.

Some of the series considered in this paper are very long, others may be too short for allowing a reliable estimation of the Hurst exponent. As a rule of thumbs, one may consider that it's difficult to identify long memory properties when the time series is not extended over several decades. Variability of the process, and the possible superimposition of a Markovian process over the long memory one, matter to determine uncertainty in long memory estimation.

I have a few minor remarks that the authors may consider when revising the paper.

1) Page 2, line 22: it is stated that "The Hurst exponent equals 0.5 for an uncorrelated white noise signal". This is not correct: the Hurst exponent equals 0.5 if the aggregated signal asymptotically converges to uncorrelated white noise. For instance, a Markovian process is correlated, but its Hurst exponent is 0.5. The reason is that a Markovian process asymptotically converges to white noise if aggregated.

2) Page 2, line 26: it is stated that "one would expect the Hurst exponent of urban streams to be closer to 0.5 when compared to rural and natural streams." This is the key of my reasoning above: what if the natural and urban stream has H=0.5, because the underlying process is, say, Markovian? Would the method be not applicable in these cases? In my opinion the idea would be applicable anyway if the lag-1 autocorrelation was considered as a metric instead of the Hurst exponent.

3) Page 3, line 10: the proposed deseasonalisation method works well for monthly data; when dealing with daily data, it leads to the estimation of seasonal averages and variances that are characterised by high day-to-day variability. If this is the case, they should be smoothed. It is possible that for the case of urban catchments the problem is less important. May be a short discussion could be provided. In any case, the problem is likely to be ineffective on long memory estimation.

4) Page 5, line 30: values of H around 0.60-0.65 may be hard to distinguish from 0.5, in view of the estimation bias and uncertainty.

5) Page 6, line 17: estimated values of H>1 have relevant implications on the nature of the underlying process that the authors should discuss. In my view they are likely to be due to estimation uncertainty. From a physical point of view, one should consider that H=1 is estimated for a non-stationary process like the Brownian motion, i.e., aggregation of a white noise. H=1.2 means that the process is non-stationary and, after differentiation, reduces to a stationary short memory process that is negatively correlated. Definitely, H=1.2 does not identify a stationary long memory process. It may be

that H=1.2 is herein obtained because the underlying process is non-stationary after urbanisation, but my feeling is that this is not the case.

6) In general the results are very interesting and definitely deserve to be published. My feeling is that the same results would be obtained by using the lag-one autocorrelation coefficient as metric, with the advantage that the method would rely on lighter assumptions and the underlying theory and practical application would be much easier.

As a final remark, I would like to point out that my preference for the lag-one autocorrelation as metric should be interpreted as my personal opinion, and not as a critic to this study. I am always in favor of simpler methods, but my opinion should be checked on the data and by no means should be taken as a suggestion to change the approach that has been taken here. I just would like to contribute to the discussion and to stimulate new ideas, but I am of course not sure that my intuition is correct. Definitely the overall idea that is presented by the authors is worth considering and the results deserve to be published.

Thank you for inviting me to review this paper.

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