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September 11, 2002

MS001001: Are Hydrologic Processes Chaotic?

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GREECE

Dear Dr. Koutsoyiannis:

Enclosed please find three reviews of the above-referenced manuscript and a summary analysis by the Associate Editor. There is little I could add to the extensive feedback that has been obtained. Based on this information, I am rejecting this paper. The AE has indicated that a Technical Note may be appropriate. A TN is a focused presentation of not more than 15 pages including text, tables, figures, and references. Although submission of a comment is given as an alternative by the AE, I do not encourage this. It seems to me that a large effort is needed to sharpen the focus of this paper, to take into account the literature, and to provide an answer to the question the title of this paper poses that is acceptable. The current version of the paper is excessively long and neglects some issues that would influence the search for an answer to the question. I am therefore rejecting this manuscript. I do not happen to be particularly hopeful that a revision can be brought up to standard. However, the decision as to how to proceed rests with the author. Certainly, the reviewers have provided much food for thought.

If you choose to resubmit, please note in your cover letter that the manuscript was previously considered as number 001001. At present, resubmission may be made by sending your contribution to:

Editors
Water Resources Research
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Washington, DC 20009

WRR will soon be converting to an electronic submission procedure, so please check the journal web site at `http://www.agu.org/journals/wr/` to make sure you are following the correct submission procedures. With your submission, please include a detailed statement indicating how you have addressed each of the points raised by the reviewers. Upon receipt, the manuscript will be given a new number. Although it will be treated as a new submission, the editor assigned to the manuscript will ask the same AE to manage the review, if possible, so that the process can proceed efficiently.

I appreciate your sending your manuscript to *Water Resources Research* for consideration and hope that the review comments are helpful to you in presenting your work. I regret the length of time it has taken to provide you with these reviews, but I hope that the insights provided make the wait worthwhile.

Sincerely yours,

William G. Gray
Editor, WRR

Enc. 3 reviews, AE assessment

cc: AE
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AE recommendati'n
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Dear Dr. Gray

The review of WR0001001 by Koutsoyiannis is complete. Three reviews were solicited. The first two reviews received were remarkable in their degree of disagreement. The third review did not really provide an assessment, other than being generally negative. My recommendation is to reject the paper in its current form, and to encourage the author to submit either a technical note or a comment on one of the papers discussed. The breadth and depth of knowledge/discussion presented is rather limited relative to the literature addressed and claims made in the paper. The central methodological points made in the paper may be of some interest, but their impact is hampered by the author's narrow perspective as to how to approach the problem addressed or interpret results. These points have been made before, though not necessarily in the same manner, and the author demonstrates a rather limited knowledge of the decade old debate in the physics literature on the same topic. The most serious deficiency is the basic thesis of the paper, namely, that hydrologic processes can be classified as chaotic or stochastic on the basis of a finite, time series. While the author is justifiably critical of some of the literature that has sought to blindly apply the correlation dimension method to a time series and claim "chaos", an equally myopic perspective hampers his presentation. There is no recognition in this paper that (1) nonlinearity in the underlying dynamics is necessary for chaos, and tests for nonlinearity are an essential part of the classification process; (2) the embedding process for state space reconstruction has an obvious analog and equivalence to generalized Markov dependence for stochastic processes (including the equivalence between a higher order Markov process in a single variable and a lower order vector Markov process); (3) low dimensional, nonlinear dynamical systems observed with noise (measurement or dynamical) can be appropriately modeled as Markov processes, with a nonlinear recurrence function, and with marginal and conditional distributions for the state variable that may be skewed or multimodal as dictated by the recurrence relation and the induced dynamics in state space; (4) given (3), an operational distinction between "stochastic" and "chaotic" processes is not feasible and the question posed in the paper is not a particularly useful one to pursue; (5) rather the inference problem is best posed as the identification of (a) the number of coordinates or dimension that are useful for process characterization and prediction, (b) whether the kernel of the dynamics is linear or nonlinear, (c) the signal to noise ratio, and the nature of the noise process (i.e., is it white or colored, and whether it is additive or multiplicative, and its density function), and (d) evidence of quasi-periodicity, regime behavior or other predictable/limiting structure in the dynamics that is interesting (e.g., non-Gaussian); (6) there are a variety of topological measures used to assess the dimension, and pursue inferences on the underlying dynamics of which the correlation dimension is just one, whose limitations have been discussed extensively in the literature; and (7) the basic issues of sample size and skewed marginal distributions have been discussed in the past with the conclusion that while these pose constraints for the inference problems considered, the nature of the constraint really does depend on the underlying dynamics (e.g., for a periodic, linear system observed with a high signal to noise ratio, the sample size needed is small), and on the degree of sampling of the attractor by the sampling design rather than the sheer number of points. Further, the speculative assertions made by the author as to the nature

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of dynamics at multiple scales or under aggregation of the fine scale dynamics are not supported by any analysis or examination of literature, and are most likely false. The paper appears to have been stimulated in part by the assertion in Sivakumar's paper that the dimension of a daily rainfall series is 1 or smaller. This claim is most likely spurious given that the underlying dynamics is intermittent, and no attempt was made to address the intermittence. It is worth addressing this issue, but this is perhaps best done via a comment on Sivakumar's paper. [redacted] provides a 22 page review of Koutsanysis's paper that raises some good points, and reinforces some of his own misconceptions. It would be much better for this discussion to take place in the published literature. It is my assessment that given the limited perspectives brought forth in the present paper, it makes more sense to recommend that it be recast as a comment on Sivakumar's paper rather than pursuing improvements to this paper and then inviting others to comment.

The first reviewer, [redacted], was extremely prompt and positive, and agreed to be identified to the author of the paper. He indicates that the paper is VERY GOOD – it makes an important contribution in distinguishing between stochastic and chaotic hydrologic processes, and in identifying the sample size and other conditions necessary for discrimination. He made several suggestions of an editorial nature and observed that the paper confirmed his intuition that many of the analyses that reported chaos in hydrologic data may have been misguided. He notes that the paper was instructive to a reader, such as himself, who is not familiar with these methods, and that it far exceeds the recommended length of a WRR paper. He also noted that the treatment of intermittence in the paper was perhaps oversimplified, in that the dynamics of the wet periods may need to be considered rather than mixing the dry and wet periods. This comment reinforces the confusion on part of both [redacted] and [redacted] as to how to treat intermittent dynamics in the context of the approach used here. [redacted]

v.2) [redacted] PROVIDES A 22 PAGE REVIEW rating the paper POOR, and recommending REJECTION. The thrust of the review is that the author has taken a biased view to the problem and that the paper does not adequately or correctly develop the underlying thesis. The most substantive comment is that the paper largely considers the correlation dimension method, whereas the "chaotic" community typically considers a broader set of measures. The reviewer notes that the chaotic vs stochastic debate has been played out a few times in the literature, and objects to the exclusivity of the question "Are Hydrologic processes chaotic?" posed by the title of the paper. While this objection is readily remedied, the comment does get to the futility of the question addressed. Nevertheless, in my view, there is utility in presenting tools and metrics (as this paper does) that allow for a better diagnosis or classification of the underlying system. This observation is tempered by the practical observation that all hydrologic data have "noise", are discretely sampled, and hence even a low dimensional, nonlinear system will be manifest as a stochastic system. Neither the reviewer nor the author effectively address this issue. The reviewer cites several other methods related to nonlinear prediction and

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continuity of trajectories that others have argued provide "better" measures of underlying behavior than the correlation dimension method that is the focal point of this paper. Many of these methods require pre-filtering the data prior to an assessment of nonlinearity or determinism. Without such filtering, a stochastic process description is appropriate. Post filtering, a deterministic kernel of the system may be identified that may be linear or nonlinear, and chaotic, or periodic etc. [redacted]

[redacted] rates the paper as FAIR, indicating that it does not amount to a significant contribution, and recommends REJECTION. However, he did not provide any detailed comments [redacted]

While I have worked on both sides of the problem addressed by the paper, I have not followed the dimension estimation literature (the real subject of this paper) since the early 1990s, convinced that the particular classification question posed by the paper – Are hydrologic processes chaotic or stochastic? (or the subquestion – what are the conditions of application and interpretation of the correlation dimension method or approaches for exploring low dimensional scaling in embedded time series) – is not a useful line of inquiry since it does not unambiguously provide any useful direction for understanding the evolution of the system or properly modeling it. [redacted] Sangoyomi cited by the author appeared in 1996, but was drawn from Sangoyomi's 1992 dissertation, and reported the application of four different methods for dimension estimation, noting that the correlation dimension approach was perhaps one of the most problematic in practice. In Sangoyomi's dissertation, tests with various synthetic series were reported for the different methods compared, and other methods of characterization were also developed and reviewed. The literature addressed (in 1992) was already considerably more comprehensive as to both methodological and conceptual issues than the discussion in the paper under review. I suspect that much more may have been published since, or the topic has been laid to rest. Basically, the inverse problem posed is too weakly constrained to be solved directly from the data. Stronger assumptions as to process structure are necessary to identify the parameters of a stochastic process or of an underlying deterministic process. The search for the "correct" flood frequency distribution that consumed hydrology in the 1970s/80s comes to mind as another example of such misguided effort that detracts from work towards addressing real diagnostic or practical problems. A closer connection that the author seems to miss is the relation between the correlation/fractal dimension and the Hurst coefficient. The latter was the subject of much debate in the 1970s and 1980s, that culminated in the recognition that many subtle factors confound the robust estimation of the Hurst coefficient from a finite data series, and hence caveats and assumptions are endemic in any assessment of long memory/Hurst behavior/low dimensional dynamics etc. Further, much of the literature on the identification of chaotic determinism from time series derives from principles that apply autonomous, or closed systems, and not from open, forced or non-autonomous systems. Spatially extended systems have received limited attention. The latter seem to be the operating paradigm for many of the hydrologic state variables. This observation itself

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renders much of the hydrologic literature related to correlation dimension/embedding analyses inadmissible, and obviates the need for a paper such as the one under review. A more useful and interesting direction could be the treatment of space-time systems to seek organization that relates phase space and physical space.

Most of the methodological work and its applications discussed by the author dates back to the 1980s/early 1990s. The exception is the recent Sivakumar work, which parallels one of the earliest hydrologic papers in the area attributed to Ignacio-Iturbe and others. Given that the intermittent rainfall occurrence process modeled by these authors does not directly correspond to the underlying assumptions of the state space reconstruction process, it is odd that these authors pursued this analysis, and the present paper justifiably attacks these applications. However, an adequate response would be to publish a review comment on these papers in the original journal of publication, pointing out the obvious problem, instead of pursuing a lengthy analysis exploring the issue.

While I agree with the author that the intermittent rainfall process is not a useful one to use for testing the underlying hypothesis using the tools used, given that the underlying assumptions of a continuous differential dynamical system (nor is a symbolic dynamics representation – see for instance Jim Crutchfield's work - that may be more appropriate) are not satisfied, the assertion at the end of the first paragraph on page 16, that if rainfall is stochastic so will be the remaining hydrologic processes driven by rainfall, mis-characterizes the situation. Certainly the surface-ground water hydrologic system is non-autonomous, forced by rainfall, which appears as an exogenous variable in a typical hydrologic model. This is consistent with the author's statement. However, there are two things to consider. First, the author has established that the correlation dimension approach is not likely to give useful insights into the underlying deterministic dynamics, which leaves the "stochastic" model standing, but does not dismiss an alternate, low or high dimensional model for the rainfall process analyzed in the space-time-frequency domain, as it should be given its intermittence, and well known multi-frequency/scale character that is poorly sampled (at least in space). Actually, the author speculates that if the rainfall process is high dimensional at fine time scales, it will also be high dimensional at coarser scales and so will be all variables driven by it. This assertion is contrary to observations in work by Henry Abarabanel and others who note that sensor averaging leads to an easier identification of the underlying determinism and to lower dimensional dynamics. In the stochastic process context, Salas demonstrated that the ARMA(1,1) model could be derived as an appropriate model for streamflow. This is a low order model considering that the underlying dimension of the stochastic processes of space-time rainfall, and basin hydrology is considerably larger. We know that a dimension reduction occurs as we integrate over space to get an ode from a pde. This leads to my second observation that the author's speculation in this regard is not warranted, and his dismissal of the potential for recovering underlying determinism in the "slow" component of the system based on issues with the analysis of intermittent daily rainfall is premature. I suggest that he experiment with a model which couples the dynamics of two systems, one low and one high dimensional. Let's design these such that the low dimensional system ha a relatively slow time scale of evolution that is weakly coupled to the fast variable in the higher dimensional system. Then, if a state variable from the low dimensional system is analyzed it may exhibit a small dimension, while that from the high dimensional system may exhibit stochastic or high dimensional dynamics.

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As the degree of coupling is changed in strength and uni to bi-directional, one can get a spectrum of results. A paper by Bras and Islam in the early 1990s covered some of this ground. All of this testifies to the complexity and futility of such data driven analyses without an underlying hypotheses. In this regard, I find it difficult to be sympathetic to either the authors whose work is criticized in this paper, or the author of the paper. Both groups seem to be relatively oblivious to the underlying problem.

The author presents a relatively long review of the basic literature. The review is generally good, and consistent with readily available material. However, the perspective is oddly limited given its length. For example, consistent with the older references cited, there it should be mentioned that the Takens/Ruelle/Whitney embedding theorem cited for the justification of embedding is actually consistent with the classical justification of the notion of generalized Markovian dependence, and emerged earlier (1900s) in a different guise in the stochastic process context. The book and work by Howell Tong is useful to pursue in this regard. The notion of low dimensional (d<2) and high dimensional chaos is also odd and inconsistent with the literature – yes there may be a citation to Kantz and Schreiber's book for the nomenclature, but I don't think this is consistent with the general physics literature, where high dimensional chaos is usually d>10 to 20 – not that such definitions matter that much. Similar problems appear elsewhere in the text, e.g., "Section 2.4 Typical Procedure for identifying chaos", refers to the application of the correlation dimension procedure, which is certainly not the only one, nor necessarily the most efficient method for estimating the underlying dimension of the system. Further, only if the correlation dimension is non-integer is the system classified as chaotic. It is well recognized that given typical data sizes and estimated dimensions, as well as the nature of the underlying noise processes, this distinction cannot usually be made. Thus, investigators typically report the dimension as one of several diagnostics, rather than as an offer of a proof of chaos. Often, geometric measures (e.g., Kennel's work, or Kaplan's work, or various approaches to the estimation of Lyapunov exponents) are advocated as more robust estimators of underlying dimension and a complementary search for evidence of nonlinearity vs linearity is offered to further advance the possibility of chaos in the time series. The biases inherent in correlation dimension estimates that the author discusses in section 3.2, are in part why many investigators in the physics literature moved to geometric estimates of an appropriate embedding dimension. Incidentally, the author's analysis in section 3.2 where he illustrates that the estimated correlation dimension (which can be non-integer) will usually be lower than embedding dimension for highly skewed data, instead of equal as expected for a random series, has been noted in the past, and accounts for recommendations in the early literature that highly clustered points in the embedding space be subsampled (basically don't count inter-point distances less than some ε in a cluster). This recommendation will also address some of the intermittency problems alluded to in the next section. However, such recommendations merely reflect the problems in applying the correlation dimension approach, and the necessary carpentry that has evolved, rather than the fundamental issue of how best one should identify attractor dimension, nonlinearity, and related measures. A complementary problem is that a small amount of colored noise can make it difficult to distinguish chaotic determinism from a stochastic system. Thus, one can misclassify in either direction. Once again, the problem I see here is that the while the author correctly chastises the misguided amongst

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us who seek to make ambitious claims from a limited understanding of the underlying methods, he is falling into a similar trap with his assertions and bent in this paper.

In summary, I feel that this paper is of some interest, in that it may make some of those tempted to explore correlation dimension analyses think twice and become aware of some of the pitfalls of the analysis. They may even be discouraged from its use altogether, which could be a good thing. On the other hand, the paper does not adequately discriminate between the one method discussed and the larger literature, or recognize the limited set of underlying questions it addresses, or refer adequately to the known mechanical problems and the fundamental issues, nor to the need to explore the different aspects (nonlinearity, etc) of the underlying system instead of making ad hoc assumptions and applying methods to data. This paper would not pass muster in a nonlinear dynamics journal in its current form. My suggestion is that the author revise this paper to be much shorter and simpler. The focus of the paper should be on "technical difficulties attendant to correlation dimension estimates, and consequent spurious claims in the hydrologic literature". Refocused in this way, it should directly present the main problems and their illustration with respect to the correlation dimension. The presentation should be toned down to reflect that the discussion is about correlation dimension estimates, and some of the more directly relevant literature on how to improve such estimates should be cited [redacted]. It would of course be nice to get a more complete manuscript that addresses the broader conceptual issues. However, I think that if the author focuses down and more narrowly and accurately defines the domain he addresses, this paper could be accepted as a technical note. An alternative would be to directly provide this as a comment on one of the Sivakumar papers. Of course this brings up the contentious issue that the author would need to directly substantiate his claim that the authors of past papers (e.g., Sivakumar) did not properly analyze their data. [redacted] this may actually be a good way to go. If this is published as a paper or technical note, the author may generate comments from some of these authors/ reviewers which could be interesting. However, to my knowledge, very few are still pursuing this sort of work, and so this may be moot. Thus, my first recommendation is to recast this as a comment on Sivakumar's paper and let him respond. However, if the author were to pursue this as a technical note, it would be appropriate to address the comments of the first two reviewers, as reasonable, and to succinctly present the criticisms of the application and interpretation of the correlation dimension method. Closing comments that put the use of correlation dimension in perspective relative to the general class of methods of nonlinear time series analysis and characterization of nonlinear dynamical systems would be appropriate.

altered by T. Burns.

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review-II 1

Review of the paper by D. Koutsoyannis entitled "*Are Hydrologic Processes Chaotic?*"

This paper is very good and very appropriate for WRR. It examines whether hydrologic time series that in some previous studies have been found to be chaotic are indeed chaotic or are best represented by stochastic models. The paper concludes what many in the field have felt intuitively, which is that hydrologic processes do not have a low-dimensional chaotic structure. The paper tackles the problem from different angles, conceptual, theoretical and numerical. It includes interesting new results (on the conditions under which the correlation dimension differs from the embedding dimension, the effects of singularities in the marginal distribution of the series, and the length of the record needed to determine the dimension of the attractor with a given level of accuracy), and substantiates its findings through the analysis of synthetic and actual time series. The recommendations I have for the author are mainly editorial and marginal, although some include technical points.

For the most part the paper is written in a tutorial, easy to read style. However, there are places where a reader not sufficiently familiar with chaos theory (including this one!) could be confused:

1. Page 3: The distinction among chaotic, low-dimensional chaotic and high-dimensional chaotic (hyperchaotic) systems is confusing. Is the distinction based on the dimension of the attractor or the number of positive Lyapunof exponents? Chaotic systems are first defined in a way that is later identified as low-dimensional chaos. Are then high-dimensional chaotic systems not really chaotic? Positive Lyapunof exponents, which seem to play an important role in the definition of chaotic systems, are not mentioned anywhere else in the paper.
2. Several times in the paper (e.g. at Page 4), the assertion is made that "stochastic models imply infinitely many degrees of freedom". This should be explained. Most stochastic processes are not infinitely differentiable and do not have analytical realizations. For them (a) derivatives exist only up to a finite order and (b) the infinite past does not uniquely characterize the future. In what sense are these processes "infinite-dimensional"? For example, in which sense is a Poisson process infinite-dimensional? Maybe one should say that they are "not finite-dimensional" and/or that their dimensionality is not defined.
3. Eqs. 4 and 5. Why should x_0 be the fixed point or be part of the limit cycle?
4. Page 9. One reads "The latter [attractor] dimension has important content as it represents the number of local directions available to the system and so it provides an estimate of the number of degrees of freedom needed to describe the state of the system". This is unclear to me. It seems that the number of degrees of freedom needed is the minimum embedding dimension m, not D. Also, the meaning of "number of local directions" is obscure, especially since this number could be non-integer.
5. Page 10. The meaning of the normalized measure μ should be explained when this measure is first introduced. The generalized entropy and associated generalized dimensions are not properties of the set (attractor) A, as stated in the paper, but of the probability measure μ. This is an important distinction, which is at the root of many

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of the shortcomings of using the correlation dimension D_2 discussed later in the paper rather than the box dimension D_0. It would be useful for many readers to include at the top of Page 12 a discussion of how D_0 and D_2 are related.

6. Eq. 14. Explain the meaning of the symbol $\approx$ (asymptotic proportionality as $\varepsilon \to 0$?).
7. Second line after Eq. 14: why is the estimation of $C_q(\varepsilon)$ more accurate than the estimation of $I_q(\varepsilon)$? Maybe the author means "more convenient"?
8. Eq. 15. I could not find the definition of k.
9. Page 14, Section 3. It would be helpful if, before considering the various issues discussed in the various sub-sections, the author would briefly lay out the issues considered and the main findings. For example, at first reading, the analysis of the effects of intermittency in Section 3.1 sounds academic, as one would be tempted to object that the chaotic behavior should apply only to the nonzero periods. This is in fact discussed in subsequent sub-sections, but a forewarning would help the reader gain an early perspective.
10. Middle of Page 16. Why does a one degree of freedom system have a one-dimensional attractor? Should it not be $D \leq 1$?
11. Two lines later, it is stated "Here we did not introduce any additional degree of freedom and thus y_n still has a one-dimensional attractor". I do not follow the logic of this statement. For example, consider the mapping $y_n = 1$ if $z_n < 1$ and $y_n = 0$ otherwise. Also in this case no degree of freedom is added, but the attractor has dimension $D = 0$.
12. Page 17, second paragraph. It is said that the use of $m = 30$-40 when $D \leq 1$ is inappropriate, as values of m as small as 3 should suffice. However, the latter result is based on the capacity dimension, not the correlation dimension.
13. Page 17, last sentence before Sec. 3.2. "As explained above, it is difficult to imagine...". This observation should be amplified and the reasoning made more explicit.
14. Page 18. "It can be easily shown that in random time series the capacity dimension $D_0(m)$ equals the embedding dimension m..." It seems to me that this statement is true under certain conditions, such as continuity of the marginal distribution. For example, it seems that the statement is not true for a Bernoulli Trial Sequence.
15. Eq. 17. In Appendix 1, the author derives conditions under which, for a random series, $D_2(m) = D_0(m) = m$ and then obtains specific results for J shaped distributions. However these results do not seem to be quite correctly summarized in the text. Eq. 17 is stated in the text as a necessary condition, whereas it seems to me that it is only sufficient [there are cases when the integral in Eq. 17 diverges and still $D_2(m) = D_0(m) = m$].
16. Immediately following Eq. 17, one reads: "In addition, we show that in purely random processes following non-symmetric J-shaped distributions, the rule is not valid and $D_2(m)$ is smaller than m." I have two remarks: 1. I read this result as a special case of the general one; hence rather than "in addition", one should say "for example" or something like that. 2. Only for certain J-shaped distributions is $D_2(m) <$ m; it depends roughly speaking on whether or not there is a square-integrable singularity at zero.
17. Page 19, third line into Sec. 3.3: change "finite" to "nonzero". Same at line 5 from the bottom at Page 29.

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18. Page 20, near the middle. I find the statement "although the actual dimension of the system is infinite due to the random character of intermittency" unclear.
19. Page 22, two lines before Eq. 21. "we can assume that ...". Explain why.
20. Pages 22 and 23. Example using a simulated Weibull series. This is a good example. However, in this case D_2 reflects the nature of the singularity of the marginal pdf at zero, which has nothing to do with the box dimension of the attractor. I know that this is one of the points the author wants to make, but then why bother with the estimation of D_2 in the first place?
21. Page 27, first two lines. I suppose "Markovian autocorrelation" should be replaced with "Markovian dependence". Any explanation for the change of the shape parameter k from 1/8 to 0.44? Does this apply near zero or for the entire distribution?
22. Page 37, next to last line. Should "necessary" be changed to "sufficient"? As is, the sentence is unclear.

The paper exceeds by far the length standard of WRR and the author should be encouraged to shorten it. He should try to do so without compromising the tutorial, self-contained style of the presentation. In particular, I find the short reviews of chaos theory and related numerical methods very fitting to the paper and would hate to see them go.

Finally, the author should take the opportunity of the revision to correct many typos and a few grammatical errors in the present manuscript.

review #2
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URGENT! PLEASE RETURN REVIEW BY DEADLINE TO ASSOCIATE EDITOR

Water Resources Research

You have received the accompanying manuscript with the understanding that you have already agreed to provide a review within four weeks. It is important to many parties that the review process proceed at a timely pace. If your review is late or does not materialize, this will significantly delay the review process. The editorial policy of *WRR* is to seek only two reviews for a manuscript in order to optimize the time and talents of the professional community. However, this policy makes it absolutely essential that reviewers adhere to the agreed upon response time. An efficient review process is in the best interests of the authors, the editors, the administrative staff, the reviewers, and the community *WRR* serves. Your willingness to participate in and contribute to this efficiency is greatly appreciated. Thank you for your responsiveness and service!

Please feel free to complete and submit your review in a form most convenient to you. However, please address the questions set forth herein, when applicable, as these topics must be dealt with in a comprehensive review. If you choose to transmit your review via e-mail, it should be handled in a manner that will maintain confidentiality and be accompanied by a request for confirmation of its receipt.

1. CONTRIBUTIONS AND AUDIENCE

   What are the important contributions of this paper? See enclosed comments

2. TECHNICAL SOUNDNESS

   Is the paper technically sound? NO, (see enclosed comments)

   Are the methods described fully? YES

   Is the mathematical development complete and accurate? NO, (See enclosed comments)

3. PRIOR PUBLICATION

   Has this work, or very similar work, been published elsewhere? NO.

4. ORGANIZATION AND STYLE

   Is the paper well written and organized? To a certain extent, (see enclosed comm

   Are all tables and figures necessary? YES

   Can the paper be shortened? YES

5. EVALUATION

   Does this paper make a significant, new contribution <u>in the area of water resources</u>? NO (see enclos comment

   How do you rate the paper? (Outstanding, Very Good, Good, Fair, or Poor) Poor (see enclosed comments)

6. PLEASE PROVIDE DETAILED COMMENTS IN A WRITTEN REPORT

   See enclosed comments

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Comments on "Are hydrologic processes chaotic?" (WRR#0001001)
by Demetris Koutsoyiannis

GENERAL COMMENTS:

Introduction:

Ideas gained from deterministic chaos theory have recently found their applications in a variety of hydrological problems. Such problems include, among others: (1) characterization and prediction of the dynamics of rainfall, river flow, rainfall-runoff, lake volume, and sediment transport; (2) scaling and disaggregation of rainfall; (3) noise (measurement error) reduction in rainfall and runoff; and (4) estimation of missing data. Such studies have certainly yielded encouraging results on the characterization, modeling and prediction of a variety of hydrological phenomena, suggesting the possibility of understanding the complex hydrological processes also from an alternative (low-dimensional) chaotic dynamical perspective in addition to the traditional (high-dimensional) stochastic dynamical view.

The purpose of the present paper (according to the author) is to question the studies that have reported the existence of (low-dimensional) deterministic chaotic behavior in hydrological processes, and to prove that hydrological processes are not chaotic but only stochastic. The study tries to achieve this intended purpose by proving that a careful application of the concepts of dynamical systems, without doing any calculation, provides strong indications that hydrological processes cannot be (low-dimensional) deterministic chaotic. Specifially, the study employs the correlation dimension method to a few time series, both synthetic (random or stochastic) and real-world hydrometeorological series, and reports that such time series do not exhibit low-dimensional chaotic behavior but only stochastic behavior. The study also claims to show that to accurately estimate chaotic descriptors of hydrological processes huge data sets are demanded (which are usually not met in hydrological records) and also attempts to quantify the required data size by statistical reasoning.

The intended purpose of the present paper is not in itself new to hydrology, as the question of whether hydrological processes are chaotic or stochastic has been under considerable debate for a decade or so. Such a debate started when *Ghilardi and Rosso* [1990] questioned the results reported by *Rodriguez-Iturbe et al.* [1989] regarding the presence of chaotic behavior in a Boston storm event. Since then, supports for the possible presence of chaos in hydrological processes and skepticisms and arguments against such results have been made in several studies. More specifically, the argument of the studies that have reported the possible presence of deterministic chaos in hydrological processes has been that such processes could be understood **also** from a chaotic perspective **in addition to** the stochastic view (depending upon the underlying dynamics of the system), whereas the studies criticizing such reports, such as the present one, have been claiming that hydrological processes are **only** stochastic. The criticisms and skepticisms have been based on the observations that the methods employed for chaos identification possess inherent limitations.

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Among the above studies, two are worth mentioning essentially due to their depth in addressing the issue. The study by *Koutsoyiannis and Pachakis* [1996] argues against the presence of chaos in rainfall, by showing that a real rainfall series and a synthetic one generated by a well-structured stochastic model are indistinguishable from each other even if tools of chaotic dynamics are used for characterization. On the other hand, the study by *Sivakumar* [2000] reviews the studies that have employed the concept of chaos to hydrological processes, addresses the important issues in the application of chaos theory to hydrological time series, and provides interpretations to the results reported by such studies. The present paper, as the author states, seems to attempt to proceed a step further than simply express skepticism about the discovery of chaos in hydrological processes, by presenting a more systematic and elaborate analysis on the use of the correlation dimension method for investigating the presence of chaos in hydrological series. In regards to the presentation of the work, the paper is reasonably well written (see below for specific comments), the methods employed are adequately described, and the results are presented fairly clearly (see below for specific suggestions and comments).

Having said the above, the quality and suitability of this paper for publication in *Water Resources Research* should be assessed in the context of the following observations. Based on such observations, which not only raise fundamental questions on the intended purpose of the present study and the inaccuracy and incompleteness of the analyses carried out but also provide numerous examples on why the hypothesis of the presence of low-dimensional chaos in hydrological processes cannot be eliminated, I recommend **rejection** of the paper for publication in *Water Resources Research* **in its present form**. However, I believe that a **significant extension and substantial revision** of the paper, **incorporating the following observations and suggestions, might possibly bring the paper to the level of publishability** in *Water Resources Research*.

Such a significant extension is absolutely necessary, since **the present study questions all the studies** that have reported the presence of low-dimensional determinism in hydrology until now and also concludes strongly and unambiguously that all hydrological processes are only stochastic, **without any exception**, irrespective of where, when, and how they occur. I envision that such an extension could *either* strengthen the claims of the present study that hydrological processes are not low-dimensional chaotic but only stochastic (though such a claim, in my opinion, cannot be universal) *or* contradict the claims of the present study and strengthen the claims of the studies that have reported the possible existence of low-dimensional chaotic behavior in hydrological processes. Either way, the outcomes would shed more lights on not only whether or not the deterministic chaos theory is suitable for understanding hydrological processes but also where and when, if found suitable, though the possibility of still further questions and debates on the same issue cannot be excluded completely. I, therefore, encourage the author to revise the paper, responding to the following comments and suggestions, so that the present findings as well as those of the extended study could reach out to wider audience.

<u>Are the methods employed adequate to provide strong conclusions?</u>

Even though the intended purpose of the present paper is to show that hydrological processes, without any exception, are not chaotic but only stochastic, the paper does not provide convincing evidence to support such a claim. A fundamental

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question while criticizing the studies reporting the presence of chaos in hydrological processes has been that the correlation dimension method, in particular the Grassberger-Procaccia algorithm [e.g., *Grassbeger and Procaccia*, 1983a], has a number of limitations when employed to hydrological time series [see, for instance, *Ghilardi and Rosso* [1990] and *Koutsoyiannis and Packakis* [1996]), since the method is designed for infinite and noise-free time series whereas hydrological time series are always finite and very often contaminated with noise. The present paper is no different with respect to this question, as it attempts to show that the correlation dimension method has its own limitations (in regards to data size, identification of scaling region, etc.) and when employed to a host of synthetic (random) and hydrometeorological time series does indeed provide strong indications to the stochastic behavior of such series. As a result, the goal of the present study is **limited to** the investigation of the **reliability of the correlation dimension method** when applied to hydrometeorological time series rather than the investigation of the (non-) existence of chaos, which can only be dealt with by viewing the problem in a much larger context. The importance of this statement lies in the fact that though the correlation dimension method is (one of) the most fundamental method(s) used to distinguish between chaotic and stochastic systems, it is neither a necessary nor a sufficient method for chaos. Because of the inherent limitations of the correlation dimension algorithms, neither a low correlation dimension does guarantee the presence of chaos [e.g., *Osborne and Provenzale*, 1989] nor a high correlation dimension (or rather a non-saturation of the correlation exponent with embedding dimension) is an indication of the absence of chaos.

The observations just made point to a fundamental limitation that exists in the present study. The study questions the past studies reporting chaos in hydrological processes only based on the limitations of the correlation dimension method since any interpretation of the (low) correlation dimension could be a "mistake," but unfortunately ends up doing the same "mistake," to prove that the hydrometeorological time series are not chaotic but only stochastic. Just as some properties of the time series (e.g. small data size) and wrong choices of the parameters (small delay time, inappropriate radius or scaling range) involved in the correlation dimension algorithm may provide a significant underestimation of the correlation dimension [e.g., *Havstad and Ehlers*, 1989; *Nerenberg and Essex*, 1990; *Sivakumar*, 2001a], some other properties of the time series (e.g. presence of noise) and wrong choices of the parameters (large delay time, inappropriate radius or scaling range) may provide a significant overestimation of the correlation dimension [e.g., *Schreiber and Kantz*, 1996; *Kantz and Schreiber*, 1997; *Sivakumar et al.*, 1999b, 2001d; *Sivakumar*, 2000]. See below for specific details.

In view of the limitations that lie with the correlation dimension method, it is imperative to employ additional, more elaborate and rigorous techniques in order to provide more evidence and strong conclusions regarding the presence/absence of chaos/stochasticity [e.g., *Porporato and Ridolfi*, 1996, 1997; *Sivakumar*, 2000]. Such a procedure has been adopted by many, if not all, of the studies that have reported the possible presence of chaos in hydrological processes to support the (low) correlation dimensions achieved at the first instance [e.g., *Rodriguez-Iturbe et al.*, 1989; *Jayawardena and Lai*, 1994; *Porporato and Ridolfi*, 1996, 1997; *Puente and Obregon*, 1996; *Sangoyomi et al.*, 1996; *Sivakumar et al.*, 1999a, 2000, 2001d, e; *Jayawardena and Gurung*, 2000; *Lambrakis et al.*, 2000; *Islam and Sivakumar*, 2001]. On the other hand, some studies have reported the **possible presence of low-dimensional chaos** in hydrological processes **without even employing the correlation dimension method,**

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but only based on the near-accurate prediction results achieved using a phase-space reconstruction approach [e.g., *Liu et al.*, 1998; *Sivakumar et al.*, 2001b; *Sivakumar*, 2001b]. In this regard, the author certainly fails to provide more evidence that could verify and support the results and conclusions of the present study and, therefore, additional verificiation and support for the present results is absolutely necessary.

Further necessity for such verification arises in the wake of the results reported by some of the past studies that a low-dimensional chaotic approach yields better predictions than a stochastic approach for a variety of hydrological and meteorological series, such as rainfall, runoff or discharge and sea surface temperature [e.g., *Jayawardena and Lai*, 1994; *Jayawardena and Gurung*, 2000; *Elshorbagy et al.*, 2001; *Lisi and Villi*, 2001]. Also relevant is the point that a low-dimensional chaotic approach has been found, in certain cases, to perform better than or equally good as artificial neural networks in the prediction of hydrological processes [e.g., *Lambrakis et al.*, 2000; *Sivakumar et al.*, 2001c, d]; the artificial neural networks, in turn, have been found to perform better than or equally good as stochastic approaches for a variety of hydrological processes [e.g., *Shamseldin*, 1997; *Abrahart and See*, 2000]. In addition to these, a number of studies have reported near-accurate predictions for hydrological processes using low-dimensional chaotic approaches, though comparisons with other techniques have not been attempted [e.g., *Abarbanel and Lall*, 1996; *Porporato and Ridolfi*, 1996, 1997; *Liu et al.*, 1998; *Sivakumar et al.*, 2000, 2001b, e; *Islam and Sivakumar*, 2001; *Sivakumar*, 2001b].

In view of the above observations, providing interpretations and conclusions regarding the presence/absence of chaos based only on the results achieved from the correlation dimension method is **very misleading and probably wrong**. Therefore, the **present study is certainly incomplete** in its intended purpose. The author should support the present results with additional evidence by employing other chaos identification methods as well and verifying the results. Such methods should include, but not limited to: (1) false-nearest neighbor method [e.g., *Kennel et al.*, 1992]; (2) Lyapunov exponent method [e.g., *Wolf et al.*, 1985] (also mentioned in this study, see below); (3) Kolmogorov entropy method [e.g., *Grassberger and Procaccia*, 1983b]; and (4) nonlinear prediction method [e.g., *Farmer and Sidorowich*, 1987], including the deterministic versus stochastic approach, i.e. DVS algorithm [e.g., *Casdagli*, 1991]. Particularly, the application of nonlinear prediction method is essential to verify the present results, since a number of studies, as mentioned above, have reported near-accurate predictions for hydrological processes using a low-dimensional chaotic approach. This is particularly important since the main purpose of characterizing a system, whether chaotic or stochastic, is to select the appropriate model for its modeling and prediction (see below for more details).

<u>Are the data sets analyzed representative enough to provide general conclusions?</u>

In the present study, only a few (five, to be precise) hydrometeorological time series are analyzed. Such a very small number of data sets cannot be representative of the hydrological and meteorological processes all over the globe, which certainly exhibit significant variabilities both in time and in space. Therefore, the **universal generalization** of the present results regarding the absence of chaos in hydrological processes **is highly misleading**. Only the investigation of a large number of time series observed in different geographical and climatological regions that represent, at least to

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some extent, the hydrological processes could provide additional support to the present results, if not definitive and conclusive.

It is also important to note that the five hydrometeorological data sets analyzed in the present study may actually be stochastic, but not chaotic, just as the synthetic data sets used are (see below for more details). If this is indeed true, then the author's conclusion generalizing that hydrological processes are not chaotic, but only stochastic, is wrong. This is why the analysis of more data sets is recommended. An example for such a possibility is the study conducted by *Rodriguez-Iturbe et al.* [1989], which reported the existence of low-dimensional chaos in the storm data observed in Boston but not for the weekly rainfall data in Genoa. The evidence of determinism in the Boston storm record was also reported by *Puente and Obregon* [1996] from a rather different perspective, the deterministic fractal-multifractal perspective. It is relevant to note at this point that such a deterministic fractal-multifractal approach was also found suitable for modeling a host of hydrological and hydraulic processes, such as rainfall [e.g., *Puente and Obregon*, 1996; *Puente et al.*, 1999; *Obregon et al.*, 2001a], groundwater contaminant transport [e.g., *Puente et al.*, 2001a, b] and turbulence [e.g., *Puente et al.*, 1999]. To be more specific, the above **deterministic fractal-multifractal procedure can generate highly irregular time series similar to the random series generated in the present study**, which the author claims **only** stochastic (see, for instance, *Puente and Obregon* [1996] for the kinds of irregular and random-looking series that can be generated using such an approach). Also, a recent study by *Obregon et al.* [2001b] reveals that both chaotic and stochastic properties can be obtained via deterministically generated multifractal measures, depending upon the parameters involved in the procedure.

In addition to the above, the synthetic data sets considered in the present study for demonstrating the problems that exist in the correlation dimension method raise serious concerns about the intended purpose of the study. On the one hand, the author agrees that the selection of the range of scaling region, the minimum data size, and others, are important in the estimation of the correlation exponent and, hence, the correlation dimension. On the other hand, the author argues that the past studies that have reported low correlation dimensions and, hence, the presence of chaos in hydrological processes have used, for instance, wrong scaling regions and small amounts of data. Under the circumstances of uncertainty in the selection of scaling region and minimum data size, **the best way to demonstrate the potential problems, if any, is through the analysis of both stochastic and chaotic time series generated synthetically**. In fact, since the intended purpose is to show that hydrological processes are not chaotic, it is more important to demonstrate such problems on well-known synthetic chaotic data than on synthetic random data. The failure by the author to demonstrate the approaches proposed on any synthetic chaotic time series eliminates the possibility of verifying whether the demonstrations and the interpretations presented in this study are indeed correct. For instance, the author argues that the correlation exponent is underestimated for random series even for an embedding dimension of one, by selecting a particular scaling region (which the author claims correct), but fails to verify whether such a scaling region is indeed correct. In fact, a perfectly deterministic system with just one degree of freedom {i.e., $X_{i+1} = X_i + 0.01$} provides a correlation exponent of 1.0 for an embedding dimension of one (and even up to 10) when the number of data points is 10000, indicating no underestimation [**Figure 1** attached herein]. Similar results are observed for a well-known synthetic chaotic data set as well, such as the Henon map [*Henon*, 1976], when the number of data points is as low as 5000 [**Figure 2(a)** attached herein]. Also see

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Sivakumar [2000]. In fact, the scaling region selected by the author could be wrong, in particular when noise is present in the data (see below for more details).

The need to analyze synthetic chaotic series and even completely deterministic series in order to select the scaling region and minimum data size and, hence, to characterize a system, may also be supported by the following. Irrespective of the length of data used: (1) Not all processes yielding finite and low correlation dimensions are stochastic (as small and large data size alike for chaotic systems and completely deterministic systems yield the same correlation dimension); (2) All low-dimensional chaotic systems yield low correlation dimensions (theoretical results available on many artificial chaotic systems); and (3) Most stochastic processes yield infinite correlation dimensions. The presence/absence of low-dimensional chaos is dependent upon the time series (or system) under investigation. The author's argument regarding the issues of scaling region and minimum data size could be wrong, for the reasons discussed below.

In view of the **analysis of only the random series** in this study, it is my opinion that the author **fails to adopt a balanced and unbiased approach**. A systematic analysis of a synthetic chaotic time series (i.e. with different lengths of time series, different amounts of noise levels and selection of scaling region), whose dimensions and other properties are known apriori, could have easily helped verify the author's arguments and the guidelines presented (see below for details).

SPECIFIC COMMENTS:

1. Selection of Scaling Region: The author states [Page 20, Line 1]: "The correlation dimension is theoretically determined for $\varepsilon \rightarrow 0$, which means that in practice the lowest possible region of the length scale must be used in estimations." And also [Page 19, Last Para]: "therefore, looking for correlation dimensions in a fine scale rainfall series is totally useless: the correlation dimension is simply zero for any embedding dimension. Positive estimated dimensions, such as those in the range 0.95-2.5 (items #6 and #12 in Table 1) simply indicate that a wrong range of scale length ε was used."

   In general, the correlation exponent is theoretically determined for $\varepsilon \rightarrow 0$. However, this need not be the case in every situation, as it depends upon the type of data one is dealing with. It may be recalled that the correlation dimension method is designed based on the assumptions that the time series is infinite and noise-free. Therefore, the above guideline of selecting the slope at $\varepsilon \rightarrow 0$ is valid only for such a time series. However, hydrological time series are only finite and noisy. In fact, as is clear, the synthetic random time series generated in the present study are themselves finite and noisy. For such finite and noisy time series, at one extreme, there is a separation of ε below which there are no pairs of points; that is, it is "depopulated." At the other extreme, when the value of ε exceeds the set diameter the correlation integral increases no further; that is, it is "saturated." Therefore, the region sandwiched between the depopulation region and the saturation region is chosen as the scaling region.

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In view of the above, additional caution has to be taken when one deals with noisy time series. The presence of noise influences the estimation of the correlation dimension primarily from the identification of the scaling region. Noise may corrupt the scaling behavior at all length scales, but its effects are significant especially at smaller length scales. If the data are noisy, then below a length scale of a few mulitples of the noise level, the data points are not confined to the fractal structure but smeared out over the whole available phase-space. Thus, the local scaling exponents may increase.

The influence of the presence of noise in the selection of scaling region may be explained with reference to the correlation exponent plots obtained for a noise-free artificial chaotic (Henon) time series and also noise-added series [**Figures 2(a) and 2(b)** attached herein]. As can be seen, for the noise-free time series, a large scaling region is easily visible at very low length scales $\varepsilon \rightarrow 0$ (i.e. between –2.0 and 0.5 on a log-log scale in this case) and, therefore, the estimation of the correlation exponent is very straightforward (i.e. equal to 1 for embedding dimension 1). [See also **Figure 1** for results obtained for a completely deterministic system.] However, such a selection is not possible in case of the noisy series. If one chooses the scaling region at $\varepsilon \rightarrow 0$, then the correlation exponent will be less than 0.5 and approaching zero for all the embedding dimensions considered. This will certainly be an underestimation of the dimension. In fact, if the time series contains noise (i.e. departing from determinism and approaching random), then a higher correlation dimension must be obtained. Therefore, the selection of the scaling region at $\varepsilon \rightarrow 0$ will be wrong and one has to find a scaling region appropriately, which is approximately ε between -0.5 and 0.5 (correlation exponent equal to 1 for embedding dimension 1), though the presence of noise makes it difficult to be accurate. A similar explanation can be provided using the results obtained for a completely random series [**Figure 3** attached herein]. As seen, if one chooses a scaling region at $\varepsilon \rightarrow 0$ (between –2.0 and -1.0 on a log-log scale in this case), then the correlation exponent will be an underestimation for dimensions of 3 and above. Therefore, the appropriate scaling region will be between –1.0 and –0.5, for instance, if one agrees that for a random series, a correlation exponent of about 3.0 should be observed for an embedding dimension of 3.

In view of the above observations, the scaling regions selected by the author both in regards to the synthetic random series and real hydrological series are wrong. In fact, for the cases shown in Figures 4, 8, 9, 10, 11, 12, and 13, there is no need to consider ε below a certain value (e.g. 1E-03), since below this value there are no pairs of points and, hence, the correlation exponent will be zero. The author should have considered ε only above this range, which would also have resulted in the intervals of ε closer than those resulted from the present analysis and thus a large number of points within the considered ε. This is also important considering the fact that the author claims the presence of "inadequate

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area," which could have been averted by considering appropriate intervals of ε, as just discussed.

For the reasons stated above, the correlation exponents estimated by the author are certainly underestimations of the actual dimensions. The underestimations are not because of the problems in the correlation dimension method or data size (see below for details), but because of an error in the implementation of the procedure by the author in the selection of the scaling region. Therefore, the author's argument that past studies have chosen wrong scaling regions for correlation exponent estimation are certainly questionable. The author would have realized the problem in the selection of the scaling region and, hence, avoided the inaccurate estimation of the correlation exponents of the data used in this study had the author employed the method on a known artificial chaotic time series.

2. Minimum Data Size:

It must be admitted that there is no clear guideline as to the minimum (if indeed the case) size of data required for correlation dimension estimation of a data set. As mentioned above, it may depend upon the time series under investigation. However, it is also important to note that a strict guideline on the minimum size derived based on the embedding dimension alone [e.g., *Smith*, 1988; *Nerenberg and Essex*, 1990] may not be useful for the correlation dimension analysis and the interpretation of the results. If one has to strictly follow the guidelines provided by *Smith* [1988] and *Nerenberg and Essex* [1990], and also in the present study, then for a data size of 10000, one may not be able to obtain accurate correlation dimensions for even an embedding dimension of as low as 2 or 3. However, the results presented in **Figures 1 and 2 (and also 3)**, attached herein, for a purely deterministic series and a chaotic series tell an entirely different story. In fact, the number of data points used in the deterministic series is 10000 and that in the chaotic series is only 5000. As can be seen, accurate estimations of correlation dimensions are obtained for an embedding dimension even up to 10.

The above observations reveal that the accuracy of the correlation dimension estimate depends primarily upon whether the length of the time series would be sufficient to represent the changes the system undergoes over a period of time, rather than the data size in terms of the number of values in the time series. As correctly pointed out by *Sangoyomi et al.* [1996]: "Perhaps, the most serious factor is whether the underlying dynamics has been sampled in a representative manner; one could collect 10^6 points in the first 1 s of a multiyear experiment and learn virtually nothing from them about the overall dynamics. On the other hand, a few thousand points collected over the entire experiment may be meaningful." The study by *Sivakumar et al.* [2001d] provides support to this, by comparing the correlation dimension estimate of a runoff series (with only 576 monthly values) with the optimal embedding dimension (or number of variables required for modeling the series) obtained using the phase-space reconstruction prediction method and also with the optimal number of inputs obtained using artificial neural networks. The

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dimension results obtained from all these three methods are consistent. It is relevant to note that near-accurate predictions for the above runoff series are achieved both with chaotic and neural networks approaches, indicating the appropriateness of the approaches.

Therefore, the author's argument that the low correlation dimensions reported by past studies for hydrological processes are the result of small data size used therein could well be wrong. Also, the guidelines presented by the author for the minimum data size are questionable; they may also be wrong due to the wrong selection of the scaling region, which was discussed above. The analysis of a synthetic chaotic series with different data lengths could have given more insight in this regard, which the author fails to present.

3. The author states [Page 3, Para 1]: "Specifically, it became clear that a simple deterministic system, even with one degree of freedom, can have a complex, random-appearing evolution. Obviously, however, the inverse is not true: Complex or erratic-appearing phenomena can be chaotic and thus deterministic, but they can also well be random."

    In using the phrase "however, the inverse is not true," the author seems to interpret that the studies that employ the concept of chaos do so because they assume that all complex or erratic-appearing phenomena are chaotic and thus deterministic, because simple deterministic system, even with one degree of freedom, can have a complex random-appearing evolution. This is not at all true. The studies that employ the concept of chaos do not question the high-dimensional stochastic nature of hydrological processes, but also do not exclude the possible chaotic nature of such processes. Since, hydrological processes vary both in space and in time, the chaotic or stochastic nature of the process is system dependent.

4. The author states [Page 4, Para 1]: "The systems with very many positive Lyapunov exponents are better modeled based on stochastic models. Theoretically, stochastic models imply infinitely many degrees of freedom (infinite dimensional systems)."

    It is important to note that a number of past studies have reported the possible presence of chaos in hydrological time series based on the single (largest) positive Lyapunov exponent obtained, in addition to the low correlation dimensions observed. In view of the suspicion raised by the author regarding such studies, the author should estimate the Lyapunov exponents for the time series analyzed in the present study so that the present interpretations and conclusions can be verified.

5. The author states [Page 6, Para 1]: "Specifically, it (the present study) shows that the hypothesis that hydrologic time series manifest stochastic, rather than chaotic, systems cannot be rejected using the standard procedures of chaotic analysis."

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The standard procedures of chaotic analysis, such as the correlation dimension method, are in fact designed only to distinguish or characterize whether a system is chaotic or stochastic, not to reject the hypothesis that hydrologic time series manifest stochastic nature. After all, the author has employed the correlation dimension method only to show that hydrologic time series are indeed stochastic. Also, such studies have adopted the necessary caution in interpreting the results.

6. Pages 14, 15, and 16, Section 3.1: Use of Whitney's embedding theorem

The author argues about the embedding dimension, m, required to embed the attractor based on Whitney's embedding theorem [*Whitney*, 1936]. According to this theorem, a dynamical system with an attractor dimension d, can be characterized by embedding it in a phase-space of $m = 2d+1$ dimensions. This was also supported by *Takens* [1981]. However, recent studies have suggested other guidelines in this regard. For instance, *Abarbanel et al.* [1990] suggest that an embedding dimension just greater than the attractor dimension (i.e. $m > d$) would be sufficient to embed the attractor. On the other hand, according to *Fraedrich* [1986], the nearest integer above the correlation dimension value provides the minimum dimension of the phase-space essential to embed the attractor, and the value of the embedding dimension at which the saturation of the correlation exponent occurs provides an upper bound on the dimension of the phase-space sufficient to describe the motion of the attractor. The lower bound (or upper bound) on the dimensions required for embedding an attractor is still a question remains unsolved; however, it is relevant to note that many recent studies have reported near-accurate predictions of both synthetic and real hydrological (and other natural and physical) processes by embedding the attractor in a dimension that is the nearest integer above the correlation dimension and increasing the embedding dimension have not yielded better results [e.g., *Lambrakis et al.*, 2000; *Sivakumar et al.*, 2000]. Some studies have also verified this with the number of inputs required in artificial neural networks to obtain the best predictions [e.g., *Lambrakis et al.*, 2000; *Sivakumar et al.*, 2001b, d]. In fact, the number of inputs required has been found to be less than the correlation dimension of the attractor, suggesting that the correlation dimensions might have been overestimated, possibly due to the presence of noise.

As of now, it is not clear whether Whitney's embedding theorem is valid for any type of system. Based on the accurate predictions reported by recent study with embedding dimensions much less than $2d+1$ dimension, the best way to verify the dimension requirement is to carry out a prediction analysis and find the optimal embedding dimension. The author can easily do this by predicting the hydrometeorological time series using the nonlinear prediction method (or artificial neural networks), mentioned above. This could be one way to present a more elaborate and complete analysis of the present study.

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7. The author states [Page 15, Para 2]: "Of course, this is not true, because at some time the system will depart from the 'attracting' zero point. Thus, the system that is described by the rainfall depth is not deterministic but rather stochastic and thus it does not have a finite dimensional attractor."

    The author argues that a departure of the system from an "attracting" zero point cannot be deterministic, but only stochastic. However, such an argument could be wrong. This can be easily explained as follows: Assuming that one considers a rainfall (or runoff) time series measured over a particular period of time that contains one particular storm (or flood) event in between no rainfall (or runoff) periods. In such a case, the rainfall (or runoff) time series could follow a deterministic and predictable behavior, since when the rainfall event starts, the depth of rainfall (or quantity of runoff) departs from zero, could increase gradually, reach its maximum, and could decrease gradually. In this case, though there is a departure of rainfall (or runoff) from zero, it need not be stochastic, but deterministic. The zero values present in the above time series will not result in an "attracting zero point" throughout the time series, and a departure from that attracting zero point could form an "attracting region." The same explanation applies when zero rainfall (or runoff) values occur in between other rainfall (or runoff) values.

8. The author states [Page 15, Last Para]: "However, if the rainfall process is high- or infinite-dimensional on fine timescales, naturally it will be of high- or infinite-dimensional on coarser time scales as well. In addition, since rainfall is the input that mobilizes all other hydrologic processes in a catchment, the number of degrees of freedom of any other hydrologic process (e.g. streamflow) will be at least equal to that of rainfall. Moreover, if rainfall is indeed stochastic, stochastic will be all other hydrologic processes in the catchment."

    By the above argument, the author seems to suggest that, for instance, annual rainfall process (coarser timescale) is equally or more variable than daily rainfall process (finer timescale). In other words, the author suggests that daily rainfall process is more predictable than annual rainfall process. Such a suggestion is clearly wrong, as is very well known with our observations and experience. The above argument is correct only if rainfall process at coarser timescales is an amplication of rainfall process at finer timescales, which is not true for hydrological and meteorological processes.

    A similar explanation (i.e. amplification in the watershed) goes for the variability of rainfall and other hydrological processes in a watershed. The author fails to note that rainfall process is dominantly dependent on the mechanisms involved in the atmosphere, most of which have no or very little influence on the runoff occurrence in a watershed. Therefore, argument on the variability of a process based on an input-output mechanism is not valid for every system. On the other hand, the author should remember that rainfall is dependent on runoff too (for evaporation) in the hydrological cycle. This is where the concept of low-dimensional

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chaos becomes very clear, as it deals with the dominant mechanisms or variables. In fact, rainfall and catchment characteristics are the dominant variables influencing runoff, whereas condensation in the atmosphere is a dominant variable influencing rainfall but not runoff, as it does not have direct influence on the latter. Eventually, the influence of such a variable on runoff may be excluded in runoff modeling. In fact, the influence of all the mechanisms that occur in the atmosphere is reflected in the rainfall process and, therefore, rainfall time series is indeed sufficient for runoff prediction, in addition to catchment characteristics (see below for more details).

9. The author states [Page 17, Para 1]: "the correlation dimension of this 2- or 3-dimensional space filling cloud could be 1 or even less, but this is totally irrelevant. What matters is the fact that the cloud of points fills up space and, thus, the capacity dimension equals the embedding dimension."

   The author's argument that the cloud of points filling up the phase-space is the only one that matters is well taken. In this regard, however, the author fails to note that for many hydrological time series, in particular runoff, the cloud of points does not fill up the entire phase-space, but form an "attracting region" even in an embedding dimension of as low as two [**Figure 4(b)** attached herein, see below for details]. Therefore, the author's argument that all hydrological time series fill up the entire phase-space and, hence, are only stochastic is wrong. The attracting region is the region where the system evolves and settles and, therefore, is may be an indication of low-dimensional chaos. In fact, at still higher embedding dimensions (i.e. three and above) the attracting region may be more clearly seen.

10. The author states [Page 17, Para 3]: "Another type of suspect results, which we meet in couples of items #6 and #7, and #13 and #14 of Table 1, is the fact that runoff appears to have an attractor with dimension lower than that of rainfall at the same area and timescale. As explained above, it is difficult to imagine how runoff (hydrologic system output) could have dimension smaller than rainfall (hydrologic system input)."

    As explained above, interpretations regarding the variability of hydrological processes should be made only based on the (number of) direct and dominant variables or mechanisms influencing the processes, rather than looking purely at an input-output point of view. The question raised by the author on the studies that have reported lower attractor dimensions for runoff than for rainfall and the argument made to support the question certainly seem to lack an understanding of the underlying mechanisms involved. With respect to the author's question on items #13 and #14 of Table 1 [*Sivakumar et al.*, 2000, 2001a herein], a glance at the monthly runoff and rainfall time series observed at the Göta River basin in Sweden [**Figures 4(a) and 5(a)** attached herein, also presented in the above articles in Figure 1] could easily indicate that the runoff series is less variable than the rainfall series and, therefore, expected to yield a lower attractor dimension. On the other hand, as can be seen from the phase-space plots of the two series [**Figures 4(b) and 5(b)** attached herein, also presented in Figure 2 in *Sivakumar et al.* [2000]], the runoff series yield a well-defined "attracting region" than the rainfall series. If one has to go with the author's earlier argument that the cloud of points in the phase-space is important to decide if a time series is chaotic or stochastic (in terms of variabilities and dimensions), then one has to conclude correctly that the runoff series exhibits less variability than the rainfall series. And the correlation dimension results reported by *Sivakumar et al.* [2000, 2001a herein] only support this. In fact, *Sivakumar et al.* [2000] verified the variability of the rainfall and runoff series also through a nonlinear prediction method, which yielded much higher prediction accuracy for runoff series than for rainfall series (i.e. runoff is less variable than rainfall), providing additional support to the phase-space and correlation dimension results [see Figures 5 and 6 in *Sivakumar et al.* [2000]]. In this regard, the author's argument regarding the variability of rainfall and runoff and, hence, their behavior (chaotic or stochastic) is certainly contradictory to the argument used in the present study through the phase-space plots to defend the stochasticity of the rainfall series. The author should clarify this issue.

11. In addition to the above, the following minor corrections (these are only a few among the many corrections required) should be made to the paper.

    Page 4, Para 2, Line 6: Spelling error

    Page 6, Para 2, Line 1: Basic concepts of chaotic behavior

    Page 7, Para 1, Line 3: system's

    Section 2 is too long and most of the information is well known

    Page 14, Para 1, Line 3: Graf von Hardenberg et al.

    Page 14, Last Para, Line 3: The word "record" is redundant here

    Page 14, Last Para, Line 6: reconstructed

CLOSING REMARKS:

1. The paper does not serve its intended purpose that hydrological processes are not chaotic but only stochastic, rather it only tries to question the reliability of the correlation dimension method.

2. The question of whether hydrological processes are chaotic or stochastic is a much larger problem than the author tries to answer in this paper. As explained above, the methods employed and the data sets analyzed are

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certainly not sufficient to answer such a question. Therefore, additional methods should be employed and more data sets should be analyzed to verify the present results.

3. The author questions the results reported by past studies based on the limitations of the correlation dimension method, in particular based on the scaling regions and minimum data size used in those studies. By analyzing only a random synthetic series, rather than both random and chaotic, the author fails to verify the claims and interpretations made. As discussed above, there are errors in the author's approach to the selection of the scaling region, in particular due to the presence of noise in the data sets analyzed. The author should clarify this issue.

4. In an attempt to question the past studies reporting the presence of chaos in hydrological processes, the author makes several arguments that defy both common sense and proven results, well known to hydrologists. For instance, the author argues that runoff process at a location is more variable than rainfall process at the same location (at the same timescale) since rainfall is the input and runoff is the output. Such an argument is absolutely wrong in most cases. Specific examples, in response to the author's question, was presented above, with regards to time series and phase-space plots (and also prediction results). Similarly, the author's argument that rainfall process at coarser time scales has equal or high variability than that at finer timescales is also wrong in most cases. Rainfall at coarser scales is less variable than that at finer scales, e.g. annual rainfall is often less variable than daily rainfall. This has been proven time and again and very well known. The author can easily verify these by employing none other than a stochastic approach.

5. In my opinion, the author, while raising skepticism on the studies reporting the presence of chaos in hydrological processes, fails to present a balanced and unbiased approach to the problem. An example for this is the author's failure to present the analysis of a synthetic chaotic series. In fact, as explained above, the analysis of a synthetic chaotic series could answer a number of questions raised and the (mis)interpretations provided by the author.

6. Finally, the purpose of characterizing a system, whether low-dimensional chaotic or high-dimensional stochastic, is to identify the appropriate mathematical model for making predictions. Since the author argues that hydrological processes are not chaotic but only stochastic, it is absolutely necessary to verify which type of model (chaotic or stochastic) yields better predictions of the system. The author should verify this by employing both stochastic and chaotic models for prediction of hydrological series considered in the present study. Since past studies have reported near-accurate predictions for runoff series, the analysis of such a series is essential to support the author's claims.

In view of the observations made above, I recommend **rejection** of the paper for publication in *Water Resources Research*.

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<u>**P.S.:**</u> I would be glad to furnish some of our papers currently in press or submitted, if requested by the author for reference. *

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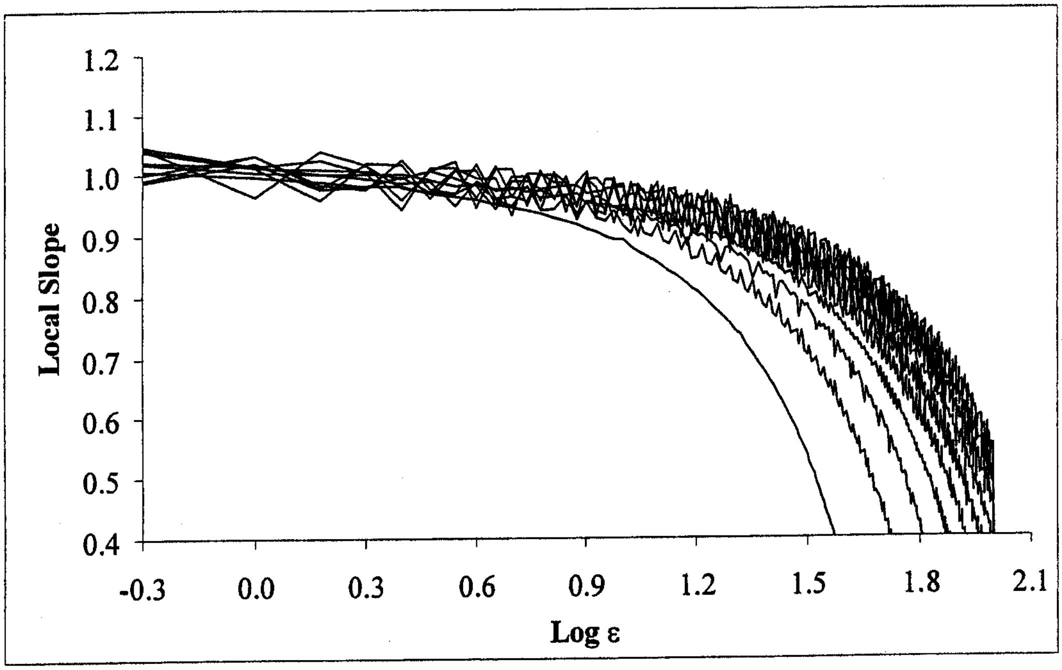

Figure 1. Correlation dimension results for a purely deterministic system

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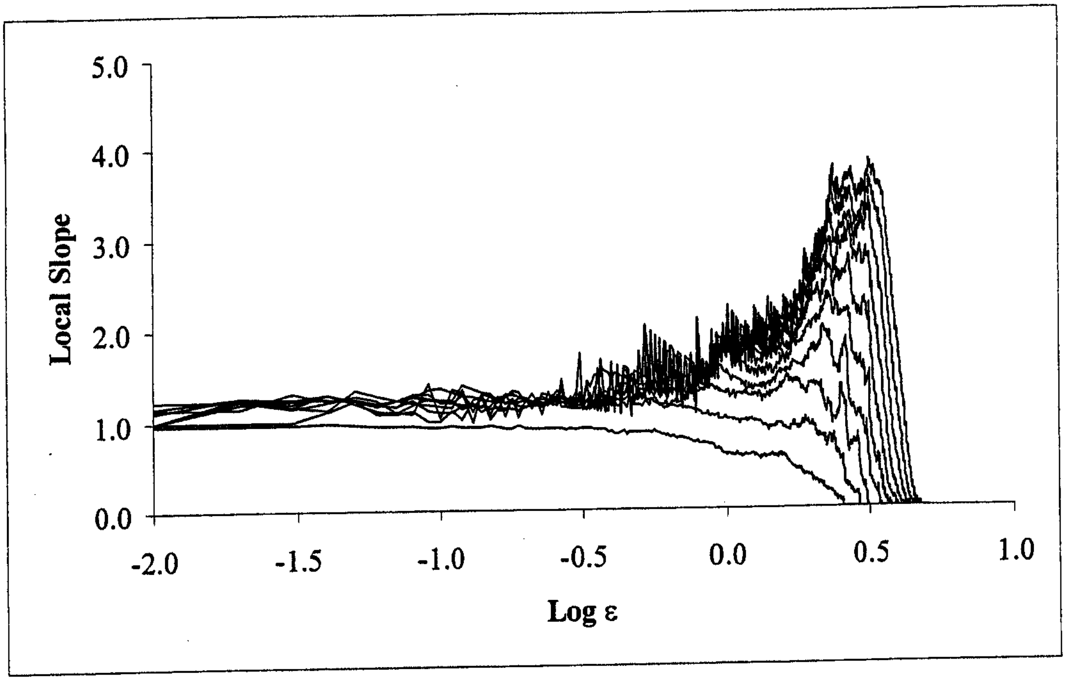

Figure 2(a). Correlation dimension results for a noise-free chaotic series (Henon data)

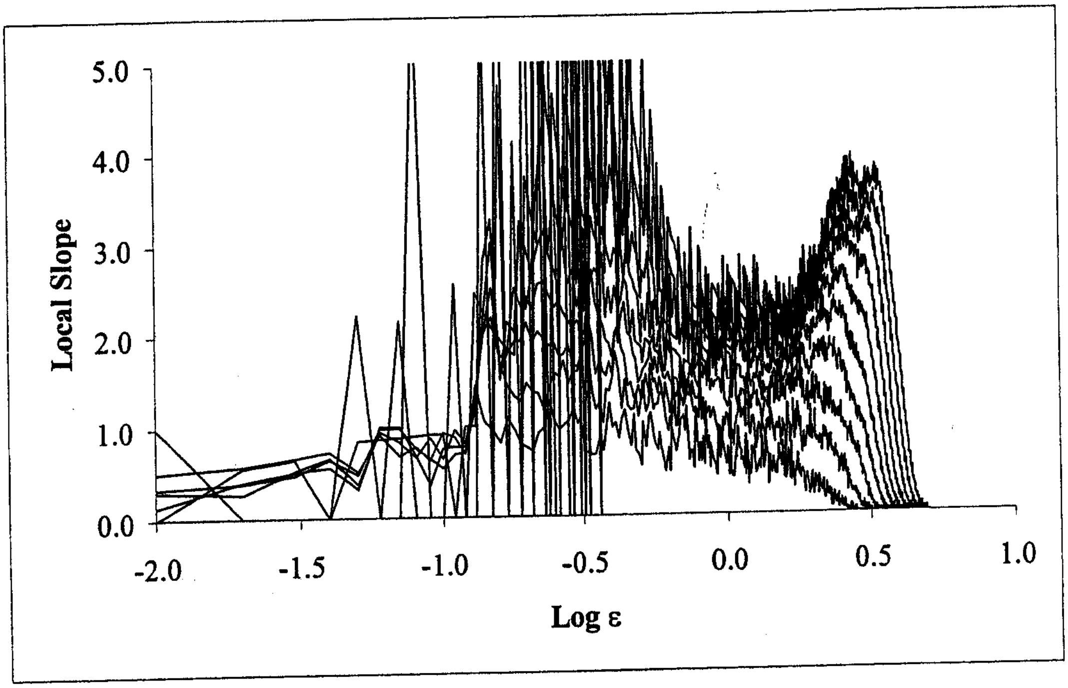

Figure 2(b). Correlation dimension results for a noisy chaotic series (Henon data)

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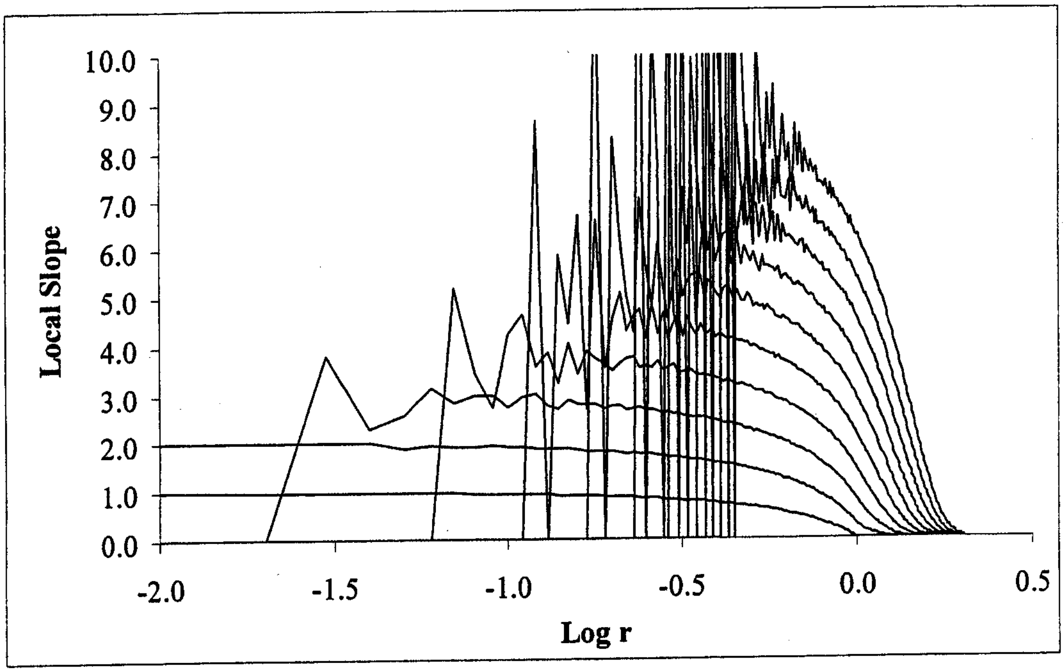

Figure 3. Correlation dimension results for a purely random series

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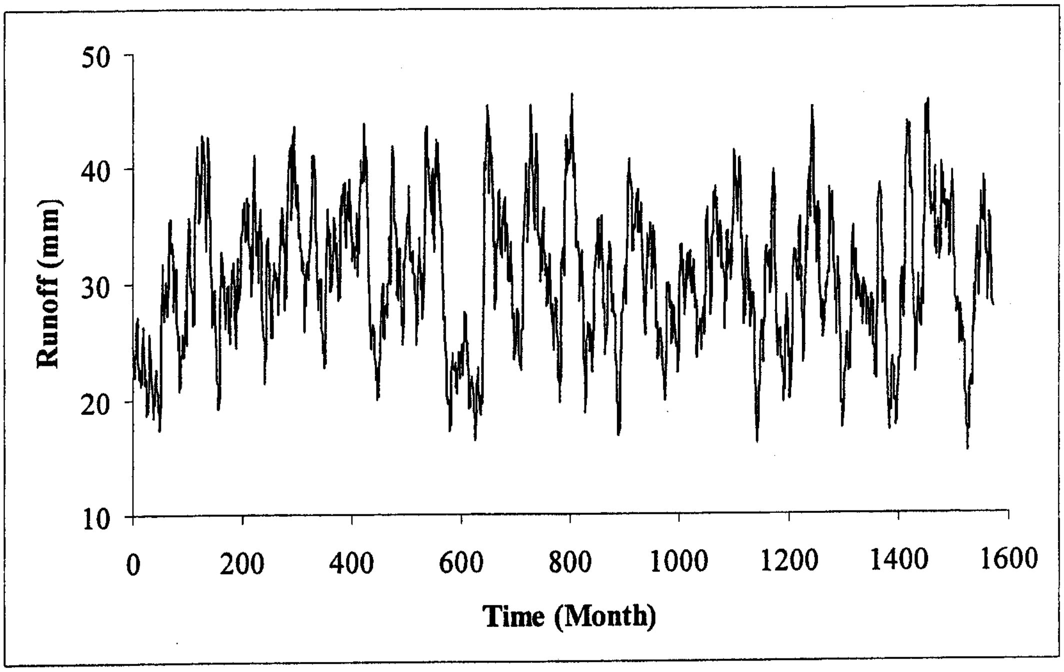

Figure 4(a). Time series plot of monthly runoff series at the Gota River basin, Sweden

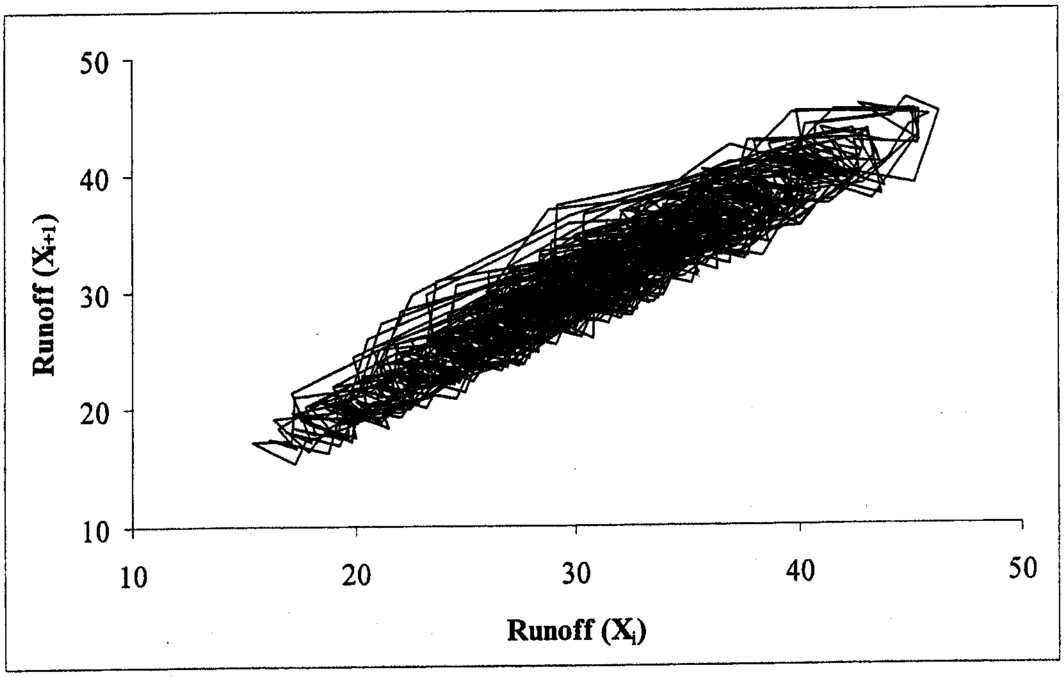

Figure 4(b). Phase-space plot of monthly runoff series at the Gota River basin, Sweden

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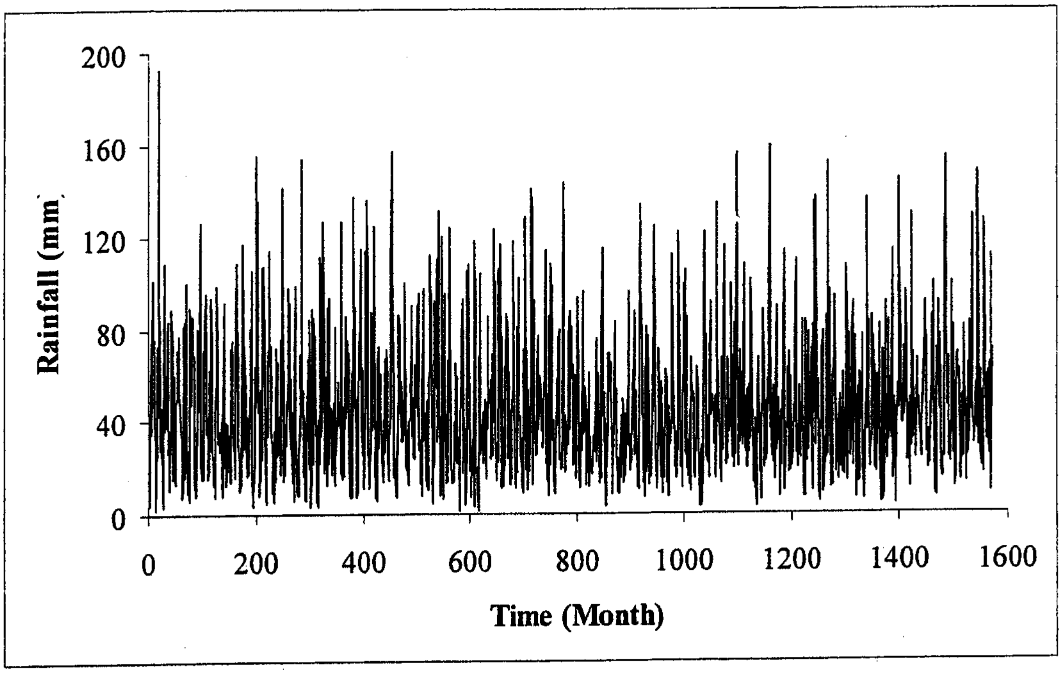

Figure 5(a). Time series plot of monthly rainfall series at the Gota River basin, Sweden

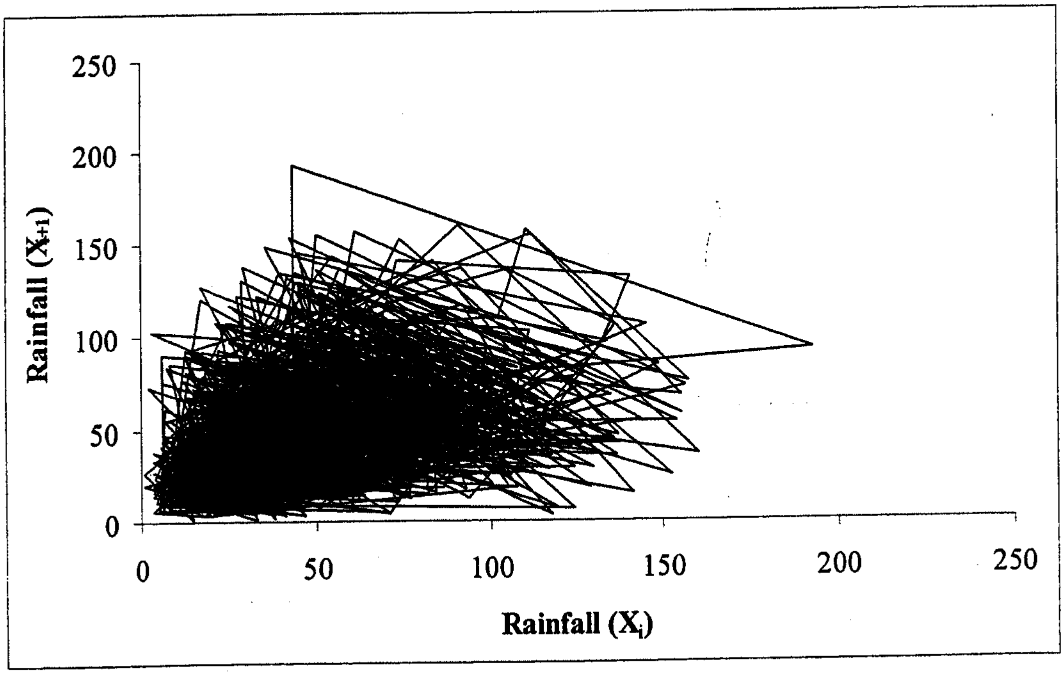

Figure 5(b). Phase-space plot of monthly rainfall series at the Gota River basin, Sweden

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Water Resources Research

Are hydrologic processes chaotic?

You have received the accompanying manuscript with the understanding that you have already agreed to provide a review within four weeks. It is important to many parties that the review process proceed at a timely pace. If your review is late or does not materialize, this will significantly delay the review process. The editorial policy of *WRR* is to seek only two reviews for a manuscript in order to optimize the time and talents of the professional community. However, this policy makes it absolutely essential that reviewers adhere to the agreed upon response time. An efficient review process is in the best interests of the authors, the editors, the administrative staff, the reviewers, and the community *WRR* serves. Your willingness to participate in and contribute to this efficiency is greatly appreciated. Thank you for your responsiveness and service!

Please feel free to complete and submit your review in a form most convenient to you. However, please address the questions set forth herein, when applicable, as these topics must be dealt with in a comprehensive review. If you choose to transmit your review via e-mail, it should be handled in a manner that will maintain confidentiality and be accompanied by a request for confirmation of its receipt.

1. CONTRIBUTIONS AND AUDIENCE
   What are the important contributions of this paper? To show applications of chaos theory and advance

2. TECHNICAL SOUNDNESS
   Is the paper technically sound? See comments
   Are the methods described fully? Yes and no
   Is the mathematical development complete and accurate? Yes

3. PRIOR PUBLICATION
   Has this work, or very similar work, been published elsewhere? No

4. ORGANIZATION AND STYLE
   Is the paper well written and organized? Portions are
   Are all tables and figures necessary? OK
   Can the paper be shortened? OK

5. EVALUATION
   Does this paper make a significant, new contribution in the area of water resources? Not really
   How do you rate the paper? (Outstanding, Very Good, Good, Fair, or Poor)

6. PLEASE PROVIDE DETAILED COMMENTS IN A WRITTEN REPORT