I am very grateful to Referee #1 for having carefully read the manuscript and for the insightful and constructive comments, which will certainly help to improve the work.

I report below my replies to both the Referee's general and specific comments.

General Comments

Comment 1) • What are the advantages of using a biased estimate of $Q_{T=2yr}$? […]

I do thank the Referee for having pointed out the importance to better underline in the paper the limitations and advantages of the proposed approach, and in particular with respect to a probabilistic one.

It is certainly true that a probabilistic approach (it is possible to find examples also for neural network models, see for example Khosravi et al., 2011) may be able to add very valuable insights for a more complete evaluation of the prediction model, supplementing the information provided by point-value predictions, and in the future I intend to attempt to investigate the uncertainty of the issued predictions, (as I will add in the concluding session in the revised version) but I do not believe it would be possible performing such complex analysis here. It should in fact be considered that uncertainty assessment methods should take into account all error sources (see for example Montanari, 2007) and not only those related to the choice of their parameters (the majority of the uncertainty methods deal only with a single source of uncertainty, for instance, Monte Carlo-based methods analyze the propagation of uncertainty of parameters only) - and are subject, as well as the prediction model itself, to errors in their underlying assumptions and structure as well as in the determination of their parameters (Xiong et al., 2009) so that it is needed, even if it is far from easy, to correctly evaluate also their quality (in many methodologies it is hypothesised and not verified if the distribution of the forecasts is the real one). As a consequence, implementing a correct, fully comprehensive procedure for a consistent and reliable estimation of the global uncertainty is certainly not straightforward (nor it would be possible to describe it briefly) and this is why it may be subject of a separate, future work.

On the other hand, I do not believe it is necessary to add the implementation of a probabilistic framework here, since the presented methodology is a deterministic one, where an optimal point forecast is obtained by minimizing the conditional expectation of the future loss.

Such framework has not the pros of a probabilistic one in terms of quantification of the uncertainty, but its advantage is the operational value of the forecast in terms of an optimal decision that minimizes the cost; in fact asymmetric loss functions are more appropriate in many types of decision settings, as shown by recent forecasting literature analysing the statistical properties of optimal predictions under asymmetric loss (e.g. Christoffersen and Diebold, 1997, Granger and Pesaran, 2000, Patton and Timmermann, 2004; and in particular Zellner, 1986, 2004, showed that once the symmetric loss function is abandoned, optimal forecasts need not be unbiased) and showing that in many " practical applications, asymmetric loss functions can be critical to effective forecasting" (Elliott et al., 2006).

Minimising the asymmetric error function has the purpose of minimizing the cost, thus optimizing the threshold from an operational point of view. A probabilistic forecasting approach applied to the symmetric error function (provided that the methodology is able to include all sources of uncertainty and its quality may be objectively assessed/verified) would certainly provide awareness on the uncertainties associated with the point forecasts, but identifying the upper (e.g. 95%) uncertainty bound would not allow the decision-maker to choose the optimal value for the threshold in terms of costs/operational utility, since such value (upper bound) would be (if reliable) the one that identifies an assigned risk of underestimation (and, even if this is not the point here, it would, I expect, result in a very high value for a small assigned risk, given the large uncertainty of the approach, mainly due to the intrinsic limitations and shortcomings of the data set for such an heterogeneous area…) but it would not take into account in any way the overestimation costs resulting from high negative errors, nor it would consider the balance between the costs of positive and negative errors, as it is, instead, done within the proposed approach.

In the revised version I will better explicit the purpose and usefulness of the proposed approach, along with considerations on the advantages/disadvantages in respect to using the upper bound resulting from a probabilistic framework.

Comment 2) • Regional Flood Frequency analysis is not regression. In a couple of locations in the text, page 6014 line 14–29 and page 6030 lines 10–18, there seems that there is the direct association between Regional Flood Frequency analysis to Regression with catchment attributes (regression or related techniques like ANN's). […]

In the revised version I will certainly rephrase ll. 24-27 p. 6014 and ll. 13-14 p. 6030, since I definitely did not mean to reduce Regional Flood Frequency Analysis to the application of regression techniques, but only to refer to that thematic area, because the runoff threshold literature generally does not include these issues. I fully agree (as highlighted, as the Referee underlines, also in the chapter on floods prediction that I co-authored of the 2013 book) that regression methodologies are only one of the possible methods (statistical and process-based) to predict floods in ungauged basins and in particular I should better specify in the text that their use is especially frequent only as far as the estimation of the index flood values is concerned.

Comment 3) • Relative error could be also very valuable. For assessing the performance of several variants of the proposed method, the measures MAE and RMSE are proposed, both functions of the error. Given the large range of discharges considered in the study, it could be also very valuable to report additionally boxplots of the relative errors. […]

I definitely agree that a more comprehensive description of the errors would be very helpful to interpret the results, especially given the large discharge range, as underlined by the Referee; I am not sure the relative errors would be the fairest way to analyse the results in the presented decision setting framework (see reply to first Comment), given that the costs are weighted in respect to the 'not-relative' errors in the loss functions, so I would prefer, if the Referees agrees, adding in the revised paper the scatterplots of observations/predictions, that I believe allow the most complete visualization of the results over the entire discharge range, showing every single prediction in respect to the corresponding observation.

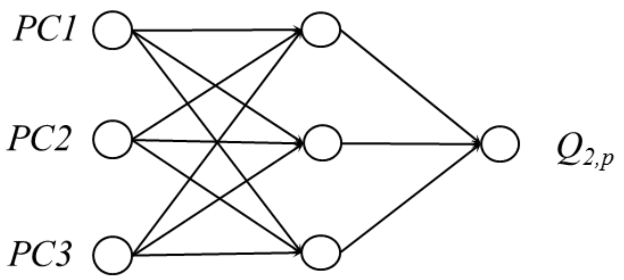
Detailed Comments

Page 6013, Line 27: is "real-world" here "real-time"?
Actually, I mean both: 'real-time' warning systems actually implemented by 'real-world' organization (that is not only in literature simulation studies): I will rephrase to make it clearer.

Page 6017, Lines 17–19: The author defines here the error as the observed minus the predicted value. To my knowledge, in runoff prediction in ungauged basins, it is almost a consensus to define error as predicted minus observed. If the author wants to define it here inversely, a stronger warning to the reader should be given, in order to avoid confusions.

I used the notation by Elliot et al (2005), for consistency with their definition of the loss function in Eq 1; in order to better warn the hydrologist readers, I will add the equation:
$\varepsilon = O - P$

Pages 6021–6023: Maybe adding an schematic figure with the structure of the selected ANN could help the reader.
I fully agree with your suggestion: I will add a figure showing the ANN architecture:



Page 6027, line 4: is here "scour" the Q2?
Page 6029, line 7: ... the errors <u>are</u> not negligible...
Thank you, I will amend these mistakes.

Page 6027, line 9: I think "prudence" is not the right word here. Maybe "tendency to over/underestimate"?
Page 6031, line 13: Again, "prudentially" is not the right word here.
I will rephrase both sentences as suggested.

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