Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-284-AC1, 2018

Hydrology and Earth System Sciences
Open Access
Discussions

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

Anne Fangmann and Uwe Haberlandt

fangmann@iww.uni-hannover.de

Received and published: 1 October 2018

Dear Gregor, Thank you very much for reviewing our manuscript. We gladly follow your suggestions. Please find below the responses to your comments:

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 signiïňĄcant time-dependence from randomness? (the criterion "all subsamples" would include ran-

domness, which is normally restricted to some conïňĄdence 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 signiïňĄcant for all subsamples, the relationship is considered continuously stationary." I guess not the chronological order is of matter, but the change of coefïňĄcients for different time slices?

- We agree that the requirement of all subsamples having to yield a significant regression is too strong. Actually the test has been set up to at least require 90% of the subsample regressions to be significant. This has been communicated wrongly in the original manuscript and has been corrected. "If the regression is significant for more than 90% of the subsamples, the relationship is considered continuously stationary." - Testing the significance of slopes fitted to individual subsamples implies analysis of the variability in the regression parameters, as shown in Fig. 1 (test carried out for random station). The advantage of using the significance test is that specific thresholds can be deduced (5% significance level). The description has been adjusted, "disrupting the chronological order" has been removed.

p.14, L.10– p.15, L.19: Modeling of other low ïňĆow 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 ïňĆow 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 difïňĄcult as predictions of the process-based model are already available. A discussion could be added under the assumption that NM7Q is a relevant ïňĄtting criterion for the low ïňĆow parts of the hydrograph and therefore useful for all mentioned low ïňĆow statistics.

- The time series simulated by the hydrological model have been used to compute all considered indices, which are directly compared to the indices simulated by the statistical approach. The NM7Q appeared to be a good fitting criterion for the hydrological model to reproduce the NM7Q, NM30Q, Q95 and Q80. In order to better reproduce volume and duration based indices, a second calibration has been applied, using the Vmean as fitting criterion, which showed one of the lowest validation performances for the statistical approach. The section and belonging Figure have been changed accordingly.

p.21, L.18 ff: Analysis of temporal shifts: I agree that the analysis of winter low ïňĆow indices provide some evidence for seasonal shifts (although more speciïňĄc analyses should be more informative). However, I ïňĄnd the analysis of annual NM7Q not conclusive, as most catchments may be expected to have a clear summer regime and this is simply reïňĆected in the annual values in the same way as in the summer low ïňĆow indices. Suggest to leave it out.

- We agree that the analysis of the annual values is redundant and removed it from the manuscript. Numbers that state the percentages of summer and winter low flows in past and future periods have been added instead.

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. Parajkaetal. (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 sufïňĄcient 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 discussion with regard to non-stationarity has been intensified (section 4.1). - A discussion about the limitation of the restricted models for future application has been added (section 4.3). - The restricted PC model is now used for the assessment of future changes.

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).

- Outlook was added (p.33, l.12-17).

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 (=ïňĄtting) 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 ïňĄgure captions).

- GOF has been changed to “predictive performance” where appropriate.

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.

- The assessment is not based on rank statistics but simply on the deviance in the original data of the two ordered samples (due to the temporal dissociation between climate model data and observed data). Clarification is added in the text.

p.21, L.3: Can the effect of area be made more explicit, e.g. by adding some statistical ïňĄgures in the text, or by co-plotting the area in Fig. 12?

- A Figure showing the distribution of area has been added.

p.23, L. 6: Modelling of annual low ïňĆow 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.

- By “annual” we wanted to state that one value per year was considered, independent of its base period used for computation. In order to avoid misunderstandings “annual” was removed.

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

- The technical comments have been acknowledged and changes have been made to the affected sections. Please see the attached supplement.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-284/hess-2018-284-AC1-supplement.pdf

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-284, 2018.

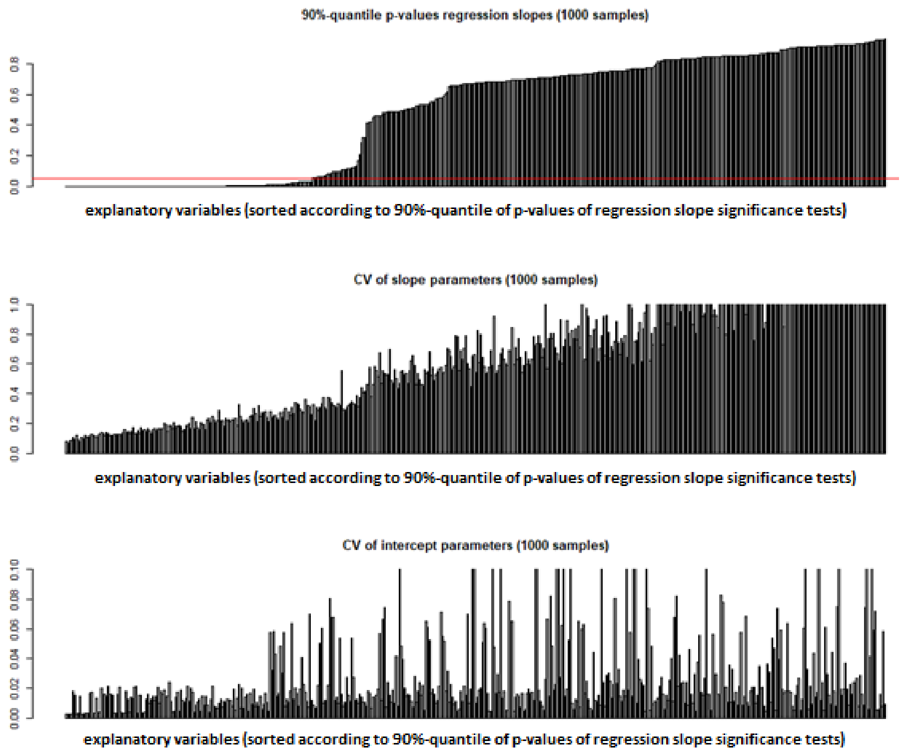

Fig. 1.