Responses to Referee #2

Dear Referee #2,

We thank you very much for the professional and constructive comments on our manuscript entitled "Comparative study of flood projections under the climate scenarios: links with sampling schemes, probability distribution models, and return level concepts" (Number: hess-2016-619). The comments are all significant for improving our research and paper. We have carefully followed these comments and suggestions. The point-by-point responses to the comments are shown below. The corresponding revision to the manuscript would be made.

General comment 1:

In this study, the Authors perform quite a standard nonstationary frequency analysis. It can seem surprising to talk about "standard analysis" when dealing with relatively new "nonstationary" fashion, but the main point is that this paper, as a large part of those on this topic, is a simple application of a set of routines already implemented in R packages. Thus, most of the results are quite speculative, as they overlook the theoretical concepts behind statistical tools and some significant papers explaining these issues. Moreover, I regret to say that a very similar version of this paper was already declined by Advances in Water Resources in 2015. In that case, I suggested a major revision, but I see that the Authors did not account for most of my suggestions. Some of them are reported below once again with updates.

Response:

We thank the referee very much for reconsidering our paper. Concerning the above mentioned incident, we feel really sorry to cause the misunderstanding, and now would like to take this opportunity to clarify this. Indeed, we submitted a paper entitled "Comparing Annual Maximum and Peaks over Threshold methods in nonstationary flood return level analysis" to the journal *Advances in Water Resources* in 2015. This paper was reviewed by two anonymous referees, but due to some unknown reasons we received a decision letter with only one referee's comment in the editor's mail. In that mail, it was just mentioned that the other anonymous referee had suggested a major revision and posted his/her detailed comments as an attachment. However, this attached comment was not found in that mail or in the submission system. We had tried to contact the editor to ask for this missing attachment but without receiving any replies. We have then revised the paper based on the comment of one reviewer and based on our improved understanding of the topic, and decided to submit the revised paper to *Hydrology and Earth System Sciences*. Once again, we deeply regret for this incident and thank the reviewer for reading our paper two times. We are happy to have this opportunity to respond to reviewer's professional comments.

General comment 2:

The main contribution of this study should be the use of the expected-number-of-events (ENE) method and the negative binomial distribution to replace the Poisson distribution under overdispersion conditions. Concerning the ENE method, it is only one of the possible approaches to define return periods and corresponding return levels. It yields the general equation $T = \frac{T}{m\sum_{t=1}^{T}(1-F_t(x_T))} = \frac{1}{mE[1-F_t(x_T)]}$, which reduces to the classical $T = \frac{1}{m[1-F(x_T)]}$ if $F_t(x_T) \equiv F(x_T)$ for each *t*. In other words, the return period corresponds the expected value of the reciprocal of the exceedance probability of a fixed quantile x_T, which is constant if $F_t(x_T)$ is constant. Therefore, sentences such as "This advantage makes the method able to provide unique design value for reference even though the flood behaviors observe nonstationarity, which is beyond the capacity of traditional stationarity strategy" make little sense because return periods and return levels, being expected values taken over *T* (for ENE) or ∞ (for expected waiting time), are always unique values in both stationary and nonstationarity frameworks (a discussion is provided by Serinaldi (2015)).

Response:

We thank the referee very much for the valuable and professional remark. We fully agree with the referee's theoretical demonstration for the ENE method. It is really true that the ENE method can provide unique *T*-year return level in both stationary and nonstationary contexts, and on stationary condition, the ENE method will be the same as the traditional stationarity strategy. Actually, in the original paper, we have admitted this important theoretical fact of the ENE method in Lines 315-333.

The sentence "*This advantage makes the method able to provide unique design value … traditional stationarity strategy*" in fact did not refer to the disability of the ENE method when this method is used in a stationary framework, but meant that the traditional stationarity strategy, assuming that the exceedance probability of *T*-year return period equals to $1-F(x_T)=1/(mT)$, cannot offer unique design values x_T when nonstationary distribution model with time-varying parameters has been applied. For example, with the use of the LNO3 model in the original manuscript, the 50-year AM flood return level (corresponding to the exceedance probability of 0.02) as shown by the red line in Figure 1 below varies from year to year. The similar examples have been earlier reported in previous literatures (e.g., Villarini et al., 2009; López and Francés, 2013). In the newly revised manuscript, we have rephrased the relevant sentences to avoid communicating this misleading message.

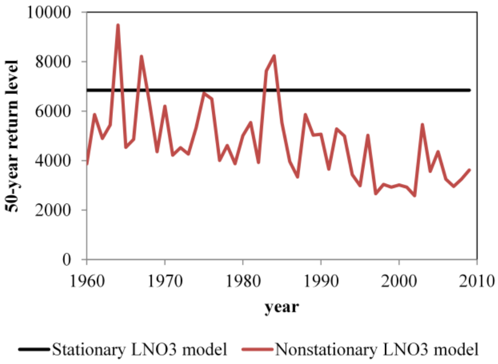

Figure 1 50-year return levels (m^3/s) (corresponding to the exceedance probability of 0.02) estimated with the stationary (black line) and nonstationary (red line) LNO3 models, respectively.

<u>Reference</u>

López, J., and Francés, F.: Non-stationary flood frequency analysis in continental Spanish rivers, using climate and reservoir indices as external covariates, Hydrol. Earth Syst. Sci., 17, 3189-3203, doi:10.5194/hess-17-3189-2013, 2013.

Villarini, G., Smith, J.A., Serinaldi, F., Bales, J., Bates, P.D., and Krajewski, W.F.: Flood frequency analysis for nonstationary annual peak records in an urban drainage basin, Adv. Water Resour, 32, 1255-1266, doi:10.1016/j.advwatres.2009.05.003, 2009.

General comment 3:

As far as the negative binomial distribution is concerned, it was already discussed in stationary flood frequency analysis, and compared with Poisson by Bhunya et al. (2013), while its introduction and theoretical justification in stationary and nonstationarity frameworks was presented by Eastoe and Tawn (2010). In particular, the latter highlighted how the overdispersion is not necessarily a consequence of nonstationarity. In fact, overdispersion can easily results from (hidden) persistence (see e.g. Serinaldi F, Kilsby CG. (2016a) and references therein) and/or mixed effects (random fluctuations of the rate of occurrence). Moreover, saying that "over-dispersion of observations" is a possible source of nonstionarity (P9L161- 168) seems to me logically flawed because overdispersion is not a cause but an effect of non-Poissonian behaviour, which can have many different causes. Moreover, other models such as generalized Poisson can be used (Raschke, 2015). Again, nonstationarity is not a necessary condition. However, what really matters is that the distribution of the number of event under nonstationarity is neither Poisson nor negative binomial, but Poisson binomial (Tejada and den Dekker, 2011; Obeysekera and Salas, 2016). Thus, a more careful literature review should be performed before running (a bit blindly) computer codes/packages.

Response:

Great thanks to the referee for the valuable and professional remark and pointing out the inaccurate statement. Sorry that we failed to make clear distinction between the important concepts of nonstationarity and over-dispersion. In the revised manuscript, we have corrected the wrong wording of “two-type source of nonstationarity” to clarify these two concepts.

In this study, we have employed the NB distribution (Anscombe, 1950) as an alternative to the Poisson distribution. There are four main considerations that motivate us to adopt the Negative Binomial (NB) distribution as follows:

(1) The NB distribution is a two-parameter mixture of the Poisson distribution and includes the Poisson distribution as a special case (Please refer to Table 1 for the probability mass function in the original manuscript).

(2) The NB distribution is theoretically justifiable for describing over-dispersed data (variance-to-mean ratio greater than unity), while the assumption of Poisson distribution is invalid as it only allows a fit of equi-dispersed data whose variance and mean is identical.

(3) The arrival rate of POT flood has been reported to be over-dispersed in many available literatures. The NB distribution has received a wide use to accommodate the over-dispersion of POT arrival rate (Ben-Zvi, 1991; Önöz and Bayazit, 2001; Silva et al., 2012; Bhunya et al., 2013). Most studies have applied a stationary NB model (with constant parameters) to fit the over-dispersed data (e.g., Cunnane, 1979; Bhunya et al., 2013), and only a few ones have focused on evaluating whether the over-dispersed data have also shown a nonstationary behavior over a certain long time period. Therefore, this study has been intended to supplement some gaps in studies with the over-dispersed data, in which the accuracy of both stationary and nonstationary (with time-varying parameters) NB models has been evaluated in flood return level analysis.

(4) The requirement of independent data is relaxed when we fit a nonstationary NB model (which is strictly required by the Poisson model, whether with constant or time-varying Poisson process intensity). This advantage has made the NB distribution become increasingly popular especially when it is doubtful whether the observed arrival rates from a stochastic process satisfy the assumption of independence (Johnson et al., 1992).

Within the above background, we believe that the use of NB distribution is in the scope of this study and should be sufficient for the current purpose. It should mention that there have been different studies of other discrete distributions proposed for describing count data, such as the generalized Poisson (Johnson et al., 1992), the Poisson binomial distribution (Obeysekera and Salas, 2016), or other mixed/derivative distributions (Johnson et al., 1992). Since the adoption of a single type of optimum distribution for POT arrival rate is usually inconclusive in practical application, all the above distributions have been ever successfully used for a specific basin with their respective advantages, e.g., the Poisson binomial distribution, derived as a convolution of Poisson and binomial distributions (Johnson et al., 1992), allows to describe the under-dispersion (i.e., variance is lower than mean). The Poisson binomial distribution is a useful tool that may be applied to model the frequency of POT events (Obeysekera and Salas, 2016) but not the single way to realize this. Although incorporation of other applicable distributions would be lengthy for this paper, it should be a very interesting and meaningful topic for further research.

Following the referee's suggestion, we have carefully reviewed the relevant literatures and summarized them briefly in the newly revised manuscript. For the purpose of giving a better understanding of the NB distribution to the readers, we have added the explanation of theoretical background of the NB distribution, the test of over-dispersion before conducting calculation, and revised the text in the related part of the revised version.

Reference

Anscombe, F.J.: Sampling theory of the negative binomial and logarithmic series distributions, Biometrika, 37, 358-382, doi:10.2307/2332388, 1950.

Ben-Zvi, A.: Observed advantage for negative binomial over Poisson distribution in partial duration series, Stoch. Hydrol. Hydraul., 5, 135-146, doi:10.1007/BF01543055, 1991.

Bhunya, P. K., Berndtsson, R., Jain, S. K., and Kumar, R.: Flood analysis using negative binomial and Generalized Pareto models in partial duration series (PDS), J. Hydrol., 497, 121-132, doi:10.1016/j.jhydrol.2013.05.047, 2013.

Cunnane, C.: A note on the Poisson assumption in partial duration series models, Water Resour. Res., 15, 489-494, doi:10.1029/WR015i002p00489, 1979.

Johnson, N. L., Kemp, A. W., and Kotz, S.: Univariate Discrete Distributions, Second Edition, New York, John Wiley and Sons, 1992.

Önöz, B., and Bayazit, M.: Effect of the occurrence process of the peaks over threshold on the flood estimates, J. Hydrol., 244, 86-96, doi:10.1016/S0022-1694(01)00330-4, 2001.

Obeysekera, J. and Salas, J. D.: Frequency of Recurrent Extremes under Nonstationarity, J. Hydrol. Eng., 21, doi: 10.1061/(ASCE)HE.1943-5584.0001339, 2016.

Silva, A.T., Naghettini, M. and Portela, M.M.: On some aspects of peaks-over-threshold modeling of floods under nonstationarity using climate covariates, Stoch. Environ. Res. Risk Assess., 1-18, doi:10.1007/s00477-015-1072-y, 2015.

General comment 4:

Most of the conclusions in the case study are quite speculative because the behaviour of the return periods under nonstationarity depends on many factors, such as the link functions, the relationships between distributions' parameters and covariates (linear or polynomial regression are surely convenient but also quite arbitrary and surely not physically based), as well as the nature of the distributions (fat tailed, heavy tailed, etc.). In this respect, conclusions are quite fair as they reflect the overall uncertainty of the empirical results, which is however exacerbated by lack of theoretical reasoning on the rationale and true nature of the methods used.

Response:

We thank the referee very much for the insightful comment/remark. We fully understand the reviewer's concern about the uncertainty involved in the return levels estimated in a nonstationary context. Admittedly, flood return level analysis comprises many procedures for sampling scheme, probability distribution model, and return level concepts. Each procedure entails many assumptions and would introduce uncertainties (e.g., choice of data, regression modeling, fitting technique; operational decision) (Yen, 2002). Therefore, improvement of reliability of flood return level is still a big challenge in hydrologic studies, not only on nonstationary but also on stationary conditions. In this study, some efforts have actually been made to relieve the impact of uncertainties in nonstationary flood return level analysis as much as possible, such as the effect of the choice of data, importance of preliminary diagnosis and attribution analysis, modeling of probability distribution including the covariates-dependent relationship, and to better understand the overall uncertainty in nonstationary flood return level analysis, we have also performed a sensitivity analysis to study how flood return level would be influenced by changing climate, etc.

In the revised manuscript, we have intended to improve the elaboration on theoretical reasoning and the methods used, and illustrated the limitation and future direction of the nonstationary flood return level analysis.

Reference

Yen, B.C.: System and component uncertainties in water resources. Risk, Reliability, Uncertainty, and Robustness of Water Resources Systems, Bogardi, J.J., Kundzewicz, Z.W. (eds), International Hydrology Series. Cambridge University Press: Cambridge, 133-142,

2002.

General comment 5:

By the way, it is worth noting that the models with parameters depending on covariates that exhibit stochastic fluctuations (such as rainfall and temperature) are not nonstationary but simply doubly stochastic. Nonstationary models require that the distribution (marginal and joint) change with time according to some well-defined function holding true for whatever instant along the time axis. In this respect, trend analysis can only detect local changes in a very small interval (i.e., the period of record), and this explains why nonstationarity cannot be inferred from trend analysis but requires exogenous information, i.e. attribution based on physical reasoning rather than statistical correlation analysis. Some additional specific remarks are provided below. I also refer the Authors to my report for the previous AWR version.

Response:

We thank the referee very much for this valuable and thought-provoking remark, which has prodded us into thinking more deeply on the concept of nonstationarity. Here, we would like to address this remark from four main aspects beginning with the question about nonstationarity and make a discussion:

(1) *What should be nonstationarity?* For a very long interval on the time axis, the so-called significant (local) changes maybe in fact belong to parts of the behavior in stochastic fluctuations since the observed hydrological records is very short compared with the infinite hydrological process. A typical example can be assumed with a stochastic regime-switching process (Serinaldi and Kilsby, 2015), which has been usually occurred in climatic variable with a periodicity characterized by the alternatively positive and negative variations. Both positive and negative variations are locally persistent fluctuations rather than the behaviors of nonstationarity. Therefore, the observed changes (in a small time interval) do not necessarily imply nonstationarity, and certainly stationarity does not contradict (local) changes (Montanari and Koutsoyiannis, 2014).

(2) *How can nonstationarity be inferred?* It is true that nonstationarity cannot be inferred simply from the result of statistical tests on a finite observation sample. The problem in the understanding of nonstationarity is that the complex natural process that evolves over time can never suffice for the requirement of the representative observation of population. No matter how large the sample size of observation is, the observed series that is updated and prolonged along time axis will always be the segment of a natural process. In order to infer the real nonstationarity, in addition to the observation data alone, detection of

nonstationarity requires exogenous information to explicitly interpret the physical mechanism of nonstationarity and further to guarantee that the patterns observed in a time slice is not just an effect of fluctuations of a stationary process (Serinaldi and Kilsby, 2015).

(3) *How can nonstationarity be described?* We accept the viewpoint that nonstationarity is justifiable if it can be well-defined by a deterministic model that is right for whatever instant along the time axis. Therefore, the current use of a nonstationary model is merely a modelling option on the basis of observation data (Serinaldi and Kilsby, 2015), which might be in fact devised to model the irregular changes in globally stationary process. However, we have to admit one fact that even if a true perfect deterministic function really exists for nonstationary case and holds true along the entire time axis, it can never be exactly known given the complexity and dynamic nature of hydrologic system (Milly et al., 2015), but estimated based on the data and await perpetually the test by time when longer observations become available. This is very different from the case of synthetically stochastic process predefined already by a mathematical construction to generate a large number of samples, as has been exemplified in some literatures (e.g., Koutsoyiannis and Montanari, 2014). Indeed, the nonstationary model may be not the best choice for modeling a nonstationary process as it is unable to ensure a perfectly reliability for future application. A more justified description of nonstationarity based on the theory of stochastic process or dynamic systems has been developed and suggested in literatures (Koutsoyiannis, 2006; Montanari and Koutsoyiannis, 2012, 2014; Koutsoyiannis and Montanari, 2014; Serinaldi and Kilsby, 2015), which would advance the development of our future studies in modeling of the nonstationarity in the hydrological series.

(4) *How did we handle nonstationarity?* In this paper, we would like to talk about nonstationarity in a conservative way, i.e., to use the word nonstationarity to simply mean that the observed time series is no longer identically distributed over a certain time period. This use implies the property of distribution models but not to say that the nature of a natural process is nonstationary, which has in fact been widely acceptable and applied in most of studies that have carried out the investigation of nonstationarity in flood series (Villarini and Serinaldi, 2012; López and Francés, 2013; Salas and Obeysekera, 2014). Here the nonstationarity, as the referee pointed out, is valid only for a finite time period depending on the data record used, and should be a local change (relative to the very long time). The nonstationarity should have actually existed in the hydrological series in many basins worldwide under the changing climate (IPCC, 2013), and in the Weihe basin, China, the significant variability in the natural river regime has been reported repeatedly

before (Zuo et al., 2012; Du et al., 2015), and has been prove to be mainly ascribed to the impact of climate change (Xiong et al., 2014; Jiang et al., 2015).

In the revised manuscript, we have explicitly demarcated the nonstationarity used in this study and made relevant discussion on the concept of nonstationarity in a natural process.

Reference

Du, T., Xiong, L., Xu, C.-Y., Gippel, C.J., Guo, S., and Liu, P.: Return period and risk analysis of nonstationary low-flow series under climate change, J. Hydrol., 527, 234-250, doi:10.1016/j.jhydrol.2015.04.041, 2015.

IPCC 2013 Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.

Jiang, C., Xiong, L., Wang, D., Liu, P., Guo, S., and Xu, C.-Y.: Separating the impacts of climate change and human activities on runoff using the Budyko-type equations with time-varying parameters, J. Hydrol., 522, 326-338, doi:10.1016/j.jhydrol.2014.12.060, 2015.

Koutsoyiannis, D.: Nonstationarity versus scaling in hydrology, J. Hydrol., 324, 239-254, doi:10.1016/j.jhydrol.2005.09.022, 2006.

Koutsoyiannis, D.: Hurst-Kolmogorov dynamics and uncertainty, J. American Water Resour. Association, 473, 481-495, doi:10.1111/j.1752-1688.2011.00543.x, 2011.

Koutsoyiannis, D., and Montanari, A.: Negligent killing of scientific concepts: The stationarity case, Hydrol. Sci. J., 60, 1174-1183. doi:10.1080/02626667.2014.959959, 2014.

Milly, P.C.D., Betancourt, J., Falkenmark, M., Hirsch, R.M., Kundzewicz, Z.W., Lettenmaier, D.P., Stouffer, R.J., Dettinger, M.D., and Krysanova, V.: On Critiques of “Stationarity is Dead: Whither Water Management?”, Water Resour. Res., 51, 7785-7789, doi:10.1002/2015WR017408, 2015.

Montanari, A., and Koutsoyiannis, D.: A blueprint for process-based modeling of uncertain hydrological systems, Water Resour. Res., 48, W09555, doi:10.1029/2011WR011412, 2012.

Montanari, A., and Koutsoyiannis, D.: Modeling and mitigating natural hazards: Stationarity is immortal!, Water Resour. Res., 50, 9748-9756, doi:10.1002/ 2014WR016092, 2014.

Salas, J. D., and Obeysekera, J.: Revisiting the Concepts of Return Period and Risk for Nonstationary Hydrologic Extreme Events, J. Hydrol. Eng., 19(3), 554-568, doi:10.1061/(ASCE)HE.1943-5584.0000820, 2014.

Serinaldi, F., and Kilsby, C. G.: Stationarity is undead: Uncertainty dominates the distribution of extremes, Adv. Water Resour., 77, 17-36, doi:10.1016/j.advwatres.2014.12.013, 2015.

Villarini, G., and Serinaldi, F.: Development of statistical models for at-site probabilistic seasonal rainfall forecast, Int. J. Climatol. 32, 2197-2212, doi:10.1002/joc.3393, 2012.

Xiong, L., Jiang, C., and Du, T.: Statistical attribution analysis of the nonstationarity of the annual runoff series of the Weihe River, Water Sci. Technol., 70, 939-946, doi:10.2166/wst.2014.322, 2014.

Zuo, D., Xu, Z., Yang, H., and Liu, X.: Spatiotemporal variations and abrupt changes of potential evapotranspiration and its sensitivity to key meteorological variables in the Wei River basin, China, Hydrol. Process., 26, 1149-1160, doi:10.1002/hyp.8206, 2012.

Specific comments

(1) L141-143: see Serinaldi (2015) for a wider discussion.

Response:

Thanks very much for this comment. We have carefully followed this reference and supplemented the relevant contents in the revised manuscript.

(2) L146: "other sampling methods. . .seem"

Response:

Thanks, it has been revised as suggested.

(3) L189: TFPW does not perform any prewhitening and does not preserve the nominal significance level. This explains why the results reported in the literature for MK and TFPW MK are often close to each other (see Serinaldi and Kilsby (2016b) for an analytical and numerical proof)

Response:

We thank the referee very much for the professional comment. We have carefully followed the reference provided by the referee and learned more from it. In the newly revised manuscript, we have corrected the use of original TFPW method to make real pre-whitening and pointed to this reference for the relevant theoretical basis.

(4) L202-205: The interpretation of the partial MK test is not correct. Moreover, the interpretation reflects a widespread merging of Neyman-Pearson testing procedure and Fisher's p-values, whose values cannot be interpreted as proxies of the strength of the

relationship between target variables and covariates.

Response:

We thank the referee very much for pointing out the improper expression. The misleading statement has been deleted in the newly revised manuscript.

(5) L268-275: AIC and BIC have different meaning and are relative measures. Thus, model selection should be based on Akaike weights and/or evidence ratios (see Burnham and Anderson (2002, 2004).

Response:

Thanks a lot for this suggestion. In the newly revised manuscript, we have applied the Akaike weights for model selection as suggested. However, we have retained AIC considering that it has received a wide use in hydrologic studies (BIC has been deleted as it is very similar to AIC).

(6) L289: If the process is not stationary, the empirical frequencies cannot be computed by the Gringorten formula. Classical qq plot does not make sense in a (true) nonstationary framework. Moreover, in GAMLSS, the residuals are not the normal quantile transform of the observed values (this holds only for stationary models) but the difference between the predictions (given the covariates) on the observations (for the same covariates). Furthermore, qq plots and coefficient of determination are not formal tests but diagnostic plots and measures of performance, respectively. In particular, no tests can be performed at the 5% significance level for coefficients of determination (unless ad hoc MC experiments are set up).

Response:

We thank the referee very much for this professional comment. It is true that the Gringorten formula should not be used to calculate the empirical frequency of the observations that have been assumed to be described by nonstationary distribution model with time-varying parameters, and the classical qq plot would be meaningless if it is routinely made for these observations.

Actually, in the original manuscript, we have introduced the residuals r_t of nonstationary distribution model (with time-varying parameters), which can be applied to the Gringorten formula and the classical qq plot. The calculation of the residuals r_t follows two steps, i.e., by inverting the well-established nonstationary distribution model into the cumulative probabilities at each time t and then transforming these cumulative probabilities to the standard normalized quantiles (Dunn and Smyth, 1996). For example, denote x_t being observation value at time t that is fitted by a nonstationary GEV model with time-varying parameters θ_t. $F_{\mathrm{GEV}}(\cdot)$ represents the cumulative distribution function. The cumulative probabilities at time t should be $F_{\mathrm{GEV}}(x_t|\theta_t)$. The residual r_t of the fitted GEV model is calculated as $r_t = \Phi^{-1}(F_{\mathrm{GEV}}(x_t|\theta_t))$, where Φ^{-1} is the inverse function of standard normal distribution. It follows from its definition that the residuals r_t are exactly standard normal when θ_t is constant (i.e., for the stationary GEV model), and when nonstationary model fits data well, the residuals r_t should converge to standard normal distribution (Dunn and Smyth, 1996). Therefore, the empirical frequency formula is applicable for r_t. The classical qq plot can be used to test the normality of r_t for evaluating how well the nonstationary model (with time-varying parameters) fits data.

In the newly revised manuscript, we have revised the relevant text to elaborate clearly the residuals r_t. Following the referee's suggestion, the qq plots and coefficient of determination have been deleted as they cannot give a quantifiable result of statistical tests. Instead, we have employed the worm plots and Filliben correlation coefficient (Filliben, 1975) for goodness-of-fit tests, which have been widely used for evaluating the performance of model at the 5% significance level (Villarini et al., 2010; López and Francés, 2013).

Reference

Dunn, P.K., and Smyth, G.K.: Randomized quantile residuals, J. Comput. Graph. Stat., 5, 236-244, doi:10.2307/1390802, 1996.

Filliben, J.J.: The probability plot correlation coefficient test for normality, Technometrics, 17: 111-117, doi:10.1080/00401706.1975.10489279, 1975.

López, J., and Francés, F.: Non-stationary flood frequency analysis in continental Spanish rivers, using climate and reservoir indices as external covariates, Hydrol. Earth Syst. Sci., 17, 3189-3203, doi:10.5194/hess-17-3189-2013, 2013.

Villarini, G., Smith, J. A., and Napolitano, F.: Nonstationary modeling of a long record of rainfall and temperature over Rome, Adv, Water Resour., 33, doi:10.1016/j.advwatres.2010.03.013, 1256-1267, 2010.

(7) L494-496: This result is not so surprising because the number of exceedances decreases as the threshold increases, and therefore the clustering of extreme events is more evident given the short time series.

Response:

Thanks a lot for this valuable comment. We have reframed the sentence according to this remark in the newly revised manuscript.

(8) Section 4.3: GAMLSS are nothing but an advanced form of regression. Using the fitted model with covariates taking values beyond the fitting range is never a good idea because we do not know if the fitted relationship still holds true in that range.

Response:

We thank the referee for this valuable comment. We fully agree that the fitted best model with parameters as functions of climatic covariates may not hold true for future prediction as no one can tell what the future really should be like. Actually, the extrapolation of future design floods by using the best fitted nonstationary model pre-determined with historical data has an underlying assumption that the priori model used for projection is acceptably reasonable at present and in the near future (Du et al., 2015; Milly et al., 2015). This assumption is built on the consideration that flood processes themselves may present correlation or locally persistent fluctuations over a certain time period. For the practical point of view, we would like to say it is not the best idea but a realistically acceptable and useful idea, which serves the practical need for prediction in design, planning, and management over the decades-long design horizon of engineering (e.g., related to a specific multi-year or short-term project plan for a certain future design period). Ignoring the detected local change induced by climate change for making prediction over a certain design period may cause large estimation errors if only considering a single use of stationarity strategy. Therefore, the current nonstationary flood return level analysis is aimed at what should be done when nonstationarity (or short-term local changes) really happen and how to understand the uncertainty when this nonstationarity has been considered in modeling and predicting floods.

Within the above background, this study is intended to make a contribution for practical applications in a nonstationary design framework over a certain time period and provide an alternative choice for decision-makers when significant changes have been informed and destroyed the identically distributed assumption in engineering design. The present study has attempted to seek a way to achieve multi-decadal flood projections, which enables us to obtain a short-term foresight of the variation tendency of flood return levels, and performed a global sensitivity analysis to help understand how the design floods would be influenced by the changing climate or how large the possible overall uncertainty would be if the pre-determined nonstationary model is applied to future application. It should mention that the current method of nonstationary flood return level analysis is in fact devised based on the observed local changes but not the true understanding of global variations of natural process, and thereby admittedly far from perfect. The researchers should always consider the stationary flood frequency analysis, an effective tool to handle the possibility of irregular local variations in a stationary process and avoid a misuse of nonstationarity for the very long future.

To address this comment, in the revised manuscript, we have illustrated more explicitly the assumption that the extrapolation analysis of future flood with nonstationary models should not be readily given for a very distant future but confined in a specific time period during which the pre-defined nonstationary model can be practically acceptable.

Reference

Du, T., Xiong, L., Xu, C.-Y., Gippel, C.J., Guo, S., and Liu, P.: Return period and risk analysis of nonstationary low-flow series under climate change, J. Hydrol., 527, 234-250, doi:10.1016/j.jhydrol.2015.04.041, 2015.

Milly, P.C.D., Betancourt, J., Falkenmark, M., Hirsch, R.M., Kundzewicz, Z.W., Lettenmaier, D.P., Stouffer, R.J., Dettinger, M.D., and Krysanova, V.: On Critiques of "Stationarity is Dead: Whither Water Management?", Water Resour. Res., 51, 7785-7789, doi:10.1002/2015WR017408, 2015.

(9) English should be revised and some typos fixed.

Response:

Thanks, we would like to employ an English editing service to improve the writing quality of the newly revised manuscript.

Once again, many thanks for your professional and valuable comments which greatly improve our research and paper.

With best wishes

Yours sincerely

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