

Climate change, Hurst phenomenon, and hydrologic statistics

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Responses to reviewers' comments

Reviewer 1

The paper makes an important contribution in specifying how to estimate statistics in the presence of long-range dependence, where classical estimates of many statistics are erroneous. I am surprised that this has not been done before; I have not been following the hydrological or climatological statistical literature over the last 10 years, so I cannot say for certain it has not. If it truly hasn't, then this paper is long overdue and is a very important contribution to the body of hydrological knowledge. It is eminently suitable for Water Resources Research, and overall is very good.

The paper is technically sound and while I have not verified the derivation of the statistical results they appear to be sensible. The methods are described in sufficient detail. The empirical functions on page 17 are a little clumsy, and alternatives are suggested in the comments.

The paper is well organised, easy to read and grammatical. The figures are generally well executed (exceptions noted in the comments) and support the arguments of the paper. There is no particular reason to shorten the paper. The abstract accurately reflects the contents of the paper.

My only serious criticism of the paper is the over-emphasis on the distinction between stochastic and deterministic processes (mainly in the Introduction, pages 10 and 11) and a repeated tendency to overstate the implications of the analysis – the paper shows the data are consistent with scaling processes but not (as the author claims) that the processes behind the data must therefore be a scaling process rather than a deterministic one with varying means. As such, some of the statements in the conclusions and the abstract should be modified along the lines of the measurements being consistent with a scaling model, rather than being conclusively proved to be due to a scaling process.

I appreciate the positive critique, which is very encouraging and constructive.

The empirical functions describing the variance of the sample standard deviation have been altered, as explained below.

The discussion about stochastic versus deterministic processes has been almost eliminated and the discussion of the implications of the analysis has been rewritten in the lines suggested by the reviewer, as explained below.

Specific comments to the author

I was surprised to discover that this has not been done before. Is there really no previous literature describing statistics for simple scaling processes?

This was a very useful comment that led me to do a more extensive literature search. I found that equations (10) (variance of sample mean, p. 14) and (12) (estimator of the variance, p. 15) are known results [Adenstedt, 1974; Beran, 1994, p. 54 and p. 156] and also equations (41) and (42) (estimators of autocovariance and autocorrelation, p. 24) are consistent with asymptotic expressions due to Hosking [1996]. Of course, I cited these works and modified the text accordingly (e.g., I eliminated derivation of (12)). For all other statistical descriptors (variance of standard deviation, cross-covariances and cross-correlations, distribution quantiles, simultaneous estimation of standard deviation and Hurst coefficient, also including estimators of autocovariance and autocorrelation in non-asymptotic status) I did not find any relevant previous works. Also, I did not find any work regarding the important implications of such mathematical results to hydrologic statistics.

Page 10. I find the discussion here concerning determinism rather unproductive, and does not provide a sufficiently strong introduction to the good work that follows. I do agree with your statement at the end of that page that the separation of chaotic signals such as climatic data into deterministic trends and random fluctuations may not be the best approach. Mandelbrot talked about the lack of distinction between signal and noise in fractal processes (can't remember the exact reference).

However, the argument you present does not lead to this conclusion.

Your argument appears to be:

- the diagnostic character of determinism is that it is predictable
- the trends were identified *a posteriori*, not predicted
- therefore they are not deterministic

For this logic to hold, you need to know not just that the trends were not predicted but that the trends *could not have been* predicted. The "could not have been" condition might be made dependent on the available knowledge (the pragmatic view of determinism), in which case the apparent trends may have been unpredictable at the beginning of the century but predictable using current knowledge, making them deterministic from our current viewpoint. More generously, you could allow that we might yet gain the knowledge to predict these trends, so that they should be considered non-deterministic now but in principle deterministic. But in any case, what does this argument gain?

To support the contention that the "trend plus noise" view of climatic data is misleading, you might be better off demonstrating that there are trends at multiple scales, so trends at one scale seem to be part of the random fluctuations at a broader scale. Or maybe show that "trends" come and go without any apparent cause. From a more theoretical base, you could use the behaviour of a dynamical system that appears to have trends and noise that both arise from a single process.

I almost eliminated all discussion regarding determinism from the paper. I thought I had no other choice as the second review was more critical about this and also I felt that the editor does not approve it. What it remains in section 2.1 of the revised version is the phrase (p. 9, third paragraph):

“In all cases these changes are irregular and, in the absence of an accurate deterministic model that could explain and predict them, are better modeled as stochastic fluctuations on many timescales.”

I hope the reviewer will agree with this. I would like to mention here that several regression models, e.g., linear equations of time, that are typically fitted to time series, although they are typically named ‘deterministic trends’ do not explain anything nor predict the evolution of the time series into the future (unless we expand them, which is very dangerous). Obviously, such trends ‘come and go without any apparent cause’ (to use the reviewer’s phrase), as they are irregular and unpredicted. Of course, the situation would be different if a physically based climatic model existed, which could describe the past and predict the future accurately. However, such a model is not available. To indicate this, I have inserted the following example (p. 6, top):

“For example, in a recent study by Carpenter and Georgakakos [2001] the large-scale climatic model used, when applied to present and past time, explains less than 20% of the observed precipitation variance and, even worse, it results in significant scale bias (model precipitation up to 5 to or up to 25 times smaller than the actual one depending on the choice of the neighboring model grid node, as displayed in their Figure 6).”

In addition, I quote a statement by some specialists in climatic models (this existed in the earlier version of the manuscript, as well) (p. 5, line 11):

“Overall, as von Storch et al. [2001] put it, ‘climate must be considered as a stochastic system, and our climate simulation models as random number generators’.”

Since I have eliminated the discussion about determinism from the revised manuscript, I think I could stop my reply to this comment here. However, I would like to continue it, honestly saying that I do not agree with the reviewer’s comment that to characterize a natural process as stochastic rather than deterministic, we need to prove that ‘*it could not have been predicted*’. Such a proof may be impossible: how can we know today if an unpredictable phenomenon could turn to be predictable with some improved knowledge of tomorrow? If this was correct, all systems should be regarded as deterministic, until someone proves that their evolution could not have been predicted using any potential model. For example, we should regard the throw of dice as a deterministic experiment: after all, its outcome depends on a few collisions of a cube onto a plane, whose deterministic dynamics can be understood much more easily than that of the global climate system.

Thus, classification of a system as a deterministic or stochastic is not a matter of characterizing its nature or structure: after all, every macroscopic physical system can be regarded as deterministic in its structure (here we must exclude microscopic quantum systems, in which indeterminism may be intrinsic). But there are cases where determinism does not help to study and predict many complicate macroscopic systems and in these cases it

is better to use stochastic, probability-based, models. In this regard, I would like to add a quote by *von Plato* [1994, p. 15], whom I cited in the earlier version of the manuscript (but not in the revised version):

“In classical physics (obviously including geophysics – my parenthesis) probabilities are basically nonphysical, epistemic additions to the physical structure, a ‘luxury’ as von Neumann says, while quantum physics, in contrast, has probabilities which stem from the chancy nature of the microscopic world itself. Epistemic probability is a matter of ‘degree of ignorance’ or of opinion, if you permit”.

Finally, I am not happy at all that I have eliminated from the manuscript this material, which I strongly believe was useful, because it put on new grounds some of the fundamental concepts of hydrologic practice, such as the appropriateness of decomposing hydrologic time series into deterministic and stochastic parts.

Page 11, line 1: where you say “the large-scale trends in the time series are closely related to the well-known Hurst phenomenon”, mightn’t it be better to say they “are a manifestation of”? Isn’t an increasing variability with increasing duration exactly what the Hurst phenomenon is?

I rephrased as suggested (p. 9, line 10 from bottom): *“Equivalently, these fluctuations can be regarded as a manifestation of the Hurst phenomenon ...”*

Page 11, second paragraph: Again I think you are making too much of the distinction between deterministic and stochastic processes. Isn’t it true that your results hold in any case wherever the data behave as an SSS? Putting such emphasis on the philosophical underpinnings is, I believe, more likely to result in semantic disputes rather than ready acceptance of your statistical results.

I have deleted the phrase. The reviewer must be right in his/her comment regarding disputes and acceptance.

Page 15, after equation 13. You state without proof that equation 13 is an unbiased estimator regardless of the type of the process. This seems fairly obvious to me, but not absolutely transparent. Can this be demonstrated, or supported by a reference? Or is this so fundamental (e.g., the definition of the mean) that it doesn’t need any support?

I added the phrases (p. 14, below equation (7)): *“As it can be directly verified by taking expected values of both sides of (7), \bar{X} is an unbiased estimator regardless of the type of the process X_i ”* and *“Moreover, it is very close to the best linear unbiased estimator of the process mean for SSS [Adenstedt, 1974; Beran, 1994, p. 150].”*

Page 17, after equation 21. Why is S an “approximately unbiased estimator”? Where does the approximate status come from, if S^2 is unbiased?

I added the phrase (p. 15, bottom line): “*The square root is a nonlinear transformation and, thus, it does not preserve unbiasedness.*”

Page 17, equations 24 and 25. The behaviour of $\kappa(H)$ and $\lambda(H)$ might be better expressed as single functions rather than as piecewise functions. $\kappa(H)$ has a small discontinuity at $H = 0.6$ ($\kappa(0.6+\epsilon)$ is not the same as $\kappa(0.6-\epsilon)$) and $\lambda(H)$ has an abrupt change in slope. Using your functions, I was able to come up with:

$$\kappa(H) = \frac{1.4 - H}{(1 - H)^{1.3}}$$

$$\lambda(H) = \frac{1.02(1 - H)}{(1.02 - H)(1 + H^6)}$$

which are both continuous functions that are reasonably close to your functions. If you chose to adopt this type of function you would need to adjust the parameters to retain the identity with the classical formula for $H = 0.5$.

I am grateful for this suggestion and appreciate the reviewer’s effort to construct these equations. The problem is they that do not comply with the classical formula for $H = 0.5$, as the reviewer notes. I re-studied this issue from scratch and it took me some days of efforts to come up with new simpler and more accurate equations that are not piecewise functions. Thus, the former equation (23) has now taken the form

$$\text{Var}[\tilde{S}] \approx \frac{(0.1n + 0.5)^{\lambda(H)} \sigma^2}{2(n - 1)} \quad (14)$$

which has only one parameter, $\lambda(H)$, instead of two of the former version. This is given by

$$\lambda(H) := 0.088 (4H^2 - 1)^2 \quad (15)$$

It is easily verified that, when $H = 0.5$, (14) shifts to the classical formula. Accordingly, I have changed the former equation (38) – now (28) that refers to distribution quantiles.

Page 23, first paragraph: Figure 7 indeed shows that the true probability distribution lies within the 95% confidence limits of the SSS estimate, but there is a substantial and consistent bias across the distribution. Is this simply because of the effect of using only one sample, and the estimates being uncertain due to the nature of the scaling process, or is there something systematic about it? In other words, if you did the analysis for a large number of synthetic samples would the average estimated distribution converge to the theoretical one or does it remain biased?

Yes, this is simply because of using only one sample. The average empirical distribution converges to the theoretical one, but our purpose here is to demonstrate the uncertainty using one sample, because in practice we have available only one sample. The situation depicted in

Figure (7), i.e., the substantial bias that locates the theoretical distribution outside of the classical confidence limits is typical for about 50% of the samples.

Page 26, end of second paragraph: Claiming that “the empirical autocorrelation function agrees *perfectly* with the model” is a bit strong; something like “fits well” would be more suitable. In fact, I wonder if the irregularities in the autocorrelation plots of Figure 11 (down) actually indicate significant departure from simple scaling. There is an unexplained peak at a lag of about 30, autocorrelation is lower than the expected at around 100, and then increases from about 0.3 to 0.4 from lag 100 to 200 where the model shows a decrease from 0.38 to 0.33. Is that a significant departure? You don’t need to answer that, but you should be more critical of your own results.

I followed the suggestion and replaced “*agrees perfectly*” with “*fits well*” (p. 25, line 4 from bottom).

Page 27, lines 3-5: the temperature anomalies quoted here (e.g. 99% quantile of annual temperature anomaly is about 0.6°C) don’t seem to agree with the values in Figure 12. There appears to be a bias of about -0.3°C between your numbers and those of Figure 12. For that matter, shouldn’t the temperature anomaly at probability of 0.5 be 0? Is this bias the same problem seen in Figure 7? Is it just a plotting problem? If not, can it be fixed?

I have added the phrase (p. 26, line 8):

“We recall from section 2.1 that in this time series the temperature anomalies are expressed as differences from the 1961-90 mean; therefore, the average of temperature anomalies over all 992 years is not zero but -0.30°C ; thus, the difference of the 99%-quantile of the annual temperature anomaly from the average is $0.32^{\circ}\text{C} - (-0.30^{\circ}\text{C}) = 0.62^{\circ}\text{C}$, etc.”

I think this gives sufficient explanation. There is no relation with Figure 7.

Pages 29-31, Conclusions. I agree with your overall conclusions and specifically with the conclusion that statistical analysis of time series must take account of long range variability associated with the Hurst phenomenon. However, I am not sure that you have proved that the time series you explore are results of SSS processes; they are certainly consistent with this hypothesis but that's not the same as identifying the cause of the variability. The same arguments have been made in soil science and in the study of land surfaces: the data show scaling properties, so soils (or landscapes) are fractal, and are the result of a stochastic scaling process. But this does not follow: the same results can be obtained by a variety of non-scaling processes operating at different scales.

Some of the strong statements in the conclusions should therefore be modified. In particular:

- Page 30, line 11. The statement that trends or jumps should not be removed “as the shifts are *in fact* stochastic rather than deterministic” (my emphasis) is not defensible (see earlier comments on the focus on determinism). You have shown (as have others) that the SSS hypothesis is plausible for these time series, but not that they are the result of a scaling process. It could be that there are *in fact* shifts and trends due to specific processes that come and go.
- Page 31, line 10. Change “agree perfectly with” to “are consistent with”.
- Page 31, lines 11-14. The claim that the trends or shifts “are nothing more than regular behaviour” should be amended to something like “are consistent with the scaling hypothesis”.

In the first case, I changed the statement this way (p. 29, line 9 from bottom):

“Observed shifts in such time series were often regarded as deterministic components (trends or jumps) and removed from the time series so that the residual can be processed using classic statistics. This would be an efficient approach if a deterministic model existed, which could explain these components and also predict their future. This, however, is hardly the case, as most typically the trends or shifts are identified only a posteriori and expressed mathematically by equations lacking physical meaning (e.g., using linear regression) and thus applicable only in the available parts of the time series and not in their future evolution. An alternative method is to approach this fact in a stochastic rather than a deterministic manner.”

In the second case, I replaced “agree perfectly with” with “are consistent with” as suggested.

In the third case, I changed the sentence this way (p. 30, bottom line):

“In addition, it is shown that several patterns within these times series would be regarded as evident trends or shifts if classic statistical tests were used, but using modified tests, based on the scaling hypothesis, it turns out that they are regular behavior of the time series, provided that these time series are consistent with the scaling hypothesis.”

Also, I added the following paragraph (p. 31, first full paragraph):

“Apparently, the consistency of geophysical time series with the scaling hypothesis is not exhausted to the three time series analyzed in this paper. In several studies, a large number of geophysical time series has been found to exhibit the Hurst phenomenon, which is equivalent with the scaling hypothesis. Besides, the scaling hypothesis is consistent with the strong conclusion of several climatological studies that climate has ever, through the planet history, changed irregularly on all time scales. The analyses of this paper show that in time series with short length the classic statistics have the property to hide the scaling behavior, because of the bias they introduce. This concerns the sample variance and, most importantly, the autocorrelation function, whose classic estimate hides a fat tail. Therefore, it can be the case that short time series, classified as random noise without scaling behavior, in fact exhibit the Hurst phenomenon.”

The same issues are evident in the Abstract: the statement that “changes of the climate on all scales ... is nothing more than a simple scaling behaviour” is overly strong. Those changes can be described by a scaling model, but that doesn’t mean the processes underlying the behaviour are scaling processes.

I have replaced this phrase with:

“The changes of the climate on all scales are closely related to the Hurst phenomenon, which has been detected in many long hydroclimatic time series and is stochastically equivalent with a simple scaling behavior of climate variability over timescale.”

In summary, what you have shown is that there is a viable alternative explanation for the shifts or trends in the observed data that does not require an explanation of changing trends. In some ways, the hypothesis of a scaling process is simpler than that of shifts or trends, but in some ways it explains nothing. What lies behind these Hurst phenomena? A statistical model is not an explanation but a description. But the point that the statistical models should include the effect of long-term dependence is well made: the statistical description is better when these effects are accounted for.

I absolutely agree with this comment.

Minor comments

Abstract, line 3: Change “leaded” to “led”

Page 9, line 13: local *overyear* average – should that be *multi-year*?

Page 12, line 2: references should be in italics as elsewhere.

Page 18, 3rd last line: change “0.067 is 0.043” to “0.067 and 0.043”.

References: The Hirsch et al 1993 and Salas 1993 references should include a chapter number within the book.

All suggested corrections are done.

Reviewer 2

Very shortly, the papers contains some contributions related to statistical estimation of simple scaling stochastic processes. I find the motivation for the application of this kind of processes to hydrologic or climatic series very confusing, lacking rigor and ignoring important contributions in other fields. Moreover, in the paper there is not an open minded approach to the problem of what can one learn and understand about climate change and climate variability from the analysis of recorded time series. The paper is so weak in this respect that I recommend to take a more simple and pragmatic approach to motivate the presentation of the paper's contributions. It is enough to quote references that have used simple scaling in climate and hydrology, state the definition and to go directly to the present section 3 and the case studies.

I have large difficulties to respond to general characterizations like 'very confusing', 'lacking rigor', 'ignoring important contributions in other fields', 'not open minded', 'so weak'. I am afraid I am not in position to convince the reviewer that the paper is not very confusing, does not lack rigor, etc. I respect his/her view but I do not agree.

In his/her subsequent remarks, the reviewer poses some issues focusing on alternative explanations. If I followed the suggestion to go directly to section 3, eliminating section 2, I would not have the possibility to discuss the alternative explanations at all.

From the physical side, we know that some driving processes of climate and hydrology have known periods or time scales (astronomical variations in solar radiation due to earth orbit parameters, solar variability). We understand some of the mechanisms of climate variability: ice-albedo feedback, CO₂ cycles and green house effects in general, ocean deep water circulation, ocean-atmosphere interactions, land-atmosphere interactions, etc. The dynamics of each one points clearly to dominant time scales. Some of those processes are not independent and interact, for instance ENSO (2 to 6 years time scale) is coupled with the annual cycle. Statistical analysis of climatic time series can ignore this (partial) knowledge? For instance, Thompson, 1994, shows how in the temperature time series one can observe clearly earth orbit precession and anthropogenic effects.

The astronomical variations due to earth orbit are indeed predictable but are apparently out of the scope of the paper. As far as I know, the periods of such phenomena vary between 21 000 years (axial path wobble) and 95 000 years (orbital stretch) whereas the time lengths used in the paper are far smaller. In terms of solar activity, surely, there is an eleven-year periodicity of the solar spots, but as far as I know, there has not been detected a reflection of this periodicity to hydrological processes. I do not think that other variations of solar irradiance are predictable in a deterministic context.

It was not my purpose to dispute the fact that some of the mechanisms of climate variability are understood or to dispute the results of climatic simulation models that are built upon this understanding. However, reading again the Introduction I understood that I gave an impression of dispute, so the reviewer must be right. In the revised version I added the following text, based on the reviewer's comment (p. 4, last paragraph):

"Climatic models describe some of the mechanisms of climate variability that are well understood, such as ice-albedo feedback, CO₂ cycles and greenhouse effects, ocean deep-

water circulation, ocean-atmosphere interactions, land-atmosphere interactions, etc. They are capable to reproduce the large-scale seasonal distributions of pressure and temperature and resemble the large-scale structure of precipitation and ocean surface heat flux, as well as sea surface temperature anomalies related to the El Niño-Southern Oscillation (ENSO) phenomena [e.g., Ledley et al., 1999].”

But there is debate about how successfully this understanding can be utilized in quantitative predictions. I cited a few representative very recent studies that reflect this debate. I did this to illustrate that inaccuracies and uncertainties cannot be eliminated using a purely deterministic approach, so there is some room for stochastic approaches, such as the one I present. I think that the questions I set, which concern the stochastic rather than the deterministic approach, are useful. These are (p. 6, end of second full paragraph):

“(1) Is hydrologic statistics, in its present state, consistent with the assumption of a varying climate? (2) If not, what adaptations are needed to achieve this consistency? (3) Can hydrologic statistics be used to quantify the total uncertainty under a varying climate?”

But before setting and studying these questions, I thought it was necessary to illustrate the current state of affairs in deterministic climatic modeling, giving emphasis to the varying character of climate and to uncertainty issues. This I did mainly quoting experts of climate modeling.

Unfortunately, the reviewer did not give any hint to locate the publication by Thompson, 1994. (I tried to locate it from sciencedirect.com but I found 1125 articles authored or co-authored by someone Thompson in 1994. In the Earth and Space Index database I found 10, but none seemed to be relative).

The paper ignores alternative explanations for the observed statistical behavior in the time series: Large but finite correlation length or scale of fluctuation (Mesa and Poveda, 1993); Composite random processes with components with significantly different scales of fluctuation (Vanmarcke, 1988, pag. 225); Power law trends (Bathacharya et al, 1983); and probably many more. Even in the case the paper will be concentrated in the self similar model, it should mention other alternative models. Most urgently, there is a need for tools that could discriminate among the competing alternative models. The paper does not provide any contribution in this regard.

I appreciate this comment, which helped me to improve the literature review and enhance the manuscript's interpretations.

I am not sure that Mesa and Poveda [1993] provide a concrete explanation. In their introduction, they classify the Hurst phenomenon as *“one of the most important unsolved problems in hydrology”* and later they wonder *“something quite dramatic must be happening from a physical point of view”* whereas in their conclusion they regard the Hurst phenomenon as *“probably the result of a mixture of scales more than infinite memory”*. Anyhow, this is absolutely consistent with the manuscript's interpretation. All these quotes have been inserted in the revision (p. 9, line 8 from bottom, and p. 10, line 11 from bottom).

The monotonic deterministic trend, expressed as a power law of time, that was proposed by Bhattachara et al., [1983] may be a good explanation from a mathematical point of view for an abstract time series. I do not think that it can be appropriate for a geophysical time series because I cannot understand how could a monotonic trend, expressed as a power law of time, could be physically explained. The example time series examined in the paper (and others mentioned in the paper) depict irregular alternating trends that come and go, rather than monotonic trends that follow a simple mathematical law. In any case, I have added in the manuscript the following:

“For example, Bhattachara et al., [1983] have shown that a monotonic deterministic trend, expressed as a power function of time, superimposed on random signals results in a composite time series that exhibits the Hurst phenomenon. ... However, such regular trends are not consistent with what we have observed in the example time series, whose trends appear irregular and which overall look stationary (in accordance with Beran’s [1994, p. 41] observation).”

The analysis by Vanmarcke [1983, p. 225] examines a composite random processes consisting of two components with significantly different scales; this results in a limited range of timescales with scaling behavior (as shown in his Figure 5.12). I think he used two scales for simplicity; if we add more than two components this range expands and Vanmarcke’s observation becomes practically equivalent with the explanation of the manuscript. Therefore, we added the following (p. 10, line 9 from bottom):

“Also, our explanation harmonizes with Vanmarcke’s [1983, p. 225] observation that a composite random processes consisting of components with significantly different scales of fluctuation exhibits the Hurst phenomenon.”

In addition to these, I mention Beran’s [1994] book that gives additional qualitative and mathematical explanations (p. 9, second full paragraph and p. 10, end of full paragraph).

In addition, I would like to point out that the works by Mesa and Poveda [1993], Vanmarcke [1983, p. 225], Beran [1994] and others (e.g., those based on infinite memory) do not assume radically different models. On the contrary, all are based on the standard model (also known as fractional Gaussian noise) that assumes stationarity, scaling behavior (even if this is for a limited range of scales as in Vanmarcke), and the same power function of autocorrelation. The model by Bhattachara et al. [1983] is an exception, because it assumes nonstationarity based on a simplified algebraic function of time, which as described above is not the case for geophysical time series. Therefore, I do not think that there is any need for tools that could discriminate among alternative models, because they are not alternative models but alternative explanations for virtually the same model.

Summarizing, the paper should be reduced to a technical showing only the statistical techniques, and briefly one application.

I followed the alternative offered by the editor for a full paper.

References

Vanmarcke, E. Random Fields: Analysis and Synthesis. The Mit Press, Cambridge, 1988.

Mesa, O. J. and G. Poveda. The Hurst Effect: The Scale of Fluctuation Approach. Water Resources Research. Vol 29, NO 12:3995-4002, December, 1993.

Bathacharya, R. N., Gupta, V. J. and Waymire, E. The Hurst effect under trends. Journal of Applied Probability. 20, pp. 649-662, 1983

All these references, along with another thirteen new references are discussed and cited in the revised manuscript.