General comment

This study analyses the streamflow elasticities (considered as sensitivities) to climate described as observed percentage changes in river flow per percentage change of a climate driver. The autors have used a large hydrometeorological dataset to draw data from over 8,000 catchments, and provide a pan-European quantification of elasticities of annual mean and extreme streamflow to precipitation and temperature. In a second part, they intend to analyze the dependence of the elasticities to precipitation in using a random forest model with 20 climate and catchment factors to explain regional difference in elasticity value.
The objective is to demonstrate that such empirical strategy advances understanding of hydrological resilience of stream flows to climate change highlighting amplified and dampened streamflow response to climate which can ultimately support water management and disaster risk mitigation across Europe.

I believe that the manuscript has some potential, but additional work is required to put it at the standard required by the Journal of Hydrology and Earth System Sciences and it should be rejected in the current form. The datasets are not described in sufficient detail, the evaluation is limited, and the overall quality of the paper is affected by several shortcomings. I summarize my comments below in the hope that they will help the authors prepare an improved version of the manuscript.

Major comments:

1) Dataset and pre-processing.

- The authors use the comprehensive large-scale EStream dataset (do Nascimento et al., 2024), but they do not properly introduce it in the article, even though information about the sources of the hydrometeorological variables is crucial. For example, if EStream relies on ERA5 reanalysis for precipitation (as does the CARAVAN dataset cited by the authors), this would introduce limitations, since it is partly based on a model.
- The authors appear to have removed catchments with NaN values in either of the two seasons (L155) only for the seasonal dominance analysis. I recommend excluding these catchments from the entire analysis, as their annual elasticities may reflect only one season due to missing data.
- The methodology for minimum flow should be applied more cautiously, as the authors report several basins with instrumental errors (Q = 0 for multiple dates). Screening these basins would help improve the analysis. In addition, presenting the distributions of Qmean, Qmax, and Qmin in the supplementary material would better support the claim that Qmin is not biased by erroneous zero values.

2) Flaws in the method.

- The author introduce the estimation of elasticities with too few reference from the literature and without supporting their choice in the chosen formula compare to others (Andréassian et al. 2016).
- Equation 1 defines the elasticities. From my understanding, the elasticity to P (or T) for the extreme flows Qmin and Qmax is also related to the annual mean of P. If this is the case, the interpretation of Fig. 3 (L266–277) remains unclear to me. Since $P_{mean} \approx Q_{mean} \times \varepsilon_{mean} \approx Q_{max} \times \varepsilon_{max}$, it follows directly from $Q_{mean} < Q_{max}$ that $\varepsilon_{mean} < \varepsilon_{max}$.
- It is unclear why and how the authors have proceed with temperature “using the absolute annual mean of temperature”. Does it means absolute temperature is used in Equation (1) ? Then the absolute operator should be used (also T is not introduced in this Eq.).
- Without normalization of T, the regression uses one normalized variable (P) and one non-normalized variable (T), so the elasticities with respect to P and T are not directly comparable. This represents a

limitation of the analysis: the authors rely on a bivariate approach to estimate elasticities, but do not (and cannot) jointly evaluate these parameters.

- The authors chose to first estimate elasticities using a linear model (Eq. 1), and then to relate these elasticities to catchment attributes with a nonlinear model (random forest) in order to derive feature importance (Fig. 7). However, while their nonlinear model does not accurately reproduce the elasticities ($R^2 < 0.52$), they still rely on it for interpreting feature importance. It rather uncommon to trust feature importance derived from a model that fails to explain more than half of the spatial variance. Furthermore, they retained climate-related attributes such as aridity in the model; aridity emerges as the most important predictor, yet it is directly linked to P, from which the elasticity is defined. This introduces a circularity in the methodology that weakens the support for their conclusions. I would recommend not relying on inaccurate nonlinear modeling and instead keeping the analysis at the linear level, avoiding the inclusion of nested variables in the modeling framework.

3) Limitation in the Analysis.

- Several figures are not discussed, including all PDFs in Figures 2, 4, and 6, and it is not specified whether the vertical bars represent means or medians.
- Figure 2 is interpreted mainly through visual inspection of spatial patterns in relation to other catchment attributes that are not shown in the main text but only in the supplementary material. Subsurface properties appear to play an important role, yet no figure directly supports this. I suggest providing quantitative metrics and additional figures to substantiate these claims.
- The strategy for selecting catchment attributes included in the random forest model is not described, and the metrics are not specified (for example, what is the VIF mentioned in L176?).
- The concept of feature importance in the random forest, and what exactly it represents in this context, is not explained in the article.

4) Unclear presentation

- The elasticities to precipitation use various and different notations through the paper (in the abstract, Eq.1, Fig.2 and in the text) that must be harmonized.
- Seasonal dominance is introduced but the notation is not used in figures or in the maintext.
- Significancy of the elasticity cannot be read from the Figs. 2, 4,6. And are not discussed either.

Specific comments:

- l70 : This “non-parametric approach”. The term here is irrelevant as linear regression is a parametric approach.
- Importance of Fig. 1 is not obvious and its deleting could save place.
- The notion of “seasonal scale” arises lately in the paper (L135), such finer grain of analysis should be better emphasised in the Abstract and Introduction of the paper.
- The notion of p-values in L145 is not clear, how it is computed?
- Table 1 : human influence: Mean area equipped for irrigation: what 10/5-year resolution means?
- Fig. 3 : the “binning in group of 2%” is not clear, is it the range of variation of elasticity inside each bin ?
-Fig4: Pdf are note introduced in caption.