Hydrol. Earth Syst. Sci. Discuss., 3, S599–S603, 2006
www.hydrol-earth-syst-sci-discuss.net/3/S599/2006/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Detecting long-memory: Monte Carlo simulations and application to daily streamflow processes" *by* W. Wang et al.

Anonymous Referee #1

Received and published: 1 August 2006

The paper describes an analysis aimed at assessing the reliability of three methods for detecting the possible presence and intensity of long memory in time series. The considered methods are the Lo's R/S test, the test proposed by Geweke and Porter-Hudak (1983) and the maximum likelihood estimator proposed by Haslett and Raftery (1989). This study follows several previous and similar analyses which considered a higher number of different estimators. In fact, the author of the present contribution restricted their attention to a few techniques and therefore did not consider approaches that were favourably considered by many authors in the past. For instance, Koutsoyiannis (2002) recommends the use of the aggregated variance method (Beran, 1994), while in climatic research many authors (see, for instance, Ribsky et al., 2006) used the detrended fluctuation analysis. The restricted number of approaches considered

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here is a significant limitation of the present study, especially for the analysis of the real observations for which the results are hardly decipherable (see my remarks below).

Overall, I believe the analysis is not rigorous and the interpretation of the results is highly subjective. The authors fail to cite important previous contribution and the results do not provide any additional and useful insight into the topic.

The reasons of my concern are listed here below.

1) The authors decided to use the S-MLE method for estimating the fractional differencing parameter d. It seems that the motivation for using it is its availability in the software S-PLUS. I believe that the Whittle's estimator, which was preferred by many contributions of the previous literature, would be a better choice as it allows to overcome significant limitations of the S-MLE approach. In fact, the Whittle's method has nice statistical properties and its limiting distributions are known. Therefore, it is possible to compute confidence limits for d which allow one to use the method as a statistical (parametric) test.

2) The authors repeatedly state that the efficiency of the long memory estimators increases with increasing sample size. This a well known result that is proved also with theoretical reasoning for many estimators. For instance, the maximum likelihood estimator proposed by Whittle is asymptotically consistent and therefore more reliable for large samples. Moreover, the heuristic methods are affected by the presence of short range dependence, which vanishes for increasing time lags. Therefore the availability of larger samples allows one to better inspect the asymptotical properties.

3) The authors removed the seasonality by subtracting the daily means and dividing by the daily standard deviations. When dealing with daily data, this method provides an estimate of the seasonal cycle which is characterised by significant variability which is not plausible from a physical point of view. When dealing with the sample sizes considered in this paper, it may happen that the seasonal component changes unrealistically from day to day. A proper approach would consider the application of a more feasible

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deseasonalisation method, or at least would smooth the seasonal component obtained here by applying a moving average.

4) In section 3 the authors state that the S-MLE method is a good indicator of the presence of long memory. Later on, in section 4.2, the authors state that the S-MLE method is erroneous and therefore caution must be taken if an estimate of zero is obtained. Such inconsistency in the evaluation of the results and performances of the methods is unacceptable from a scientific point of view. The interpretation of the results is to be reformulated.

5) The authors' conclusion that the observed river flows possess long memory is subjective. First, the results of the S-MLE method are evaluated accordingly to the above misleading interpretation of its reliability. Second, the results of the GPH test are accepted uncritically, even when they are not confirmed by both the other two methods. When the interpretation of the results is difficult, more estimators should be considered.

6) One of the authors findings is that the size of the watershed is not effective on the long memory behaviours. I do not believe this is a useful conclusion. While it is recognised that the watershed size is effective on the intensity of short-memory in the river flows, it is well known that the long memory behaviour cannot be justified only by the basin size. Most of the rainfall-runoff models proposed by the hydrologic literature do not exhibit any long memory structure independently of the basin size. The physical explanation for the presence of long memory should be searched in the intrinsic dynamics of the rainfall-runoff transformation. Being the basin size notoriously influent on the intensity of short-memory, and being the short-memory effective on the reliability of the long memory estimators, the weak effect of the basin size on the estimation of long memory detected by the authors is simply an artefact of the presence of short-memory.

7) The physical explanation for long memory proposed by the authors are not sound. First, the authors are probably not fully aware of the works by Koutsoyiannis (2005)

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who proposed a theoretical explanation for long memory based on the maximum entropy principle. This explanation is very appealing, in my opinion. Second, if one would like to justify the presence of long memory with the groundwater dynamics, a more detailed interpretation should be worked out. For what physical reason the groundwater dynamics should induce the presence of correlation in river flows that does not vanishes in time? Which kind of mass balance and/or energy balance equation can induce such a correlation? Can the authors propose a physical formulation that justifies the presence of long memory? The same reasoning applies if one justifies the presence of long memory in the river flows by claiming that it is induced by the weather. The weather can induce long memory if itself exhibits long memory. Do the authors believe that rainfall is affected by long memory? What would be the physical explanation for it? To propose a physical explanation for the possible presence of long memory in river flows is not an issue that can be dealt with in a few lines of discussion. This topic was discussed by the hydrologic and climatic literature for already more than 50 years. A scientific paper cannot resume in a few lines an interpretation which is not adequately substantiated.

Technical comment The paper is affected by many spelling errors. Moreover, many acronyms are not defined before their first use (like R/S) and some of them are defined twice (like ARFIMA and the backshift operator). There is no mention in the paper of the relationship which links d to H. It is not clear why the authors restrict the sample size of synthetic and observed time series. The literature shows that the longer the record, the more reliable the estimate is. It is not clear why the authors restrict their attention to the gauging stations located along the main channel.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1603, 2006.