Hydrol. Earth Syst. Sci. Discuss., 12, C1245–C1248, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C1245/2015/

Hydrology and Earth System Sciences
Discussions

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

A. Viglione (Referee)

viglione@hydro.tuwien.ac.at

Received and published: 24 April 2015

In this paper regression models are used to evaluate the climate elasticity of streamflow in France. A bivariate regression model where M-years streamflow anomalies are related to M-years precipitation and potential evaporation anomalies is preferred to univariate ones. A countrywise analysis is conducted in France, thus allowing to check for the robustness of the methodology. The paper is interesting but I have some concerns that the Authors may find useful to discuss.

- I appreciate the fact that the Authors choose multivariate regressions to jointly ac-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

count for the influences of precipitation (P) and potential evaporation (E0) anomalies on streamflow (Q) anomalies. I would do the same because both precipitation and evapotranspiration control the long term water balance . However, my personal feeling is that comparing univariate and bivariate regressions to the Turc-Mezentsev formula is not a proper test for choosing among them. I may have missed something, but hereafter is my point. Since the Turc-Mezentsev formula (Eq. 3) involves P and E0, the total differential of Eq. (3) at a particular E0/P point exactly corresponds to the bivariate regression in Eq. (13). In other words, if we call the precipitation elasticity in Eq. (5) eQP and the potential evaporation elasticity in Eq. (4) eQE0, then

dQ = eQP*dP + eQE0*dE0

which by liner approximation with finite differences leads to

ΔQ = eQP*ΔP + eQE0*ΔE0

which essentially is Eq. (13), with the only difference that in Eq. (13) the elasticities are assumed constant while in Eqs. (4)-(5) they vary with E0/P.

Moreover, because the relationship between eq. (4) and (5), the Turc-Mezentsev model implies that a strong (> 0) precipitation elasticity always corresponds to a strong (< 0) potential evaporation elasticity (see Figure 1 below). Therefore it is not surprising that a bivariate regression always outperforms univariate ones (unless maybe for very arid catchments where eQE0/eQP -> 0 in the Turk-Mezentsev model).

- The Authors claim that the “empirical” elasticity framework is “model free”. My feeling is that the regression models for the empirical elasticities (e.g., eqs. 12 and 13) are equivalent to assuming a linear water-balance model. In other words, for constant eQP and eQE0, ΔQ = eQP*ΔP + eQE0*ΔE0 is equivalent to Q = const + eQP*P + eQE0*E0 (plus residual terms).

- It is not clear to me why GLS should be preferred to OLS. Is the advantage of GLS over OLS just in failing less often the Durbin-Watson test (Figure 11b)? Is this fact

HESSD
12, C1245–C1248, 2015

enough for discarding the much more straightforward OLS model? My feeling is that GLS is needed because the M-years anomalies are calculated with moving windows and, therefore, the strong correlation between the points has to be accounted for. Is it the case? If so, this should be discussed in the paper.

I hope that my concerns can help the Authors to improve the paper.

Detailed comments:

Page 3646, line 24: "Δ indicates the difference or change" from what? I guess from the long term mean value (p.s., the Authors define the Δ for their analyses at page 3652).

Page 3652, line 17: there is a typo in "to compute the relative rather than the relative elasticity".

Pages 3653-3654, Section 3.3.2: I would suggest to add literature references for the bootstrap significance test for GLS.

Page 3657, Section 4.2: it is unclear how the Turc-Mezentsev formula has been fitted to the data. Has the parameter n been calibrated? If so how? And what values have been obtained for n (how different from 2.5)?

Page 3657, lines 10-12: actually I cannot see, in Figure 8, a link between the two elasticities even for the elasticity to precipitation. What I see is that the Turc-Mezentsev formula implies bounds at -1 and +1 for the elasticities.

Caption of Table 5. "Univariate" should be bivariate.

Figure 6: it would be better to use consistent scales for (a)-(b) and for (c)-(d).

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

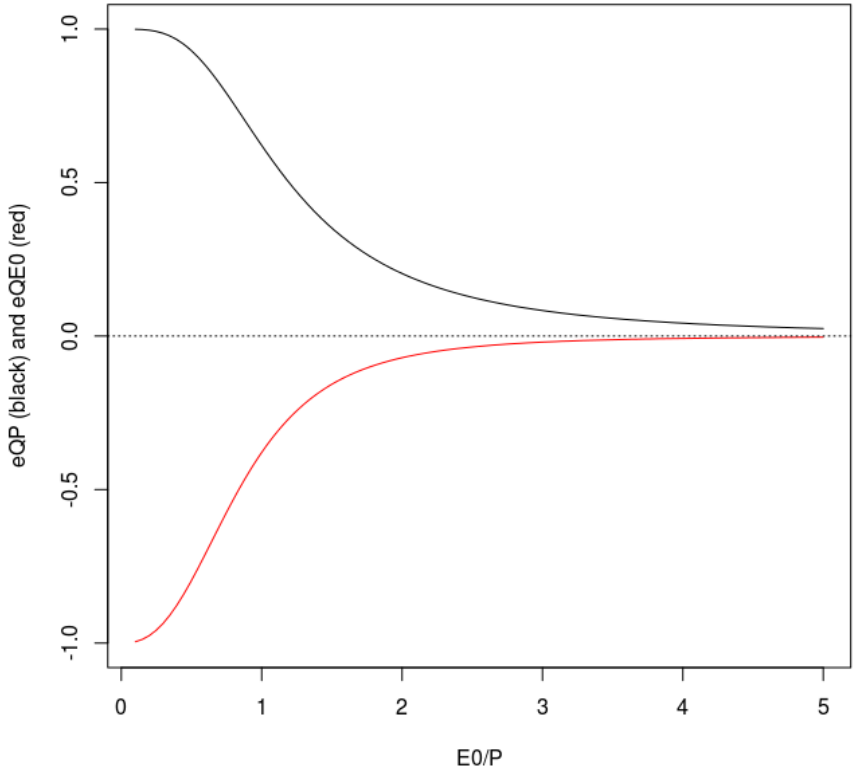

Fig. 1. Curves corresponding to Eq. (4), red, and Eq. (5), black, for the Turc-Mezentsev formula with n=2.5.

HESSD
12, C1245–C1248, 2015