Antony, 11th July 2016

Dr Ralf Merz
c/o Editorial Office
Journal of Hydrology

Revised version of paper

Dear Dr Merz,

Please find attached the revised version of the paper entitled “Climate elasticity of streamflow revisited – an elasticity index based on long-term hydrometeorological records”.

We submit below the answers to the questions raised by the reviewers and the action taken by us, as well as a revised version. With these changes and the additions made on request of reviewers, we believe that the paper is now much clearer and ready for publication in HESS.

Looking forward to hearing from you, I remain

Yours sincerely

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ANSWER (in black) TO THE QUESTIONS RAISED BY REVIEWER #2 (in blue) WITH THE MOIDIFICATIONS TAKEN IN THE FINAL VERSION (in red)

SUGGESTED CLARIFICATIONS/ADDITIONS:

(1) I realized that the authors did not provide a reference for the considered streamflow data. For guaranteeing the reproducibility of the presented research I would consider it essential for having the data source specified precisely (e.g. through a reference, a URL to the data centre or by naming the institution holding the data).

In section 2, we had the following sentence: "Long series of continuous daily streamflow and precipitation were available over the 1976–2006 period."
It now reads:
"Long series of continuous daily streamflow and precipitation were available over the 1976–2006 period. They are freely accessible from the Hydro database at the following address: http://www.hydro.eaufrance.fr/."

(2) I highly appreciate the fact that the elasticity of all catchments is now provided in the Appendix. I do however, miss information on the geographical location (e.g. coordinates of the gauging stations), which would be essential for other researchers who would like to expand on the authors findings. In addition I would find it useful if longterm mean runoff, precipitation and potential evaporation could be provided, although I acknowledge that this might not be possible due to copyright restrictions on the data.

They are indeed copyright restrictions on the precipitation time series, but we are allowed to provide long-term average values. We added in Appendix 3 the following information for each catchment:

- Area (km²)
- X outlet in decimal degree
- Y outlet in decimal degree
- Long-term precip. P (mm/yr)
- Long-term potential evap. E (mm/yr)
- Long-term streamflow Q (mm/yr)

Note: all long-term values have been computed over the 1986-2006 time period

(3) There is no reference for SAFRAN. In addition I wonder if SAFRAN precipitation is statistically interpolated (which technique) or an atmospheric reanalysis (relying on a physics based model of the atmosphere).

SAFRAN is a reanalysis product for all its fields except for the precipitation field which is krigged in a quite complicated way (we added the reference to Le Moigne, 2002, which is the only available describing the process in detail).

(4) Line 115 – 120: I find the option to refer to the discussion paper for comparison with another Epot parametrisation confusing. I therefore suggest

to either (i) omit this discussion completely form the paper or (ii) provide the necessary details as supplementary information for the article.

The original sentence read:

"As far as potential evaporation data is concerned, we used the Penman-Shutlleworth equation (Shuttleworth, 1993). Note that tests implemented with the classical Penman-Monteith reference evapotranspiration equation showed little difference (they can be found in the discussion version of this paper), but we preferred to switch to the Penman-Sutlleworth potential evapotranspiration formula because Donohue et al. (2010) suggested that it was the most appropriate form of atmospheric evaporation demand when considering a changing climate."

We simplified it for clarity at follows:

"As far as potential evaporation data is concerned, we used the Penman-Shutlleworth equation (Shuttleworth, 1993) because Donohue et al. (2010) suggested that it was the most appropriate form of *atmospheric evaporation demand* when considering a changing climate".

(5) Figures 8 and 10: Please explain the colour scales (uncertainty) of the dots in sufficient detail. I suspect they indicate the relative width of the bootstrap confidence intervals but I am not sure after reading.

Indeed, the color refers to the relative width of the bootstrap confidence interval. The legend was modified and now reads

"width of the bootstrap 95% CI"

In the caption of Fig. 8 we added:

The points are coloured according to the width of the 95% bootstrap confidence intervals on the empirical elasticity estimate.

In the caption of Fig. 10 we added:

The points are coloured according to the width of the 95% bootstrap confidence intervals on the GLS2 elasticity estimate

(6) Figure 6: paired sub-panels a&b and c&d should have Ideally the same colour scale.

Done

(7) Figure 11: Incomplete caption. Panels c,d,e,f are not described.

A previous version of Figure 11 had been left in the manuscript. We updated the figure and its caption.