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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Statistical approaches for assessment of climate change impacts on low flows: temporal aspects" *by* Anne Fangmann and Uwe Haberlandt

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Review of "Statistical approaches for assessment of climate change impacts on low flows: temporal aspects", submitted to HESSD by A. Fangmann and U. Haberlandt

This paper assesses the value of temporal regression models for predicting past variability and future projections in low flow characteristics. Variable selection and reduction techniques incl. principal component analysis have been used to obtain best performing models that use a selection and combination of various climate characteristics including SPEI, aridity index and climatic water balance at various time scales and lag times. The performance of the model has been assessed for an ensemble of

28 gauges in Lower Saxony, and compared to a process-based hydrological model for a subset of 7 catchments. The paper gives a comprehensive, in-depth assessment of parameters and model quality and shows that the statistical approach seems feasible and valuable. The paper fits very well in the scope of the journal and is a significant contribution to regional hydrology in general, and hydrological extremes in particular. The paper is generally well written and easy to follow (albeit some, textual modifications are necessary in several places). The methods are sound and appropriate but some points need to be clarified before publication.

General comments p.9, L.25-33: Restricted models: "For the OLS, GLS and principal component model, a second variant with restricted variable selection is applied, via resampling 30 yrs. of data by bootstrapping.": I wonder what criterion is exactly used to separate significant time-dependence from randomness? (the criterion "all subsamples" would include randomness, which is normally restricted to some confidence level alpha of e.g. 5%). It is also said that "The random sampling disrupts the chronological order of the time series." And "If the regression is significant for all subsamples, the relationship is considered continuously stationary." I guess not the chronological order is of matter, but the change of coefficients for different time slices? p.14, L.10 – p.15, L.19: Modeling of other low flow indices: This is a very useful discussion but should be extended to include the process-based model as well. The latter is expected to better cover the dynamics of low flow events and this should be an advantage of the more dynamic indices, such as onset, duration, peak. It would be interesting to learn more about this point. I think that such an evaluation might be not too difficult as predictions of the process-based model are already available. A discussion could be added under the assumption that NM7Q is a relevant fitting criterion for the low flow parts of the hydrograph and therefore useful for all mentioned low flow statistics. p.21, L.18 ff: Analysis of temporal shifts: I agree that the analysis of winter low flow indices provide some evidence for seasonal shifts (although more specific analyses should be more informative). However, I find the analysis of annual NM7Q not conclusive, as most catchments may be expected to have a clear summer regime and this is simply reflected in the annual values in the same way as in the summer low flow indices. Suggest to leave it out.

p.23, L.10: "Non-stationarity within the relationship ... appear to be an issue, ... Ideally the models should be revised through inclusion of methods to map potential non-stationary processes and interrelationships." I guess this conclusion is not comprehensive enough, and should be formulated more strongly. Stationarity in the sense of parameter-stability is a pre-requisite for future predictions (e.g. Parajka et al. (2016). In your MS you have proposed a model selection method that allows only for stable parameters during model selection. The question remains if this is sufficient to guarantee parameter stability in the future... Priority could have been given to the restricted PC model for the sake of more robust future predictions (cp. To p.16, L.4). The paper performs a spatio-temporal assessment on a station-by station basis. A reference to space-time methods could be given in the outlook, together with some expectations based on the assessments of Fangmann (2017).

Minor comments Sect. 4.1 Model performance: I think the term G.O.F measures is not properly used here: All of the measures are "performance measures", only those referring to the calibration (=fitting) period are usually termed "G.O.F" measures and the ones referring to an independent validation period "predictive performance". Please clarify and make consistent use of terms to avoid confusion (text and figure captions). p. 17 L.1: "NSE of the ranked simulated and observed index time series" - Does this mean that you are assessing rank statistics (ranks) instead of original data? Pls. Clarify. p.21, L.3: Can the effect of area be made more explicit, e.g. by adding some statistical figures in the text, or by co-plotting the area in Fig. 12? p.23, L. 6: Modelling of annual low flow values as a function of annual meteorological indices - I think annual and seasonal would be correct (cp. to temporal index scheme in Fig. 2, and p.11, L.22-23) – pls clarify.

Technical comments See the annotated MS for further, mainly technical comments on the MS.

Gregor Laaha, 16 Aug 2018

Reference: Parajka, J., Blaschke, A. P., Blöschl, G., Haslinger, K., Hepp, G., Laaha, G., Schöner, W., Trautvetter, H., Viglione, A. and Zessner, M.: Uncertainty contributions to low-flow projections in Austria, Hydrology and Earth System Sciences, 20(5), 2085–2101, doi:10.5194/hess-20-2085-2016, 2016.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-284/hess-2018-284-RC1-supplement.pdf

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