Hydrol. Earth Syst. Sci. Discuss., 12, C1270–C1273, 2015
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Hydrology and Earth System Sciences
Discussions
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HESSD
12, C1270–C1273, 2015

Interactive Comment

Interactive comment on "Climate elasticity of streamflow revisited – an elasticity index based on long-term hydrometeorological records" *by* V. Andréassian et al.

Anonymous Referee #2

Received and published: 28 April 2015

SUMMARY & OVERALL EVALUATION:

The manuscript of Andréassian and colleagues focuses on techniques for estimating streamflow elasticity to precipitation and potential evaporation (later referred to as "elasticity"), a quantity that can be used to assess expected effects of climate change on water availability. The paper first introduces the concept and systematically revisits past theoretical and empirical research in this field. Subsequently the authors introduce five methods for estimating elasticity from data of which one is a nonparametric approach and the other four are based on linear regression. The capabilities of the respective methods are then assessed with respect to elasticity of the Turc-Mezentsev model and an optimal method is selected. Finally the authors apply the method to estimate elasticity at hundreds of catchments in France.

Overall I do like this paper. The topic is well suitable for the broad audience of HESS and the newly developed method for estimating elasticity has the potential to advance the research in the field. The paper is well structured and both methods and results are clearly presented. Nevertheless the paper would profit from additional clarifications, which I outline below:

SUGGESTED CLARIFICATIONS/ADDITIONS:

(1) I find it unexpected that the authors produce the log-likelihood for the GLS model, as linear regression models with auto-correlated residuals are available in widely used statistical software. Therefore I would consider this a technical detail, which might be better captured with a reference to a comprehensive textbook. If the Authors feel that it is necessary to provide the log-likelihood to reproduce their results, I would recommend moving it to the Appendix.

(2) I do find the ad-hoc preference of GLS over OLS not convincing. Of cause it is expected by construction that the residuals of the proposed GLS model are not auto-correlated. Residual auto-correlation is mostly an issue for inference of confidence intervals etc. (as it would violate the assumption of independent and identically distributed (iid) variables). Consequently an explicit treatment of the correlated residuals might affect the estimation of the confidence intervals (and the p-values) of the bootstrap procedure. It is, however, a-priory not clear whether residual autocorrelation negatively affects the precision of the estimate. Therefor I would welcome if the authors could provide an empirical comparison of elasticity (and confidence intervals), estimated with OLS and GLS on the basis of all catchments considered. In addition I do not understand why the Authors check the normality of the residuals, as they assess the significance of the parameters using a bootstrap approach.

(3) Alberto Viglione (Reviewer #1) noted that the bivariate linear model used to esti-

mate elasticity, closely resembles the total differential of the Turc-Mezentsev model and concludes that the comparison of the univariate and the bivariate elasticity estimates might be biased. While I like the authors idea to test the different methods with respect to a theoretical model, I do share the concerns of Alberto Viglione. A possible approach to resolve this issue is to assess the capability of the suggested estimators with respect to other theoretical models (although the total differential is still additive). Possibly this could be done with respect to the formulation of Fu (1981) which is described in Zhang et al. (2004). In addition Table 1 of Zhang et al. (2004) provides an overview of some additional formulations of the coupled energy-water balance over land.

(4) For interpreting the estimated elasticity values, the authors compare the estimates to catchment area (Fig. 10). The correlation is almost zero, suggesting that catchment area is not a primary control of elasticity. Instead, theoretical work suggests that the sensitivity of Q and E to climate forcing is controlled by mean climatic conditions. This is e.g. shown in the figure provided by Alberto Viglione, which plots elasticity as a function of Ep/P.

(5) While I find the overview of the figures provided in the appendix very insightful, I would welcome if some of the results of the individual catchments could be presented in a tabular format. As a minimum output I could imagine information on: catchment name; coordinates; elevation; elasticity and longterm mean annual P, Q, Epot.

MINOR COMMENTS:

p. 3654, l. 20f: “Note for the…”: It would be more consistent to place this in section 3.3.2 Table 4,5: please provide a “full name” for sigma, not just SD (I assume this standard deviation) Table 5, caption: Change from “Univariate” to “Bivariate”

REFERENCES:

Zhang, L.; Hickel, K.; Dawes, W. R.; Chiew, F. H. S.; Western, A. W. & Briggs, P. R. A rational function approach for estimating mean annual evapotranspiration. Water

Resources Research, 2004, 40, W02502

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3645, 2015.