Decision and reviews of first submission (23 February 2015)

25-Aug-2015

Dear Dr Lombardo,

Manuscript title: Hurst-Kolmogorov downscaling model revisited

The above manuscript that you submitted to Hydrological Sciences Journal has been reviewed. The reviewer comments are included at the bottom of this email, along with those of the Associate Editor, Professor Yeboah Gyasi-Agyei, who coordinated the review of your paper.

The reviewers have made a number of suggestions for modification. Since some of the requested revisions are quite major, a re-review of the revised manuscript is recommended, after which a decision will be made whether or not to accept your paper for publication. Therefore, I invite you to respond to the comments and revise your manuscript.

When you revise your manuscript, please highlight the changes you make by using Track-Changes mode or by coloured text. Please enter your responses to the comments made by the reviewers and list any changes you made to the original manuscript in the space provided (Response to the decision letter) or upload this as a Supplementary file. Please be as specific as possible in your response to the reviewer(s). If you disagree with some of the reviewers' comments, please provide an explanation of your disagreement. Although it may contain a reply to this letter, please do not upload your Response as a Cover letter.

To submit the revision, log into https://mc.manuscriptcentral.com/hsj and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions". Under "Actions" click on "Create a Revision". Your manuscript number will be updated to denote a revision. NOTE: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing your submission to avoid confusion.

When preparing your revised paper, please pay particular attention to the IAHS house style given in the detailed IAHS Instructions for Authors, available at http://www.tandfonline.com/action/authorSubmission?journalCode=thsj20&page=instructions (or at the IAHS web site http://iahs.info), particularly with regard to equations, references, tables and figures. Please ensure that you follow the SI naming of units.

I look forward to receiving your revision.

Yours sincerely,

(sent on behalf of)
Professor Mike Acreman
Co-editor, Hydrological Sciences Journal

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Reviewer: 1

Recommendation: Accepted as it stands, apart from Editorial changes

Comments:
The work is an upgrade of an existing model published in the same journal by the same authors. This model evolves upon the existing one by extending it from spatial downscaling to temporal disaggregation, properly considered the intermittent structure of rainfall processes. The derivations involved in this study are correct, the manuscript itself is well written, though the discussed model is somewhat too technical to be easily understood and implemented by readers without solid knowledge in statistics like most hydrologists. I would like to suggest publish as it stands, but will be happy if the authors can summarize the step-by-step implementation procedure of the method in appendix or supplemental file.

Additional Questions:
Please note that the contents of the manuscript remain confidential until published. Reviews are anonymous unless reviewers wish their names to be made known to the author(s). Would you like your name to be revealed to the author(s)? If yes, please enter the information in the comments to author below.: No

How do you rate this paper in absolute terms? Poor to fair, good, very good to excellent.: (2) Good

What is the main contribution of this manuscript?: Valuable contribution to factual information about the hydrology of a region

Reviewer: 2

Recommendation: Subject to major revision. If revised paper is resubmitted, it needs to be reconsidered and re-reviewed

Comments:
The manuscript provides a method for downscaling in time of precipitation series, including intermittence, which is of interest in
general fro hydrologists.

However, the manuscript in the present form requires large (major) revision before it can be published, and I am borderline rejection in fact.

The manuscript provides a "new" model building upon a former one (Lombardo et al 2012), published on HSJ, by improving "consistency" , i.e. mass conservation, and introducing intermittence.

The method is basically a random cascade ("canonical", i.e. with average mass conservation), which the authors modify to enforce mass concervation by a "power adjusting procedure".

However the authors do not explain convincingly

1)Why their proposed method is better than other methods to save mass conservation. No clear comparison in made with the present methods in literature, nor numerical benchmarking against other methods introduced.

2) How their cascade model is different from others. They use lognormal distribution of the weights, which is already largely diffused, and adopted for several purposes in the field of rainfall studies.

3) How their intermittence model is different from previous methods. They use a binomial, multiplicative generator for cascade weight, which is largely diffused ever since the 90s', with no apparent improvements. Their modified approach using "correlated intermittence" is not clearly explained (e.g. it is not clear how dry spell probability depends upon rainfall rate, absolute or relative, at the father nodes, nor how the presence of a dry spell affects wet spells elsewhere for nodes that are "children" of the same "father").

4) How their method describe rainfall statistics any better than other methods. The authors simply display some synthetic simulation from their approach (random, and correlated intermittence). However, in my opinion they would ,need to i) compare their approach against real rainfall series, and ii) compare statistical fitting to real data from their models, and from other models, including mass conservation, correlation structure, scaling, intermittence properties (e.g. by scaling of the "zero order" moments),etc.... I think without comparison against actual data, all the conjectures here make little sense.

5) How their methods can be applied to real world rainfall downscaling, and synthetic simulation. No mention is done about the crucial item of model's estimation. The more parameters as here, the more complex the estimation procedure, and the uncertainty therein. Which methods may one use to estimate such parameters (scaling of sample moments, empirical correlation coefficients, maximum likelyhood, etc.. ?). Real data have noise, so parameters estimation may be complicated, and requires assessment, and more complex models may not provide better results.

On top of these items, the authors seem not aware of a large deal of litetarure dating back to the last two decades having already tackled the issue of intermittency in random cascade, and using similar methods to here, and also having used random cascade approach (with intermittence) for downscaling of rainfall series (from real data at coarse scale, global circulation models also for hydrological projections), data assimilation in time and space, and even rainfall short term forecast.

Accordingly, the authors should investigate such literature, and benchmark their hypothesis, and findings against such.

Find in the .pdf attached pathwise corrections/suggestions/questions

Additional Questions:
Please note that the contents of the manuscript remain confidential until published. Reviews are anonymous unless reviewers wish their names to be made known to the author(s). Would you like your name to be revealed to the author(s)? If yes, please enter the information in the comments to author below.: No

How do you rate this paper in absolute terms? Poor to fair, good, very good to excellent.: (1) Poor to fair

What is the main contribution of this manuscript?: Original contribution to hydrological theory or methodology

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Associate Editor
Comments visible by authors and editors:

I have received two reviews both indicating that the subject matter is of interest to the hydrology community. One reviewer has provided constructive comments with annotated pdf and I encourage the authors to consider them carefully in a revision for further consideration. Also, authors should note these two particular comments.

- The literature review section should include other current papers on disaggregation (e.g. Gyasi-Agyei papers, and references there in, using the binary (wet/dry) chain model and an auto-correlated jitter model). Most of the problems of the MRC model may be overcome by other methods in the literature.
- It is important that the authors measure the usefulness of the proposed methodology against real data sets.

References

1. Gyasi-Agyei, Y. (2012) Use of observed scaled daily storm profiles in a copula based rainfall disaggregation model, Advances in Water Resources, 45, 26-36.
2. Gyasi-Agyei, Y. (2011) Copula-based daily rainfall disaggregation model, Water Resour. Res., 47, W07535, doi:10.1029/2011WR010519.
3. Gyasi-Agyei, Y., Mahbub, P.B. (2007) A stochastic model for daily rainfall disaggregation into fine time scale for a large region, J. Hydrology, 347, 358-370.
4. Gyasi-Agyei, Y. (2005) Stochastic disaggregation of daily rainfall into one-hour time scale, J. Hydrology, 309, 178-190.

Professor Yeboah Gyasi-Agyei

Hurst-Kolmogorov downscaling model revisited

by F. Lombardo, E. Volpi and D. Koutsoyiannis

Background information for resubmission

The *original version* of this paper was submitted on the 23 February 2015.

The paper was handled by the Associate Editor (AE) Yeboah Gyasi-Agyei and was reviewed by two anonymous Reviewers. The first Reviewer recommended accepting the manuscript as it stood, while the second one recommended major revisions. The paper was returned to the Authors for major revision with some additional comments by the AE.

We addressed the review comments in the revised version we are submitting, which includes some improvements (i.e., two more appendices and several changes to the main text) with respect to the original version. Our responses to the review comments and the explanations about how we addressed them in the revised version are given in the following pages.

The resubmission comprises three files: (1) a *clean copy* of the manuscript, (2) an *annotated copy* of the manuscript with tracked changes (new text is in red characters, while deleted in ~~blue~~ ones), (3) the present *report* with background information and responses to review comments. The line numbers referred to in this report are those of the clean manuscript printed in green—not the ones added automatically by the system.

Thanks to the interesting and constructive review comments, we believe that the revised version is substantially improved.

Sincerely,

Federico Lombardo, Elena Volpi and Demetris Koutsoyiannis

Responses to review comments

Key:

> Review comment.

Response.

Quotation from revised paper.

Reviewer #1

> The work is an upgrade of an existing model published in the same journal by the same authors. This model evolves upon the existing one by extending it from spatial downscaling to temporal disaggregation, properly considered the intermittent structure of rainfall processes. The derivations involved in this study are correct, the manuscript itself is well written, though the discussed model is somewhat too technical to be easily understood and implemented by readers without solid knowledge in statistics like most hydrologists. I would like to suggest publish as it stands, but will

be happy if the authors can summarize the step-by-step implementation procedure of the method in appendix or supplemental file.

We thank very much the Reviewer for the positive assessment of our manuscript and the constructive suggestion, which we have addressed in the revision by providing instructions on the implementation steps of our model in a new appendix (Appendix B, lines 636-673).

Reviewer #2

The manuscript provides a method for downscaling in time of precipitation series, including intermittence, which is of interest in general for hydrologists. However, the manuscript in the present form requires large (major) revision before it can be published, and I am borderline rejection in fact.

The manuscript provides a "new" model building upon a former one (Lombardo et al 2012), published on HSJ, by improving "consistency", i.e. mass conservation, and introducing intermittence. The method is basically a random cascade ("canonical", i.e. with average mass conservation), which the authors modify to enforce mass conservation by a "power adjusting procedure".

However the authors do not explain convincingly:

1) Why their proposed method is better than other methods to save mass conservation. No clear comparison is made with the present methods in literature, nor numerical benchmarking against other methods introduced.

We thank very much the Reviewer for allowing us to clarify some key issues of our paper, which could be misinterpreted reading the unrevised version.

Indeed our model is not a multiplicative random cascade (MRC). We have rephrased Section 2 accordingly, and added the following clarification in the text (lines 232-246):

> In summary, our model assumes lognormal rainfall, and then it is reasonable to use a (scale-dependent) logarithmic transformation of variables (eq. 5) and perform disaggregation of transformed variables in a Gaussian (auxiliary) domain, thus exploiting the desired properties of the normal distribution for disaggregation schemes (Koutsoyiannis, 2003a). Therefore, we do not disaggregate rainfall by a multiplicative random cascade (MRC). Rather, we assume a Hurst-Kolmogorov process in the auxiliary domain whose characteristics are changed (by eq. 5) based on the last disaggregation step of interest. The Hurst-Kolmogorov process is effectively generated using a stepwise disaggregation approach introduced by Koutsoyiannis (2002), which is based on a random cascade structure. Finally, the generated lower-level variables are transformed back (eq. 12) to the original lognormal domain. Our specific transformation enables to preserve the scaling properties of the Hurst-Kolmogorov process also in the target (lognormal) domain; thus, it allows to reproduce the empirically observed characteristics of rainfall time series (e.g. at the daily scale).

Nonetheless, Lombardo et al. (2012) already presented a detailed theoretical and numerical comparison of our model with discrete MRCs. In essence, our model is characterized by a structure equally simple as that of MRC models, but it is based on a different approach (Hurst-Kolmogorov)

and it proves to be stationary. In the revision, we added some references providing further justification for our claims about nonstationarity of MRC models (lines 120-122).

In response to the question on mass conservation, Section 3 was rephrased as follows (lines 267-274):

> The power adjusting procedure is more effective and suitable for our modelling framework than the classical linear and proportional adjusting procedures (see e.g. Grygier and Stedinger 1988, Lane and Frevert 1990). Indeed, a weakness of the former is that it may result in negative values of lower-level variables, but rainfall variables must be positive. Conversely, the proportional procedure always results in positive variables, but it is strictly exact only in some special cases that introduce severe limitations. The power adjusting procedure has no limitations and works in any case, but it does not preserve the additive property at once.

2) How their cascade model is different from others. They use lognormal distribution of the weights, which is already largely diffused, and adopted for several purposes in the field of rainfall studies.

We thank the Reviewer for such an interesting comment. Despite being based on the same marginal distribution of the variables, i.e. lognormal, the structure of our model is substantially different from that of the most widely used disaggregation methods, i.e. MRC models (see the answer to comment 1). Even though the two may look equally simple, we emphasized that our model should not be confused with the classical intermittent lognormal β-model (lines 330-332).

3) How their intermittence model is different from previous methods. They use a binomial, multiplicative generator for cascade weight, which is largely diffused ever since the 90s', with no apparent improvements. Their modified approach using "correlated intermittence" is not clearly explained (e.g. it is not clear how dry spell probability depends upon rainfall rate, absolute or relative, at the father nodes, nor how the presence of a dry spell affects wet spells elsewhere for nodes that are "children" of the same "father").

We are grateful to the Reviewer for this useful comment, because we needed to make this important issue of our paper clearer. Indeed, the analysis of intermittency and the modelling of the occurrence process of rainfall is an open issue in hydrology, which is increasingly attracting the interest of many researchers.

We believe the intermittency model is better discussed now in the revised paper. We modified and expanded Section 4, with an emphasis on the dependence structure of rainfall occurrence models, as follows (lines 365-397):

> Generally, we could classify such models into three types: (i) independence, which includes the Bernoulli case, characterized by one parameter only; (ii) simple dependence, which includes Markov chains characterized by two parameters; (iii) complex dependence, characterized by more than two parameters (Koutsoyiannis 2006). For the sake of numerical investigation, we investigate the first two modelling categories of the occurrence processes:
>
> 1. Purely random model
> 2. Markov chain model
>
> It was recognized in early stages of analysis and modelling attempts that the rainfall occurrences are not independent in time, and the Markov chain model was widely adopted for discrete time representations of this process (Gabriel and Neumann 1962, Haan et al.

1976, Chin 1977). It was later observed, however, that Markov chain models yield unsatisfactory results for rainfall occurrences, despite being much closer to reality than the independence model (De Bruin 1980, Katz and Parlange 1998). Moreover, there exist other types of models intended to simulate more complex dependence structures that are consistent with empirical data, such as positive autocorrelation both on small scales (short-term persistence) and on large scales (long-term persistence) (see e.g. Koutsoyiannis, 2006).
Our main purpose is to generate intermittent rainfall time series at a certain time scale, which are fully consistent with a given coarse-scale total. We focus on a modelling approach of a mixed type with a discrete description of intermittency and a continuous description of rainfall amounts. By eq. (20), we introduce the intermittent character in the (back-transformed) synthetic series at the "basic scale", which is represented by the last disaggregation step. In other words, we assume to model intermittency on a single time scale setting, and then we confine our interest only at the basic scale of disaggregated series.
In summary, we generate both independent and autocorrelated (binary) time series of rainfall occurrences at the basic scale, which are then multiplied by the continuous rainfall depth time series (generated by our disaggregation model) in order to obtain the final intermittent rainfall series. Note that our intermittency model is general and allows using any type of autocorrelation function, and we use the independent and Markovian cases as simple applications of our theoretical framework for Monte Carlo experiments.

Furthermore, to better explain the autocorrelation function of Markovian occurrences, we added a new appendix with the theoretical derivation of our paper eq. (35) (Appendix C, lines 675-697).

4) How their method describe rainfall statistics any better than other methods. The authors simply display some synthetic simulation from their approach (random, and correlated intermittence). However, in my opinion they would, need to i) compare their approach against real rainfall series, and ii) compare statistical fitting to real data from their models, and from other models, including mass conservation, correlation structure, scaling, intermittence properties (e.g. by scaling of the "zero order" moments),etc.... I think without comparison against actual data, all the conjectures here make little sense.

Thanks for the interesting comment. The following clarification has been added in the revision (lines 449-455):

Our main purpose is to provide theoretical insights into modelling rainfall disaggregation in time. Then, we propose and theoretically analyze a model that is capable of describing some relevant statistics of the intermittent rainfall process in closed forms. We combine a continuous-type stochastic process (representing rainfall amounts) characterized by scaling properties with a binary-valued stochastic process (representing rainfall occurrences) that can be characterized by any dependence structure.

Concerning the use of real data, Lombardo et al (2012) already tested the validity of the continuous component of our model against observational data without intermittency.

For intermittent rainfall, in our reply to the previous comment we highlighted that even the Markovian model for occurrences is significantly different from empirical data, despite being much closer to reality than the independence model. Therefore, Koutsoyiannis (2006) proposed a specific model of intermittency (derived in a multiple-scale entropy maximization framework), which determines any conditional or unconditional probability of any sequence of dry and wet intervals at any time scale. The implementation of such an intermittency model in our modelling framework

may be the purpose of a future work together with a comparison with data series of intermittent rainfall. Indeed, although very interesting, this goes beyond the scope of the present work, which is basically theoretical.

In addition, our modelling framework is simple and parsimonious; therefore, we believe our contribution has some value and usefulness for the hydrological community.

5) How their methods can be applied to real world rainfall downscaling, and synthetic simulation. No mention is done about the crucial item of model's estimation. The more parameters as here, the more complex the estimation procedure, and the uncertainty therein. Which methods may one use to estimate such parameters (scaling of sample moments, empirical correlation coefficients, maximum likelyhood, etc.. ?). Real data have noise, so parameters estimation may be complicated, and requires assessment, and more complex models may not provide better results.

We thank the Reviewer for the constructive comment. We added an appendix (Appendix B) where we briefly describe the model parameters as follows (lines 639-654):

Input parameters

- Hurst coefficient H: it is dimensionless in the interval (0, 1);
- Mean μ_0 and variance σ_0^2 of the given rainfall amount $Z_{1,0}$ to be disaggregated in time;
- Last disaggregation step k: it is assumed that the desired length of the synthetic series to be generated is 2^k, where k is a positive integer;
- Probability dry $p_{0,k}$: probability that a certain time interval is dry after k disaggregation steps;
- Lag-one autocorrelation coefficient $\rho_{Y,k}(1)$: it completely determines the dependence structure of dry intervals after k disaggregation steps, in case of Markovian intermittency.

Estimating such parameters from rainfall data series is relatively straightforward (see also Koutsoyiannis 2003b). In addition, it should be emphasized that our model fitting does not require the use of statistical moments of order higher than two, which are difficult to be reliably estimated from data (Lombardo et al. 2014).

On top of these items, the authors seem not aware of a large deal of literature dating back to the last two decades having already tackled the issue of intermittency in random cascade, and using similar methods to here, and also having used random cascade approach (with intermittence) for downscaling of rainfall series (from real data at coarse scale, global circulation models also for hydrological projections), data assimilation in time and space, and even rainfall short term forecast.

Accordingly, the authors should investigate such literature, and benchmark their hypothesis, and findings against such.

We added several references throughout the revised manuscript following the suggestions of the Reviewer (see also our replies to previous comments).

Find in the .pdf attached pathwise corrections/suggestions/questions

We addressed most of the corrections/suggestions/questions of the Reviewer both in the revised manuscript and in the replies to the previous points.

Associate Editor

Comments visible by authors and editors:

I have received two reviews both indicating that the subject matter is of interest to the hydrology community. One reviewer has provided constructive comments with annotated pdf and I encourage the authors to consider them carefully in a revision for further consideration. Also, authors should note these two particular comments.

- The literature review section should include other current papers on disaggregation (e.g. Gyasi-Agyei papers, and references there in, using the binary (wet/dry) chain model and an auto-correlated jitter model). Most of the problems of the MRC model may be overcome by other methods in the literature.

References

1. Gyasi-Agyei, Y. (2012) Use of observed scaled daily storm profiles in a copula based rainfall disaggregation model, Advances in Water Resources, 45, 26-36.

2. Gyasi-Agyei, Y. (2011) Copula-based daily rainfall disaggregation model, Water Resour. Res., 47, W07535, doi:10.1029/2011WR010519.

3. Gyasi-Agyei, Y., Mahbub, P.B. (2007) A stochastic model for daily rainfall disaggregation into fine time scale for a large region, J. Hydrology, 347, 358-370.

4. Gyasi-Agyei, Y. (2005) Stochastic disaggregation of daily rainfall into one-hour time scale, J. Hydrology, 309, 178-190.

We thank very much the Associate Editor (AE) for calling our attention on his interesting papers dealing with rainfall disaggregation. The following text has been added to the revised manuscript (lines 133-139):

> Most of the problems of MRC models reported above might be overcome by other disaggregation methods in the literature (see e.g. Gyasi-Agyei 2011, 2012, Pui et al. 2012). However, MRC models gain their popularity due to their ease of use and understanding.
> We propose a model characterized by a structure equally simple as that of MRC models, but it is based on a different approach (Hurst-Kolmogorov) and it proves to be stationary.

Furthermore, we have inserted several additional references in the revision in order to improve the literature review.

- It is important that the authors measure the usefulness of the proposed methodology against real data sets.

Thanks, indeed. Please, refer to our reply to comment 4) by Reviewer #2 for a detailed answer to this comment.

Decision and reviews of second submission (11 December 2015)

29-Feb-2016

Dear Dr Lombardo,

Manuscript title: Hurst-Kolmogorov downscaling model revisited

The above revised manuscript that you submitted to Hydrological Sciences Journal has been further reviewed. Comments from the Associate Editor, Professor Yeboah Gyasi-Agyei, are provided below.

There remain a substantial number of issues, some of which are quite major. A further re-review of the revised manuscript is recommended, after which a decision will be made whether or not to accept your paper for publication. Therefore, I invite you to respond to the comments and revise your manuscript.

When you revise your manuscript, please highlight the changes you make by using Track-Changes mode or by coloured text. Please enter your responses to the comments made by the reviewers and list any changes you made to the original manuscript in the space provided (Response to the decision letter) or upload this as a Supplementary file. Please be as specific as possible in your response to the reviewer(s). If you disagree with some of the reviewers' comments, please provide an explanation of your disagreement. Although it may contain a reply to this letter, please do not upload your Response as a Cover letter.

To submit the revision, log into https://mc.manuscriptcentral.com/hsj and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with Decisions". Under "Actions" click on "Create a Revision". Your manuscript number will be amended to denote a revision. NOTE: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing your submission to avoid confusion.

When preparing your revised paper, please pay particular attention to the IAHS house style given in the detailed IAHS Instructions for Authors, available at http://www.tandfonline.com/action/authorSubmission?journalCode=thsj20&page=instructions (or at the IAHS web site http://iahs.info), particularly with regard to equations, references, tables and figures. Please ensure that you follow the SI naming of units.

It would greatly help our publishing plans if your revised manuscript can be uploaded within two months.

I look forward to receiving your revision.

Yours sincerely,

(sent on behalf of)
Professor Mike Acreman

Co-editor, Hydrological Sciences Journal

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Associate Editor
Comments visible by authors and editors:

The authors have addressed some of the reviewers' comments but there remain some important issues that have not been addressed. Reviewer 2 who provided critical comments was unavailable for further review. However, my comments below suffice the re-review.

The usefulness of the proposed model is not clearly articulated. Is the model intended to disaggregate point daily rainfall to hourly (sub-hourly) timescales? How does it measure up with existing disaggregation models in the literature?

Demonstration of the applicability of the proposed model to real/historical data has not been addressed. A test case of real data set to demonstrate the reproduction of the first and second order statistics, and intermittency properties, including dry probability, mass conservation, correlation structure, scaling, etc at several aggregation levels/timescales is paramount.

In essence, Reviewer 2 comment (4), and partly comment (5), has not been adequately addressed and should be revisited. A concrete real data example with clear parameter estimation should be provided.

The authors have indicated the inadequacy of purely random and Markov chain (first order) models for the intermittency, yet they did not evaluate the more complex dependence structures that are consistent with empirical data. Reproduction of the intermittency properties of real data by the presented model is doubtful.

Review at L133-L136 needs to be expanded to highlight the differences in the modelling philosophy compared with the presented approach.

Figures 3 and 4 are not necessary, or poorly presented scale-wise. Zoom in to better reflect the differences or remove all together and replace with concrete real data examples.

Professor Yeboah Gyasi-Agyei

Temporal disaggregation of intermittent rainfall

by F. Lombardo, E. Volpi, D. Koutsoyiannis, F. Serinaldi

Background information

The present manuscript is the resubmission of the paper HSJ-2015-0071 entitled "Hurst-Kolmogorov downscaling model revisited" by F. Lombardo, E. Volpi and D. Koutsoyiannis.

The previous version of the manuscript was handled by the Associate Editor (AE) Yeboah Gyasi-Agyei who returned the manuscript to the Authors for major revision twice.

This letter comprises a point-by-point reply to the AE's comments (the only second round comments we received) to the previous version for the sake of clarification.

Thanks to the constructive review comments, we believe that the revised version is substantially improved.

Sincerely,

Federico Lombardo, Elena Volpi, Demetris Koutsoyiannis and Francesco Serinaldi

Responses to review comments

Key:

> Review comment.

Response.

Associate Editor

> The authors have addressed some of the reviewers' comments but there remain some important issues that have not been addressed. Reviewer 2 who provided critical comments was unavailable for further review. However, my comments below suffice the re-review.
> The usefulness of the proposed model is not clearly articulated. Is the model intended to disaggregate point daily rainfall to hourly (sub-hourly) timescales? How does it measure up with existing disaggregation models in the literature?

Thank you very much for taking the time to review our manuscript. We propose a temporal disaggregation model for at-site rainfall accounting for intermittency (we changed the paper title accordingly to better convey this message). Our model is parsimonious and enables the analytical formulation of the first- and second-order statistics of the intermittent rainfall process. In particular, we derive the general formulation of the autocorrelation function (ACF) for a mixed-type stochastic process (paper eq. 31) in terms of the ACFs of both the continuous and the discrete components. When deriving eq. (31), note that we have not made any assumption about the dependence structure or the marginal probability of the process; the only assumption is that the process is stationary. Therefore, eq. (31) can be used also when simulating mixed-type processes other than rainfall from the real world. Furthermore, our model is not built ad hoc for specific timescales. Due to the seasonality of the rainfall process, the aggregation scale of higher-level

variables should preferably not exceed the monthly timescale. Concerning existing disaggregation models in the literature, most of such modelling schemes are (often overparameterized) ad hoc techniques rather than consistent generalised methods. The present manuscript reports some progress in this respect.

Demonstration of the applicability of the proposed model to real/historical data has not been addressed. A test case of real data set to demonstrate the reproduction of the first and second order statistics, and intermittency properties, including dry probability, mass conservation, correlation structure, scaling, etc at several aggregation levels/timescales is paramount. In essence, Reviewer 2 comment (4), and partly comment (5), has not been adequately addressed and should be revisited. A concrete real data example with clear parameter estimation should be provided.

We are grateful to the AE for this useful comment, which we addressed by inserting an entirely new section entitled "Application to observational data" in the revised manuscript. Comparisons between model simulations and intermittent rainfall time series from the real world show extremely encouraging results with a very parsimonious modelling framework (just four parameters). Note that we confine our interest to the basic scale of disaggregated series, because we assume to model the intermittent rainfall on a single timescale setting.

The authors have indicated the inadequacy of purely random and Markov chain (first order) models for the intermittency, yet they did not evaluate the more complex dependence structures that are consistent with empirical data. Reproduction of the intermittency properties of real data by the presented model is doubtful.

As stated throughout the revised paper and in our reply to the first AE's comment above, our modelling framework for intermittency is fully general and allows for use of any arbitrary dependence structure. In our comparison with real data, we use indeed a simple ACF formulation with power-law decay and only one parameter, which fits very well the empirical ACF (see paper Figs. 8 and 9).

Review at L133-L136 needs to be expanded to highlight the differences in the modelling philosophy compared with the presented approach. Figures 3 and 4 are not necessary, or poorly presented scale-wise. Zoom in to better reflect the differences or remove all together and replace with concrete real data examples.

The purpose of this paper is neither to review extensively the state of the art of the research related to rainfall disaggregation in time (in the revision we improved the literature review anyway), nor to compare different modelling philosophies. As stated above, we aim to propose a consistent generalised method for rainfall disaggregation in a stationary setting, with an emphasis on minimal parameterization. Furthermore, it is our opinion that the paper Figs. 3 and 4 are necessary in order to show that the ensemble statistical behaviour of the model output is consistent with the theory, which is a fundamental issue for stationary models.

Decision and reviews of third submission (9 October 2016)

30-Jan-2017

Dear Dr Lombardo,

Manuscript title: Temporal disaggregation of intermittent rainfall

Your manuscript which you submitted to Hydrological Sciences Journal has been reviewed. The reviewers' comments are included at the bottom of this email, along with those of the Associate Editor, Dr Thomas Kjeldsen, who coordinated the review of your paper.

I regret to inform you that the reviewers have raised serious concerns, and therefore your paper cannot be accepted for publication in Hydrological Sciences Journal.

However, since the reviewers / Associate Editor do/es find some merit in the paper, I would be willing to reconsider it if you are prepared to undertake a major overhaul, addressing the referees' concerns so as to substantially upgrade the present manuscript, and re-submit. Please note that re-submitting your manuscript does not guarantee eventual acceptance, and that your re-submission will be subject to further review before a decision is made.

When preparing your revised paper, please pay particular attention to the IAHS house style given in the detailed IAHS Instructions for Authors, available at http://www.tandfonline.com/action/authorSubmission?journalCode=thsj20&page=instructions (or at the IAHS web site http://iahs.info), particularly with regard to equations, references, tables and figures. Please ensure that you follow the SI naming of units: http://www.iahs.info/Publications-News/Other-publications/Guidelines-for-the-use-of-units-symbols-and-equations-in-hydrology.do

Once you have upgraded your manuscript, go to https://mc.manuscriptcentral.com/hsj and login to your Author Centre. Click on "Manuscripts with Decisions" and then click on "Create a Resubmission" located next to the manuscript number. Then, follow the steps for re-submitting your new manuscript. In response to the question "Has the manuscript been submitted to this journal before?", you should answer "yes" and give the MS ID above. The new MS will be given a new ID, but will be linked to this one.

If you decide to revise and re-submit your manuscript, please save and upload, in addition to the new version, a file in which changes have been highlighted using Track-Changes mode or coloured text, as it is likely the Ms will be sent to the same reviewers. Please also provide a response to the comments made by the reviewers and list any changes you made to the original manuscript; you should upload this and the track changes file as Supplementary files. Although it may contain a reply to this letter, please do not upload your Response as a Cover letter.

As you have been asked to carry out complete revision, the re-submission deadline will be extended to six months.

Yours sincerely,

(sent on behalf of)
Dr Ross Woods
Co-editor, Hydrological Sciences Journal

Reviewer: 1

Recommendation: Accepted after minor revision

Comments:
This is a solid paper with development of new methodology. The major improvement required is to further justify the advantage of the proposed method by comparing to other alternative ones.

Additional Questions:
Please note that the contents of the manuscript remain confidential until published. Reviews are anonymous unless reviewers wish their names to be made known to the author(s). Would you like your name to be revealed to the author(s)? If yes, please enter the information in the comments to author below.: No

How do you rate this paper in absolute terms? Poor to fair, good, very good to excellent.: (2) Good

What is the main contribution of this manuscript?: Original contribution to hydrological theory or methodology

Reviewer: 2

Recommendation: Subject to major revision. If revised paper is resubmitted, it needs to be reconsidered and re-reviewed

Comments:
A strong aspect of the proposed method is that (1) it can deal with non-stationary data such as rainfall and (2) the statement on L511-514 of the MS. Perhaps the authors can strengthen their contribution by pulling this upfront and clearly articulating their contribution on the background of the state-of-the-art in rainfall disaggregation.

A couple of overall issues are outlined below:

- Given that the authors claim the proposed method is general and appropriate to the disaggregation of other variables and time steps, it would be relevant to include examples of its universal applicability beyond the rainfall time series from a single station and at different time steps.

- Numerous times the authors refer to their previous publication, Lombardo et al 2012, and it is not clearly articulated how the present MS differs from their earlier work and how it makes an original contribution.

- A comment from a previous reviewer appears to remain unaddressed: in the authors' Responses to review comments, see the last comment and response. It seems only appropriate to review the state-of-the-art in temporal rainfall disaggregation, especially given the journal's readership and the numerous methods that have been applied. If the method proposed here is advantageous to MCR stationary techniques, the authors ought to demonstrate this.

Some detailed suggestions for amendments are included below:

* L46 - temporal resolution or temporal resolution and extent? please clarify
* L60-63 - sentence does not read well, consider revising for clarity
* L65 - replace 'about some' with e.g. 'of key'
* L69 - replace 'Nevertheless' with 'However'
* L72-73 - 'some important properties' is not sufficiently clear, please revise for clarity
* L76 - remove double quotation marks around 'power adjusting procedure'
* L85 - insert comma after 'thus'
* L88 - consider revising the sentence starting with 'Then' or delete altogether
* L89 - replace 'such as' with e.g. 'this', move 'Section 4' to the end of the sentence, and delete 'entire'
* L91 - move 'component' after '(non-zero rainfall)' and replace 'Our' with 'This'
* L94 - revising as follows might help with clarity: 'In the case study presented in Section 5, we test...'
* L104 - delete 'very'
* L116 - replace 'Unfortunately' with 'However'
* L119 - delete 'simply'
* L129-130 - delete 'by several researchers and practitioners'
* L143 - consider moving the sentence starting on this line to where MCR models are first introduced
* L145 - delete the sentence starting on this line as it repeats with a previous one
* L148 - delete 'it is' and replace 'and it' with 'which'
* L149 - delete 'Indeed, we emphasize that'
* L155-156 - revise for clarity as follows after 'model', e.g. 'and provide a step-by-step implementation procedure in Appendix B.'
* L157 - avoid using 'etc.'
* L180-184 - long sentence, consider revising for concision
* L213 - what are the authors considering a 'good solution'? justify
* L246-247 - this was made clear already in a previous section
* L270 - consider being specific about the 'certain statistics'
* L301-302 - it is not clear what the authors mean by 'is a matter of common experience', revise for clarity
* L323 - consider replacing 'are allowed' with 'can' or omit altogether
* L328 - delete comma after 'Whereas'

Additional Questions:
Please note that the contents of the manuscript remain confidential until published. Reviews are anonymous unless reviewers wish their names to be made known to the author(s). Would you like your name to be revealed to the author(s)? If yes, please enter the information in the comments to author below.: No

How do you rate this paper in absolute terms? Poor to fair, good, very good to excellent.: (2) Good

What is the main contribution of this manuscript?: Original contribution to hydrological theory or methodology

Dr Thomas Kjeldsen

Associate Editor
Comments visible by authors and editors:
The manuscript has been reviewed by two external experts. In general, there is a consensus that the manuscript addresses an important issue. However, concerns are raised as to the presentation of the innovative aspects of the method, especially given the heavy reliance on references back to a previous paper by the team from 2012. Having read the manuscript, I agree that it is indeed difficult to clearly demarcate the lines between innovations in this paper and previous papers. Also, I found the introduction to the modelling concepts in Section 2 and subsequent discussion almost impenetrable. For example, a more helpful definition of all the subscripts in Eq. (1) and how they actually relate to rainfall data would have been useful. If the final decision is major review or reject+resubmit, then the authors need to consider the comments by thttps://mc.manuscriptcentral.com/images/en_US/buttons/submit.gifhe reviewers carefully, and also make sure to be more helpful to potential readers without the same level of insight into the problem as they themselves have acquired; I am sure this would help to improve potential future impact of the paper if published in Hydrological Sciences Journal.