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Natural Hazards and Earth System Sciences
Discussions

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

Anonymous Referee #1

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This paper proposes a method to fit a non-stationary flood frequency distribution to flood data. The method consists of calculating a linear trend for the moments of the annual/seasonal maximum flows with Weighted Least Squares, subsequently detrending the series, and then using the method of L-moments on it. The Authors compare this simple method to the Maximum Likelihood method and demonstrate, through Monte-Carlo experiments, its lower bias in case the assumed population of floods is unknown. The other conclusions of the paper are (i) that the stationary and non-stationary (linear trend) models differ more far from the centre of the series' time span - which is, in my opinion, a trivial consequence of the assumptions rather than a result - and (ii) that, if

a linear trend exists, it is better to account for it - which is very true but not useful. I find very misleading the sentence in the abstract saying that "It (this second result) proves that use of traditional stationary methods in conditions of variable regime is too much a simplification and leads to erroneous results.". This is not something demonstrated in this paper. For instance, would it be better to use the linear-trend model if the reality varies differently? It may turn out that the stationary model is more robust to the unknown future than the non-stationary one, exactly as L-moments are more robust to the misspecification of the population distribution than the maximum likelihood method. Regarding the last sentence of the abstract ("Therefore, when the phenomenon is non-stationary, so should be the methods used for its interpretation"), the problem is that the variability in time of extremes is far from being understood and probably very far from being linear (see e.g., Montanari et al., 2013; Hall et al., 2013). Moreover, in my opinion (but this is just philosophical and subjective), there is not such a thing as a stationary or non-stationary phenomenon. There are stationary or non stationary models.

In brief, the paper presents and assesses a simple method of non-stationary flood frequency analysis and draws conclusions which are trivial consequences of assumptions and are irrelevant. The Authors should instead focus on stating clearly and discussing under which assumptions would the method be valid.

Detailed comments:

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).

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

Page 6010, line 26: where in the plots is it to see that WLS tends periodically to the

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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?

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)?

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

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

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.

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)?

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.

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

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

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the method, incorporating the uncertainty in the decision process, and performing a risk-analysis such as in Vogel et al. (2013).

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.

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.

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.

References:

Hall, J. et al.: Understanding flood regime changes in Europe: a state of the art assessment, Hydrol. Earth Syst. Sci. Discuss., 10, 15525-15624, doi:10.5194/hessd-10-15525-2013, 2013.

Montanari et al. (2013) "Panta Rhei - Everything Flows": Change in hydrology and society - The IAHS Scientific Decade 2013-2022, Hydrological Sciences Journal, 58(6), 1256-1275, doi:10.1080/02626667.2013.809088.

Vogel, R. M., Rosner, A., and Kirshen, P. H.: Brief Communication: Likelihood of societal preparedness for global change: trend detection, Nat. Hazards Earth Syst. Sci., 13, 1773-1778, doi:10.5194/nhess-13-1773-2013, 2013.

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