Comments/Text of anonymous reviewer 2 posted in black, our text in blue with additions to existing text in red.

Review comments on “Elasticity curves describe streamflow sensitivity to precipitation across the entire flow distribution” by Anderson et al.

General Comments

This manuscript proposes a new concept in which streamflow elasticity is estimated across the full range of streamflow percentiles in a large-sample context, which is called “elasticity curve” by authors. The aim is to develop a more complete depiction of how streamflow responds to precipitation. They find three different elasticity curve types which characterize this relationship at the annual and seasonal timescales in the USA, based on two statistical modelling approaches, a panel regression which facilitates causal inference and a single catchment model which allows for consideration of static attributes. The idea is novelty and fits well with aims and objectives of HESS. This was why I accepted the review invitation. However, there are significant shortcomings in current version so that I have to recommend a rejection (below specific comments for detail).

We thank the reviewer for taking the time to read the manuscript and for clearly articulating their concern with our work. When reading their responses and criticism, we realized that the methodology was not explained well enough, which has led to major misunderstandings about our approach. Principally, the way in which Reviewer 2 has interpreted equation 2 in the manuscript is incorrect and does not represent how we have calculated elasticity for individual streamflow percentiles.

In fact, the particular way in which they have misinterpreted the method matches a point which Reviewer 1 suggested might be misunderstood. In their separate comment, Reviewer 1 stated: “I feel there is a strong possibility of readers misunderstanding the method. Specifically, the focus on different flow percentiles (or ranges of percentiles) may lead readers to believe that the method only focusses on precipitation that falls during the relevant percentile/range.” This is precisely what has happened here. To clarify, mean daily precipitation and mean daily potential evaporation (calculated either annually or seasonally) is used for **all streamflow percentiles**, allowing us to develop a picture of how streamflow in different periods is affected by changes in the total precipitation. For this reason, we feel that this is a result of a confusingly worded description of the method in our manuscript and propose one primary change based on their comments.

Our proposal is to add the following clarifying statement, and to include similar examples throughout the manuscript, in order to make this clear: “As presented in this study, the elasticity curve characterizes the sensitivity of different percentiles of annual and seasonal streamflow to changes in the average annual or seasonal precipitation. For example, an elasticity of 0.5 for the 15th percentile of annual streamflow would indicate that a 1% change in the overall mean annual precipitation would correspond to a 0.5% change in the 15th percentile of annual flow.”

In addition, we will adjust the text describing equation 2, so that the description of the model parameters is less ambiguous. This text is included in our response to the specific comment below. We completely understand that new methodologies can be difficult and confusing, so deeply appreciate the clarity with which Reviewer 2 has presented their concerns, as this affords us the opportunity to improve how we present our novel and useful concept.

We address each of the reviewer's specific concerns below in greater detail, and hope we have sufficiently clarified the misunderstanding so that the manuscript can be more fairly reviewed.

We have split this into 2 responses because there are 2 main critiques: 1. That we have misunderstood the concept of elasticity, and 2. concerns about the temporal distribution of the climate variables as they relate to equation 2 in the manuscript.

Specific Comments

First of all, authors clearly misunderstand the concept of elasticity precipitation of streamflow proposed by Schaake (1990) and Sankarasubramanian et al. (2001). The original formula is as:

$$\varepsilon_p(P, Q) = \frac{dQ/Q}{dP/P} = \frac{dQ}{dP}\frac{P}{Q} \tag{1}$$

However, the difficulty with this elasticity is that we never really know dQ/dP, which is often estimated from a hydrological model and, of course, the form of the hydrological model is always unknown and validation of such a model remains a fundamental challenge (Sankarasubramanian et al. 2001; Fu et al., 2007).

In order to solve this problem, Sankarasubramanian et al. [2001] introduced a specific case of (1) at the mean value of the climatic variable:

$$\varepsilon_p(\mu_p, \mu_Q) = \frac{dQ}{dP}\Big|_{P=\mu_P}\frac{\mu_P}{\mu_Q} \tag{2}$$

They (Sankarasubramanian et al. 2001 further verified that the non-parametric estimator:

$$e_P = median\left(\frac{Q_t - \overline{Q}}{P_t - \overline{P}}\frac{\overline{P}}{\overline{Q}}\right) \tag{3}$$

is a robust estimator of the precipitation elasticity of streamflow for a wide class of hydrological models that does not depend on the form of the hydrological model. This is the formula that has been wildly used in the literature to estimate the precipitation elasticity of streamflow. That is to say, the elasticity is the median value of ratio of annual streamflow anomaly in terms of long-term means to precipitation anomaly, not the long-term mean for the 50th percentile of streamflow as author claimed.

I do understand that there are some exceptions in the literature not to take this median value. For example, the two-parameter elasticity to include temperature (Fu et al., 2007) is to plot every annual ratio is plotted in a 2-d space or fitted a linear regression with these two anomalies (Zheng et al., 2009).

$$e_{P,\delta T} = \left(\frac{Q_{P,\delta T} - \overline{Q}}{P_{P,\delta T} - \overline{P}} \frac{\overline{P}}{\overline{Q}} \right) \quad (4)$$

$$\Delta Q_i / \overline{Q} = \varepsilon \cdot \Delta X_i / \overline{X}. \quad (8)$$

We thank the reviewer for this detailed comment and explanation. We are familiar with this section of the literature on elasticity in hydrology. However, after revisiting the Sankarasubramanian et al. (2001) paper, we acknowledge that they do, in fact, estimate elasticity relative to the annual mean, rather than the median, as the reviewer points out and that the median of the ratio is taken, rather than the mean, as we originally stated. The second half of this (taking the median of the ratio, rather than the mean), was an error on our part. However, respective to the first half of this, we were explicitly referencing the elasticity of the median flow here and thus used a slightly different notation to do so. We acknowledge that this choice could have been expressed, and cited, more clearly. Regardless, we feel that this point is of little relevance to the method which is applied in the manuscript and do not believe that it implies that we have failed to understand the definition of elasticity, as the reviewer suggests.

What is of greater relevance is the abundance of literature in recent years which uses alternative methodologies, in particular, bivariate and multivariate regression-based approaches, to estimate elasticity in hydrology (for example: Andréassian et al., 2016; Bassiouni et al., 2016; Cooper et al., 2018; Potter et al., 2011; Tsai, 2017; Zhang et al., 2022, 2023). These types of approaches are often functionally equivalent (e.g. Cooper et al., 2018), or achieve a similar, or often better, result to the reference methods which are mentioned by the reviewer, as is demonstrated in Andréassian et al., 2016. Our approach, definition, and application to calculating the percentile-specific point estimates is consistent with hydrological literature in the past 15 years, as well as with the abundance of broader literature on the concept, in addition to presenting some novel additions. For the above cited reasons, we feel that this critique is of minor significance. We propose a simple modification to the manuscript in order to correct our mistake.

We have concluded that retaining the original formal definition does not add value to the paper and propose removing equation (1), where we briefly describe the Schaake (1990) and Sankarasubramanian et al. (2001) definition of elasticity, from the paper. We will change that section to read:

Historically, streamflow elasticity has been estimated using a reference approach as proposed initially by Schaake (1990) and further developed into a nonparametric estimator by Sankarasubramanian et al. (2001), in which elasticity is expressed as the median of the ratio of the annual streamflow anomaly to precipitation anomaly, relative to the long term mean. Many recent studies have instead relied on the coefficients from multivariate regression models, such as generalized and ordinary least squares regression (Andréassian et al., 2016; Potter et al., 2011), or regionally-constructed panel regression models (Bassiouni et al., 2016), to estimate elasticity. These types of approaches are often functionally equivalent (e.g. Cooper et al., 2018), or achieve a similar, or often better, result to the reference approaches, as is demonstrated in Andréassian et al., 2016. The benefits of regression-based approaches include simultaneous estimation of sensitivity to potential evaporation and precipitation, accounting for co-variation in these phenomena and providing a more robust estimate of elasticity (Andréassian et al., 2016). Probabilistic statistical tools also enable straightforward calculation of confidence intervals.

My main scientific concern is Eq 2 of the manuscript, which is the base of this study. This does not make any scientific sense, because the same percentile of streamflow and precipitation could happen in different time of year. For example, 95th percentile of streamflow is located in June and 95th percentiles of precipitation/PET could be in December.

How possible to build a regression model between them?

$$\ln(Q^q_{i,t}) = \alpha_{i,t} + \varepsilon^q_P \ln(P_{i,t}) + \varepsilon^q_E \ln(E_{i,t}) + \eta^q_{i,t}$$

In addition, this approach requires non-zero daily streamflow for the entire study period, i.e., it cannot be applied to ephemeral rivers and streams, which limits its applications. I am surprised that it includes some rivers in Nevada and Arizona states where the number of rainfall days in a year is only 30-60 days. How can it result in a non-zero streamflow days?

We completely agree that this approach would be problematic and scientifically invalid. Fortunately, this is not what we have done!

As it is currently written, in the original submission of the manuscript, this is the text beneath equation 2 (cited above):

> "where $Q^q_{i,t}$ is the natural logarithm of a streamflow percentile (q) calculated for time period (t) for catchment (i), $\alpha_{i,t}$ is the intercept, ln($P_{i,t}$) is the logarithm of catchment averaged daily precipitation, and ln($E_{i,t}$) is the logarithm of catchment averaged daily potential evaporation. The point estimate of precipitation elasticity is represented by the regression coefficient: ε^q_P and potential evaporation elasticity is represented by ε^q_E The error term is $\eta^q_{i,t}$."

A close examination of the text and the associated equation will reveal that the superscript "q" is not present for the variables ln($P_{i,t}$) or ln($E_{i,t}$). This indicates that the variables are not specific percentiles, but rather the "catchment averaged daily [variable]" as described in the text.

However, we acknowledge that the wording here was ambiguous and can completely understand why this would be confusing. To rectify this, we propose adding the following to the text (red) in the interest of clarifying this:

> "where $Q^q_{i,t}$ is the natural logarithm of a streamflow percentile (q) calculated for time period (t) for catchment (i), $\alpha_{i,t}$ is the intercept, ln($P_{i,t}$) is the logarithm of catchment averaged mean daily precipitation for the time period of interest (year or season), and ln($E_{i,t}$) is the logarithm of catchment averaged mean daily potential evaporation in that period. Note that mean seasonal and annual climate time series are used, not percentiles equivalent to the streamflow percentile of interest (denoted with the superscript "q"). The point estimate of

precipitation elasticity is represented by the regression coefficient: ε_P^q and potential evaporation elasticity is represented by ε_E^q The error term is $\eta_{i,t}^q$."

In addition, we will include clarifying examples as suggested by Reviewer 1 and described in the overview of changes above.

Reference

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