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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.

V. Andréassian et al.

vazken.andreassian@irstea.fr

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We thank Alberto Viglione (AV) for his detailed review and relevant comments, which we will take into account in the revised version of the paper. We give here a rapid answer to the points raised:

1. Concerning the use of the Turc-Mezentsev (T-M) formula: AV underlines that because the T-M formula does explicitly include the effect of both P and PE, the result that we find (bivariate regression better than monovariate) is obvious. Let us clarify what was our aim with this exercise:

1.1. First of all we wanted an objective way to define how empirical elasticity should be computed;

1.2. But since there is no absolute reference, we needed a relative reference which would at the same time "behave as a real catchment" and allow for an explicit computation of elasticity. This is why we chose the T-M formula. Let's also add that it is the most widely used formula in elasticity-related litterature, so that it seemed natural to use it;

1.3. The fact that the T-M formula imposes a distinct and well-defined elasticity for Q vs PE and Q vs P reflects the "hydrological good sense". Thus, even if our synthetic experiment had an expected outcome, it does however provide a way to quantify what we gain by using the bivariate approach

As a partial conclusion, we do argue that indeed, using the T-M formula as the theoretical reference is a 'strong' assumption which explains that a bivariate solution to elasticity is better that a monovariate one. However, beyond reflecting the "hydrological good sense", it also reflects the visual impression from the graphs that we present in supplement. Moreover, we believe that the T-M formula's assumptions are likely to be shared by most hydrologists, and that the T-M formula is both extremely simple and widely-used, which should turn our demonstration acceptable to many of our readers. We thank AV for his suggestion to show graphically that the TM formula implies well defined elasticities at moderate aridity conditions, and we will use this graph in the revised version.

2. Concerning the advantage of using GLS regression:

As you mentioned it, GLS is needed in theory because the M-years anomalies are calculated with moving windows, which results in the strong correlation between the points. Our test with the synthetic data shows that it is indeed the best solution (but there is no revolution when comparing with OLS).

3. Concerning the claim that the empirical elasticity framework is “model free”:

We do agree that this is not entirely true, because a linear model... is a model. By “model free”, we wanted to underline the distinction which exists with most of the elasticity literature which deals with simulated data (i.e. before this linear model step, there is a full hydrological model). We will precise it in the revised version.

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