Interactive comment on "Stochastic simulation of streamflow and spatial extremes: a continuous, wavelet-based approach" by Manuela I. Brunner and Eric Gilleland

Ioannis Tsoukalas (e-mail: itsoukal@mail.ntua.gr)

Introduction

This is an interesting work aiming to provide a method (as well as implement it in an R package; function called PRSim.wave within PRsim R package) for the simulation of multivariate hydrological processes (for now, focusing on streamflow) - which according to the results presented by the Authors has a good potential, requiring yet some improvements.

In general, I find the manuscript well-organized and straightforward to understand, yet in my view, there are several points that require the Authors attention.

All comments and suggestions are meant to be constructive and aim to improve the quality of the manuscript, as well as the findings obtained.

Comments

L6-7. I suggest to write: "To do so, we propose the stochastic simulation approach called Phase Randomization Simulation using wavelets (here after called *PRSim.wave*) which combines… ".

L11. To avoid confusing the reader, and provided that a few lines above it is mentioned that "We apply and evaluate *PRSim.wave* on a large set of 671 catchments in the contiguous United States.", I suggest to write: "…at multiple sites (up to four)…"

L34. I wonder what are the "potential non-stationarities" mentioned by the Authors? Are you refereeing to the typical cyclostationary behavior exhibited by hydrological processes?

Given the opportunity, and as a side note, I would like highlight that stationarity is an essential tool for inferencing from data (e.g., model fitting). Stationarity should not be seen as a shortcoming, nor dead. Non-stationarity implies non-ergodicity, which in turn makes inference from observed data impossible, unless of course the deterministic dynamics of the process are known; which in my understanding, is never the case in hydrological sciences. On this topic, I recommend the recent work of Serinaldi et al. (2018), with emphasis on section 4.2, as well as the works of Koutsoyiannis and Montanari, (2007), (2015), Lins and Cohn (2011), Matalas (Matalas, 2012), and Montanari and Koutsoyiannis (2014), that argue in favor of stationarity. See also the very interesting, note of Harry F. Lins[1]. As Harry F. Lins concludes his note:

Stationarity ≠ static

Non-stationarity ≠ change (or trend)

L36. I am not sure what is the meaning of "continuous" here? Can you please elaborate/specify? Also, some references would be useful.

[1] http://www.wmo.int/pages/prog/hwrp/chy/chy14/documents/ms/Stationarity_and_Nonstationarity.pdf

L40. Although I understand its rationale, I am not a big fan of the now-typical classification of stochastic models on "parametric and non-parametric", since in my view, there is no model without parameters. Typically, the literature uses the term "non-parametric" to refer to approaches that use some kind of resampling mechanism (e.g., k-nn algorithm) and/or "non-parametric" distribution functions (e.g., kernel-based approximation of the density function) to generate synthetic data. But, one should take a moment and think, are these really non-parametric? Isn't the *k* (i.e., the number of nearest neighbors) in k-nn algorithm a parameter? Isn't the choice of the kernel smoothing function (e.g., normal, epanechnikov, box, triangle) a parameter? Isn't the bandwidth of the kernel a parameter (also called the smoothing parameter)? Aren't the data *per se* used as parameters (e.g., when a non-parametric method relies on the sampling from the empirical CDF or kernel-based CDF. What if we have new data or alter a few? Does the model change?)? Having said these, I suggest to the Authors to reconsider using the employed classification, as well as review the recent literature (e.g., Serinaldi and Kilsby, 2014; Tsoukalas et al., 2019) for finding an alternative classification.

L41-43. The Authors write: "…and temporal disaggregation models such as fractional Gaussian noise models (Mandelbrot, 1965), fast fractional Gaussian noise models (Mandelbrot, 1971), broken line models (Mejia et al., 1972), and fractional autoregressive integrated moving average models (Hosking, 1984).".

To clarify, these are not disaggregation models, but models able to simulate processes exhibiting long-range dependence (particularly, designed to simulate fractional Gaussian noise (fGn) processes or else, processes exhibiting Hurst behavior). See a similar discussion in the introduction section of Tsoukalas et al. (2018b).

L44-45. The Authors write: "Nonparametric models are based on disaggregation and resample from the data with perturbations and include…".

I think that this statement can be confusing, and needs some refinement. "Non-parametric" models are not necessarily based on the notion of disaggregation. Of course, the literature offers "non-parametric" disaggregation methods (e.g., Lee et al., 2010; Tarboton et al., 1998), but this does not makes all "non-parametric" methods, methods that "based on disaggregation and resample". Further details on disaggregation methods can be found on the seminal works of Valencia and Schakke (1973), and Mejia and Rousselle (1976), as well in the work of Koutsoyiannis (2001) who provide a detailed overview on the subject. For a more recent overview and discussion on the topic of disaggregation and multi-temporal simulation see also work of Tsoukalas et al. (2019).

L48-49. The Authors write: "…but none of these time domain methods can capture the spectral properties of the observed time series (Erkyihun et al., 2017).".

In my view this statement is a bit confusing, requiring the Authors attention, for two reasons.

> 1) A timeseries (i.e., a sequence of observations ordered in time) does not has spectral properties, it exhibits some form of dependence structure (which can be quantified using statistics/stochastics, e.g., through the empirical correlation coefficients and the empirical spectrum). What has spectral properties is the stochastic process that it is assumed that generated the observed timeseries.
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> 2) Having said the above, and since correlation and spectrum are interrelated quantities, if a model is capable of reproducing the process's correlation structure it also reproduces its

spectrum (and vice versa). For further details and references, see my previous comment (Tsoukalas, 2019) on a recent work co-authored by the first Author of this work.

L49-50. In my view the sentence "Furthermore, these time-domain models struggle with the representation of spatial dependence" is a bit "strict", since as far as I see it, there is no struggle, but many research efforts (past, and new).

The stochastic hydrology literature offers several "time-domain" models that can simulate parsimoniously multivariate processes, including both stationary and cyclostastionary processes (e.g., Efstratiadis et al., 2014; Koutsoyiannis, 2001, 2000), reproducing also the moments of the observed processes (typically up to third order). Further to these models/methods, more recent approaches allows the parsimonious simulation of multivariate stationary and cyclostationary processes with any marginal distribution and correlation structure (Kossieris et al., 2019; Tsoukalas, 2018; Tsoukalas et al., 2018a, 2018b), also in a multi-scale context (Tsoukalas et al., 2019). Apart from the last work, for another multi-scale and multivariate simulation study involving daily rainfall at 4 sites the Authors are referred to Appendix D, section D.2, of Tsoukalas (2018). Therefore, taking into consideration the above-mentioned works I would suggest the Authors to revise the sentence accordingly, as well as provide some references.

L54-55. The Authors write: "In contrast to time-domain models, frequency-domain models allow for the simulation of surrogate data with the same Fourier spectra as the raw data".

This can be also true for time-domain methods (but not a good modelling practice in either of the two cases; see below). For instance, if one employs an AR or MA model of high order can simulate a realization of a process exhibiting exactly the empirical autocorrelation coefficients up to the order dictated by the model. However, this is not a good modeling practice since it is well-known that the empirical estimators of auto- (and cross-) correlation coefficients are (downward) biased (Beran, 1994; Koutsoyiannis, 2003, 2000), especially in the case of long-range dependence, short samples, and large lags. See also Matalas (1967 p. 945) who remark that:

"*Parameters that are determined in terms of high order moments of large time lags are subject to large standard errors and consequently large operational biases. Operational biases can never be eliminated, but they can be minimized by the use of regionalization to account for the temporal and spatial variations inherent in the historic sequences…"*.

Of course, the same applies for the empirical estimators of spectrum (see the comparative work of (Dimitriadis and Koutsoyiannis (2015)). Note that this kind of approaches are not parsimonious (since all the empirical estimates used in model fitting are essentially model parameters). To cope with these, the recent literature (Kossieris et al., 2019; Tsoukalas et al., 2019, 2018b), as well some works already cited in the manuscript (i.e., Papalexiou (2018)), has leaned towards the use of parametric models (e.g., with two or three parameters) to parsimoniously describe the dependence structure of the processes. The Authors are referred to the work of Koutsoyiannis (2000) which in my view popularized that idea in hydrological domain, also introducing a parsimonious two-parameter auto-correlation structure. It is also interesting to note the work of Papalexiou (2018) (already cited in the manuscript), who employed the functional form provided by the survival function of a distribution to define several auto-correlation structures.

L70-72. The Authors write: "In addition, it may help to improve the representation of spatial dependencies because it does not require a transformation to the normal distribution and back to

the original, skewed distribution, which usually weakens spatial correlations (Embrechts et al., 2010)."

First, the comment on "weaken spatial correlations" applies for all "types" of correlations (that emerge from the mapping/transformation from the Gaussian to the actual domain) – not only spatial. Particularly, in the case of stochastic processes, it also applies for the auto-correlation structure of a stationary processes, as well as for the season-to-season correlations of a cyclostationary process (Tsoukalas et al., 2018a, 2017). It also holds for multivariate cases. However, I am afraid that I cannot see the improvement of the representation of spatial dependencies mentioned above by the Authors. The cross-correlations as well as the auto-correlation are still not accurately reproduced (see my comments below on the results/plots). It is my understanding that a previous comment of mine (Tsoukalas, 2019) on a recent work co-authored by the first Author of this work holds also for this method. This is due to the following:

> **L145.** The Authors write: "Derivation of random phases: A random discharge time series (white noise) of the same length as the input series is sampled from a normal distribution with mean 0 and standard deviation 1."
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> **L170-173.** The Authors write: "Transformation to kappa distribution: The simulated values are transformed to the kappa domain using the fitted daily kappa distributions from Step 2. For each day, a random sample is generated from the fitted, daily kappa distribution. The simulated values are replaced by the values generated from the kappa distribution using rank-ordering. This procedure is repeated for each day in the year."

Based on the above the method presented herein depends on an auxiliary Gaussian process and uses the target ICDFs, as well as the rank-correlations to establish the (auto- and cross-) dependence structure. It is reminded that such a procedure will preserve the ranks correlation coefficients (which do not depend on the marginals) but not the Pearson's, (which depends on the marginals; since it involves the cross-product moment of the among the variables). For further details the Authors are referred to the comment mentioned above, as well as in the references therein. It is my understanding that the mechanics of the method that dictate the preservation of ranks is the reason why the auto- (and cross-) correlations are not so well reproduced by the proposed method.

L149 (and elsewhere): The use of Kappa distribution. As mentioned in a previous comment of mine in HESS related with a work co-authored by an Author of this manuscript there are few complications worth considering when using the Kappa distribution. The following comments are excerpted with minimum or no modifications at all from Tsoukalas (Tsoukalas, 2019).

1. Since you are using the Kappa distribution it could be insightful to mention that under certain parameter combinations, this distribution may lead to infinite moments. This can be a delicate issue, since if the fitted distribution exhibits infinite variance then the Pearson's correlation cannot be defined (the denominator contains the variance), and thus the proposed model (as well as many other models) cannot be used. This situation is discussed in section 3.4 of Tsoukalas et al. (2018b; and references therein), where it is advocated (based on empirical, as well as theoretical reasoning) that physical processes are characterized by finite variance (Koutsoyiannis, 2016).
   Particularly, if X is a Kappa-distributed random variable, and $\mu_r = E[X^r]$ denotes the r^{th} raw moment, as discussed in Hosking (1994), and elsewhere, the existence of the r^{th} depends on the values of h and k. Specifically, the moments exist:

$$\text{for all } r \quad \text{if} \quad h \geq 0 \text{ and } k \geq 0$$
$$\text{for } r < -1/hk \quad \text{if} \quad h < 0 \text{ and } k \geq 0, \text{and}$$
$$\text{for } r < -1/k \text{ if } k < 0$$

It is also interesting to mention that Hosking (1994) notes that the first four moments cannot uniquely determine the parameters of the distribution, since some combinations of moments (expressed by skewness and kurtosis coefficients) correspond to different pairs of h and k.

2. How do you handle negative values? As far as I am aware the left (and right) support of Kappa distribution is not necessarily zero (e.g., when $k = 0$ and $h \leq 0$, then the supports of the distribution are, $-\infty < x < \infty$; see Hosking (1994)). In any case, the generation of negative values can be eliminated by using a distribution function defined in the positive real line. Particularly, I would suggest the investigation/use of the Generalized Gamma and Burr type-XII distributions, which are more parsimonious (they entail three parameters; instead of four as in Kappa) and were found adequate for modelling of hydrometeorological variables; particularly rainfall (e.g., Papalexiou and Koutsoyiannis, 2016). Examples of their use within the context of stochastic modelling can be found the work Papalexiou (2018), as well as in Tsoukalas et al. (2019, 2018b) and Tsoukalas (2018).

L158-160. The Authors write: "We fit a separate distribution for each day to take into account seasonal differences in the distribution of daily streamflow values. To do so, we use the daily values in a 30-day window around the day of interest."

Can you please elaborate on this? Just to be sure, for each "site" and for each day of the year a Kappa distribution has been fitted with different parameters? If this is the case, just for the marginal behavior, and for each site you fitted Kappa 365 times, which implies that the model has 365×4 (the number of parameter of Kappa) $= 1460 \times$ the number of sites, parameters (not accounting those for the specification of the auto and cross-dependence structure of the process). If this is the case, I am afraid that this is not a parsimonious model, something that should be clearly stated in the manuscript (also mentioning the total number of its parameters).

Also, I don't think that it is reasonable to assume that days belonging in the same month (e.g., the 19th and the 20th of August) have different marginal distribution (although, I have seen stochastic simulation related works following that approach, I am not aware of any paper supporting this assumption). The standard approach for daily (or finer time) scales is to consider stationarity within the monthly interval (i.e., in the case of daily data consider that all days belonging in the month have the same marginal distribution). An arguably more parsimonious approach, since in this case the total number of parameters for the marginal behavior would be 12×4 (the number of parameter of Kappa) $= 48 \times$ the number of sites. Note that the number of parameters could be further reduced by using, instead of Kappa, alternative 2- or 3-parameter distribution models.

L162-164. The Authors write: "In a few regions with many zero discharge values (e.g. some catchments in the Great Plains) fitting the kappa distribution is not possible and we therefore use the empirical distribution instead."

This is a work-around that could work, but I wonder, why not use an alternative distribution model (e.g., zero-inflated or mixed) that can model simultaneously both the discrete (i.e., probability of no discharge) and continuous part (i.e., distribution of non-zero discharge) of the process? Also, can you

provide an estimate on the number of cases where the empirical distribution is employed instead of Kappa?

L218-219. The Authors write: "the seasonal (3d) and monthly distributions (3e–g) are well captured by the simulations."

This is a confusing description of the plots. The (3e–g) plots show that some seasonal summary statistics are reproduced (i.e., monthly mean, monthly maxima, monthly minima), not the seasonal distributions (to do so you need alternative plots, comparing the empirical distribution of each month with the corresponding theoretical one). Also, I don't understand what plot 3d shows? What does "seasonal statistics" means in the title of this plot? What does the phrase "the seasonal discharge distribution" means at the legend of Figure 3? These are box-plots, and by no means should be used to compare distribution functions (they provide way too few information - about specific quantiles).

L220-222. The Authors write: "the temporal correlation characteristics (4a–c), …. are well captured by the simulations as well."

In my view there is an important difference between the simulated and empirical autocorrelation coefficients. For instance, by eye-balling the median of the orange lines (simulated) for time lag 10 we get a value about equal to 0.5, while the observed one is 0.4. This should be clearly stated in the manuscript. For the readers convenience, I also suggest the inclusion of a line depicting the median of the simulated quantities (orange lines). Providing lines for a low and high quantile (say 0.05 and 0.95) would be nice also. This comment applies for all similar plots throughout this manuscript.

L223-225. The Authors write: "Both high- and low- extremes are realistically modeled as illustrated by the distributions of the above and below threshold events of the four catchments in the Pacific Northwest (Fig. 5)."

To avoid confusion with actual distribution functions, e.g., the Kappa, instead of using the phase "as illustrated by the distributions of the above and below threshold events", I would suggest the use of phrase similar to: "as illustrated box-plots of Fig.5, constructed by the values of the above and below threshold events".

L223-225. Please remind the reader the selected threshold values.

Figure 6 (legend). I assume that you wanted to write: "… (b) for the **three** catchments in the….".

L230-233. The Authors write: "This visual impression of a good performance with respect to the reproduction of spatial correlations in daily discharge data is confirmed by comparing observed and stochastically simulated cross-correlation functions for the catchments in the Pacific Northwest (Fig. 7). Both the shape and magnitude of the cross-correlation functions are well simulated".

I am afraid that this sentence is needs some refinement, since as with the case of auto-correlation coefficients, Fig. 7 shows a significant deviation of the simulated cross-correlation coefficients from the observed ones. This should be reflected in the manuscript. For the readers convenience, I would also suggest the inclusion of a line depicting the median of all simulations (orange lines). Providing lines for a low and high quantile (say 0.05 and 0.95) would be nice also.

Figure 7. There is something wrong with the labeling of the panels (i.e., multiple panels are labeled as ii, iii, iv, while some others are completely unlabeled).

L239. The Authors write: "…but also for extreme values as illustrated by the peak-over-threshold (POT) values for the different stations in the three illustration regions (Fig. 9). These results show that besides regional flood co-occurrences, the temporal clustering behavior of events is also reproduced."

In my view, Figure 9 is not very informative (the y-axis has been omitted intentionally?). Can you please provide an alternative figure, as well as a quantitative metric, quantifying the reproduction of temporal clustering behavior of events?

Discussion section. In my view all the above points should be discussed in this section, highlighting also the limitations of the presented method. Further to these, it should be noted that the proposed model has been tested for multivariate problems involving 4 processes, as well as the method is capable of generating synthetic timeseries with length equal to the observed one (I haven't read how to handle the case where one wants to generate longer timeseries – is it possible to generate synthetic timeseries with length different than the observed one?).

L258-259. The Authors write: "This difference between methods may be related to the fact that the wavelet transform compared to the Fourier transform does not necessitate a transformation to the normal domain, and a back transformation to the domain of the skewed distribution, which has been shown to weaken spatial correlations".

I think that the answer is is much simpler than the one stated above. The former method is simply designed for the simulation of univariate processes, i.e., not to account for the cross-correlations (or cross-spectrum) among processes. I suggest the Authors to consider more carefully the "mechanics" of the aforementioned methods, and revise the sentence accordingly. Also, as mentioned before, the comment on "weaken spatial correlations" applies for all "types" of correlations (that emerge from the mapping/transformation from the Gaussian to the actual domain) – not only spatial. Particularly, in the case of stochastic processes, it also applies for the auto-correlation structure of a stationary processes, as well as for the season-to-season correlations of a cyclostationary process (Tsoukalas et al., 2018a, 2017). It also holds for multivariate cases.

L270-271. The Authors write: "Thanks to a spatio-temporal model based on phase randomization, temporal short- and long range dependencies, non-stationarities, and spatial dependencies are reproduced."

Please consider my comments on the parameterization (i.e., number of parameters), as well as on the performance of the model and revise this sentence accordingly.

A general comment. A final comment regards the title of the manuscript, which is: "Stochastic simulation of streamflow and spatial extremes: a continuous, wavelet-based approach". By reading the paper I see that the Authors pay special focus on the reproduction of extremes, but it is not clear to me why this model is different from any other in that aspect (e.g., see those mentioned above)? What makes this model suitable when aiming to reproduce extremes? Other similarly parameterized models behave differently? If yes, why? I believe that a theoretical justification or even an empirical comparison with alternative model(s) would be particularly useful and an added value for the paper.

Regards,

Ioannis Tsoukalas

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