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Discussions

Interactive comment on "Application of a hybrid approach in nonstationary flood frequency analysis – a Polish perspective" *by* K. Kochanek et al.

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Reply to interactive comment on "Application of a hybrid approach in non-stationary flood frequency analysis – a Polish perspective"

by

K. Kochanek et al.

Anonymous Referee #1

Dear Anonymous Reviewer,

The authors would like to thank you for the careful reading of the manuscript and painfully honest review and comments. We do hope that our answers and corrections made in the text will alleviate your criticism.

Our paradigm, rationale and assumptions, that we found easy to understand by applications oriented reader, are as follows:

1. The one of the main aims of the article was to satisfy the needs of Polish hydrological service for a simple (which we consider as an advantage) and reliable tool for flood frequency analysis (FFA) for the non-stationary datasets.

2. It is obvious, that the famous climate changes cannot be ignored, although their roots, roles and different hydrological impacts are still under debate.

3. It is commonly recognized and verified by hydrological observations and models that changes in intensity of summer precipitation, anthropogenic stressors and land use in catchments area do increase the floods magnitude/frequency and so the upper quantiles that are essential for the flood protection measures and hydrotechnical structures design.

4. There is a strong pressure to develop non-stationary methods which can deal with this problem as an extension of classical FFA and methods of climate analysis which can be applied in engineering practice, even only to check the safety of existing structures.

5. Those methods ought to be data driven, although the existence of trends tends to be masked by natural variability of high waters.

6. Due to the shortness of hydrological series the trends ought to be described in the simplest possible form, i.e. linear trend in mean and standard deviation.

7. We had an impression that the Reviewer evaluates our work from the point of view of the at-site analysis. However, as the climatic change affects regions rather than individual river cross sections, the methods should be developed not for at-site but for

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regional analysis. The regional approach enables indirect verification of the form of assumed trends.

8. Trend estimators based either on the different trend forms or different distribution models for at-site data cannot be used in the regional analysis.

9. Moreover, the condition for regional analysis is the application of the same measures (indices) of non-stationarity for all stations in the region. In our case these are measures based on the assumption of a linear relationship for trend. Of course, a detailed analysis of different trend forms (e.g. exponential, logarithmic or trigonometric) could show the spatial variation of best fitting forms.

10. However, our experience has shown that two models of distributions with time covariates having similar goodness of fit to the series of observation assessed by the value of AIC may differ even in a direction of trends. This points out to excessive cognitive expectations in relation to the record length or to the non-significance of the trends.

11. To make the results comparable we decided to estimate trends in the moments (not in parameters) which are independent of the probability distribution type and apply the same trend models for the entire data set.

12. If the best probability distribution is chosen within the set of candidate distributions by means of AIC, then the best distribution may differ over the larger area. To avoid such a situation, the multimodel approach can be used (Bogdanowicz E., 2010, Multimodel approach to estimation of extreme value distribution quantiles. Podejście wielomodelowe w zagadnieniach estymacji kwantyli rozkładu wartości maksymalnej. In: Hydrologia w inÅijynierii i gospodarce wodnej. Tom 1, Ed. B. WiÄŹzik. Monografie Komitetu InÅijynierii ÅŽrodowiska nr 68, in Polish). Another possibility is to apply the non-parametric estimation methods of probability density function i.e. kernel estimation (Feluch, W., 1994, Selected methods of kenrnel estimation of probability density function and regression in hydrology. Wybrane metody jÄĚdrowej estymacji funkcji gÄŹs-

tości prawdopodobieństwa i regresji w hydrologii. Prace Naukowe InÅij. ÅŽrodowiska, PW, Warsaw (in Polish)). The two proposed solutions can release the inference from the type of distribution model. A useful non-stationarity index in this case may be the ratio of design quantiles at two distant time points e.g. the reference one at the beginning of data time span, another at the end, or the first in the current year and second ten or hundred years later.

Our answers to the specific comments.

A. Page 6003, lines 1-6: I disagree with the statement that scientists do not question the nature of change. Regarding Panta Rhei, this is the title of the new decadal initiative of the International Association of Hydrological Sciences (http://distart119.ing.unibo.it/pantarhei/) which is well described in Montanari et al. (2013).

We rather meant that the scientists do not question the changes themselves. It is due to the fact that detection of a regime shifts or trends is however easier than understanding the process or processes determining them. You have pointed out to works of Montanari, Hall, Vogel et al., which were released after notification of our article, so we could not refer to them. We are not within the possession of the 'copyrights' for the 'panta rhei' term, of course, and we doubt if anybody has; as a matter of fact we like this term and we have kept using it in our presentations and papers at least since the IHP symposium in Capri in 2008. All in all, congratulations and best wishes to the Panta Rhei Initiative. We are sure that Heraclitus of Ephesus, called a 'crying philosopher', would have been proud of it. By the way, we wonder if one of a contemporary 'crying hydrologists' sayings or statements is will be still remembered in 25th century. For the sake of this paper we can also adopt: Tempora mutantur et nos mutamur in illis (Lotarius the First, 815-855, the Ruler of the Franks).

B. Page 6007, line 9: b is the mean parameter of what?

Sorry for undetected permutation of words. Corrected: b – the parameter of mean

C. Page 6010, line 26: where in the plots is it to see that WLS tends periodically to the correct solution? In the TS method, I guess more robust trend estimators such as the Theil-Sen estimator could also be used. Is it the case?

In regression analysis the weighted least squares is an efficient method that makes good use of small data sets. The method works by incorporating nonnegative weights, associated with each data point, into the fitting criterion. The similar philosophy underlies the method of weighted least squares (WLS) to simultaneous assessment of trend in mean and variance elaborated and published by Strupczewski & Kaczmarek (Strupczewski, W. G. and Kaczmarek Z.: Non-stationary approach to at-site flood-frequency modelling. Part II. Weighted least squares estimation. Journal of Hydrology, 248, 143-151, 2001.) If the mean value is non-stationary the trend in variance (or standard deviation) cannot be assessed separately. Whilst variances are unequal and unknown the system of 4 equations ought to be solved for linear form of trends in mean and (here) standard deviation:

where xt – elements of time series, mt, σt – mean value and standard deviation of Xt, mt=at+b; σt=ct+b; [t,1] –gradient of the trend function. The equations are solved in respect of unknown trend parameters. It seems that Theil-Sen estimator known from its robust behavior can be a good tool in other applications. Thank you for this hint.

D. Page 6012, line 3: the advantage of reducing bias in respect to reducing RMSE should be discussed somewhere in the paper, so that the advantage of using TS instead of ML can be clarified. In what applications is the sign of the error (bias) more dangerous that the error itself (RMSE)?

The total mean square error of parameter estimate is the sum of the square bias and variance. Consequently, the design of estimators is the subject to a compromise between variance and bias. Commonly used estimation methods give the estimators which are asymptotically the most efficient provided the distribution is the true one. Well known fact is that for small samples the estimates can be biased, and that they can have smaller variance than the unbiased ones, which in some way compensate for their bias. There are methods to make the variance smaller at the expense of increasing bias, while ensuring that the overall estimation error is reduced. The bias and the variance of parameters estimates translate into bias and variance of quantiles estimates. Reduction of estimate variance and MSE can be intuitively appealing and desirable in some applications (e.g. image restoration) but not in classical FFA. For engineering design of high waters depending structures the small variance of quantile estimate, although required, is less important than the estimate itself. It means that firstly we are interested in reducing bias of the quantile estimates keeping in mind the reduction of variance. And as the variance is loosely speaking o(N-1) (where N is the sample size) the best way to reduce it is to wait for longer series. Good asymptotic properties of estimation methods vary significantly in the case of model misspecification, what is the case in FFA, where the parent distribution of maxima is not known. Therefore estimation methods try to find the 'true values of parameters of untrue probability distribution' and one can check e.g. via MC experiments errors in upper quantiles for different types of hypothetic and true distributions in goal to be aware of them in real data estimation problem. We have corrected the paper and added references to our former works on this issue. Why TS instead of ML? Because the common estimation of trend and distribution parameters leads to trend assessments which depend on distribution type i.e. our trend assessments for the same input data may be different while we choose different probability distributions. If trend in data is weak the results can be contradictory.

E. Page 6013, line 11: are trends estimated through WLS weighted on record length? Or weighted on some other characteristic?

See answer to the remark C.

F. Page 6013, line 23: why would a positive-trend extrapolation be more likely than a negative-trend extrapolation? This is unclear.

We are talking about floods in Poland (refer to the title of the article). The climate change scenarios for the Polish territory will rather result in increasing trend for extreme events i.e. floods and droughts. Therefore the increasing trend in flood peaks is commonly expected and, if we can say so, 'politically correct'. From the point of view of engineering design and planning, the increasing trend is eagerly acceptable in regard to ensure the safety of future investments and so increase a design structure dimensions. If the trends are decreasing, the decision to lower the safety levels would not be taken even when the result of performed risk analysis would have been positive. The saying of professor Vit Klemeš 'The illusion of knowledge can do more harm than awareness of ignorance' fully describes this situation.

G. Page 6014, line 4: I do not agree with the greatest value of extrapolating the trend. There is a value if the trend exists, which is assumed by the method, but could be very different in reality.

We have changed this unfortunate sentence. We absolutely agree about the reality! The new wording of this sentence is as follows: The difference of stationary and non-stationary quantiles in future years shows that if the trends continue one can expect the important changes in design values.

H. Page 6014, lines 5-8: isn't the fact that the stationary and non stationary models are similar in the series' centre a trivial result due to assuming (and fitting) a linear trend to the data (in the non stationary case)?

No, it is not. Because WLS method is not a simple regression performed for mean and standard deviation separately, so it is only an illusion that the midpoint of time interval will represent the mean values of mean and deviation corresponding to stationary conditions. In fact, the 'stationary' quantiles are equal to the quantiles for T/2 only when the standard deviation is stationary (or its linear trend is negligible), regardless the trend in the mean value. In the case presented in the paper, the trend in ïĄş, i.e. parameter c = 2, is small in comparison to the 'stationary' part of the standard deviation, i.e. d

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= 500, so: ïĄşt = 500 + 2 âĂć t. Therefore, even though the generated time series revealed trend in ïĄş, it was small enough to equalise Qt=T/2 and Qstationary. In consequence, knowing that the trend in standard deviation is ‘small’, one can simplify the calculations by substituting the WLS equations with ïĄşT/2 = ïĄşstationary. We would like to thank the Reviewer to pointing our attention to this unfortunate statement which will be changed and commented in the new version of the article.

I. Page 6014, lines 9-13: The Authors state that “This result proves that the use of traditional stationary flood quantile estimation methods for the cases where the variation of hydrological regime of rivers is evident is a far reaching simplification and leads to erroneous results and decisions. So, when the process is known to be non-stationary, also non-stationary methods should be used for its analysis.” The problem is that the variation of flow regimes (and specially the kind of variation) is far from being evident.

You wrote in your opinion that there is not such a thing as a stationary or non-stationary phenomenon. There are stationary or non stationary models. Indeed, as the modellers we got used to treat the nature from the mathematic point of view. This evident simplification of the power of nature is the clear abuse and will be corrected.

J. Page 6015, line 1: I do not understand the statement regarding getting the “desired” results. Nevertheless it seems to me a dangerous statement.

Because it really is. If one can choose between two accepted methods which give opposite results it is easy to manipulate the conclusions. We do not approve such practices, of course, but we cannot fully eliminate them.

K. Page 6015, line 17: I’m sceptical about drawing conclusions based on accepting the uncertainty. I would rather draw conclusions based on accepting the assumptions of the method, incorporating the uncertainty in the decision process, and performing a risk-analysis such as in Vogel et al. (2013).

Hydrologists are accustomed with the uncertainty – it is, after all, a permanent feature of all hydrological activities starting from observation and measurements. Whilst uncertainty is not directly accounted in many hydrological calculations and analysis, drawing conclusions based on accepting the uncertainty is a common practice.

L. Page 6016, lines 19-22: the fact that the stationary and non stationary models are similar in the series' centre is a trivial result due to assuming (and fitting) a linear trend to the data. In my view it is an assumption, not a finding.

See the answer to the H remark.

M. Page 6016, line 25: as before, the problem is that the variation of flow regimes (and specially the kind of variation) is far from being evident.

Yes, it is.

N. Figs. 1 and 2: the resolution should be improved. A couple of figures could be added, such as the time series of floods for the Warszawa-Nadwilanówka station.

The graphs were improved. In fact we changed the Fig. 2 because in our opinion it is more informative. We added Fig. 3. With the time series for the Warszawa-Nadwilanówka station.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C2786/2014/nhessd-1-C2786-2014-supplement.pdf

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