RC: 'Comment on hess-2022-264', Anonymous Referee #3, 27 Feb 2023
Projection of the streamflow changes is vital for the climate adaptation and mitigation. Simon Richard and co-authors propose a new modeling workflow to provide streamflow projections without resort to the usual step of bias correction of climate projections. I went through the precious two anonymous referees' comments and read the manuscript carefully. In general, I agree with the reviewer #2. The topic is important, but the current manuscript is not strong enough. My comments are below:

1.The author pointed out numerous weaknesses of the traditional approach including the " the statistical post-processing of climate model outputs may disrupt the physical consistency between the simulated climate variables and even alter the corresponding trends", " the modelling work flow is relying highly on the availability and quality of meteorological observations" and " it also requires a high level of expertise and computing capacities to postprocess the outputs and uses non-trivial statistical methods".

However, whether these weaknesses can be solved by the new approach is doubtful. For example, the new approach still needs model calibration, hydrological model simulation and projection, and the construction of hydrologic scenarios using quantile mapping and correction. It still requires a high level of expertise and computing capacities to postprocess.

We get the point. We all agree on the drawbacks and limitations affecting traditional hydroclimatic modelling approaches involving bias correction/post-processing of raw climate model outputs, namely the disruption of physical consistency, a strong requirement for observations and a high (still increasing) level of complexity. The latter are well documented in the scientific literature. We also fully agree with Reviewer #3 that the proposed asynchronous framework does not completely solve all the weaknesses of the traditional approach. We rather introduced the framework as a complementary analysis tool that can provide a sound alternative to impact modelers considering that (1) it preserves the physical consistency of climate model outputs (although it still requires the calibration of a hydrologic model, discussed below), (2) it requires no meteorological observations (a significant benefit since most of the earth system is affected by data scarcity), and (3) it involves less modelling processes (post-processing is exclusively applied to streamflow instead of numerous climate variables such as precipitation, air temperature, but also air humidity, radiations and wind speed, in some cases). We also discussed how impact modellers should ponder the use of the proposed framework weighting

corresponding drawbacks and benefits, within the scope and aims of a given study. Ultimately, we believe that while our approach requires further test and validation, it is innovative, relevant to the scientific community and worthy of publication. We remain fully available to discuss these issues with Reviewer #3. Comments and notifications have been added to the manuscript [lines 453-464] to summarize the above arguments.

Moreover, the author complain that traditional approach needs bias-correction before hydrological simulation, while the new method does not. I am confused that, why we cannot directly calibrate the hydrological model using the raw climate output from GCM/RCM, and then use the future meteorological forcings to perform hydrological modeling? By doing this, the systematic errors in meteorological forcings are also bias-corrected (during the hydrological model calibration) and no additional meteorological observations are needed.

We indeed calibrate the hydrologic model using the raw climate output from RCM over the reference period. We also subsequently use the "future" raw RCM forcings to perform hydrological modeling (using a model calibrated over the reference period), thus projecting the future hydrologic response corresponding to raw RCM forcings. At anytime, however, we correct/post-process systematic errors in RCM forcings. We could then consider that the calibration of the hydrologic model corrects systematic errors of the resulting simulated hydrologic response, but not the raw RCM forcings, although both are obviously related. We hope these clarifications are valuables to Reviewer #3.

In addition, the third comment of Reviewer #2 is not well answered, leading to a doubt that "whether the new method can conserve the corresponding trends"

To our knowledge, two approaches help preserve the physical consistency of climate model outputs and their trends: to apply trend-preserving multivariate methods or to use raw model outputs straightforwardly for impact analyses, accepting biases. Our proposed framework is based on calibrating a hydrologic model using of raw model outputs, assuming a consistent relative change (within climate simulations) from the reference to the future period. We acknowledged, in the manuscript, the requirement for calibration as a limitation of to the proposed framework, considering that it may disrupt the consistency of simulated hydrologic processes at the catchments scale. We also acknowledged that we do not know yet to which extent parametric compensation (resulting from calibration) affects the trends of the projected hydrologic responses, but we identified this issue as a key question for further

research. However, our approach is based on the use of raw climate model outputs. We have verified that corresponding trends encrypted within climate model simulations are conserved. A comment has been added to the discussion [lines 432-438] to summarize the above arguments.

2.The method is still somewhat confusing, especially for the readers who are not familiar with the “asynchronous modelling” and the reason for “using an objective function that does not consider the temporal correlation”. In addition, “let the hydrological model to run in an asynchronous fashion, considering that the same hydrologic model is expected to behave in a different way (the response to Reviewer #2)” also confused me. If the hydrological model behave differently in asynchronous fashions, the can the physical mechanism of the hydrological model be ensured? I appreciate the reference the author provide, but I still suggest to give a brief introduction to make it more readable.

The proposed asynchronous modelling framework has been previously explored by the authors. The two papers mainly focused on exploring different types of asynchronous objective-function and on the modelling framework that could be applied to implement more complex physically based description of hydrological processes, considering the scarcity meteorological fields such as air humidity, radiation, and wind speed. An asynchronous objective-function is equivalent to a signature-based calibration metric in the way it is designed. Both criterions aim to identify parametric solutions by optimizing the asynchronous statistical properties of the simulated hydrograph such as means (annual, seasonal or monthly), variance and quantiles, capturing the broad hydrologic behavior of a catchment instead of the precise sequence of hydrometeorological events observed at the catchment outlet. However, the purpose of asynchronous modelling is different from signature-based modelling. Based on the assumption that streamflow regime is a functional proxy of the corresponding forcing climate system, asynchronous modelling proposes a specific reconfiguration of the conventional hydroclimatic modelling chain, circumventing the (double, potentially redundant) requirement for meteorological observations typically used for post-processing raw climate model outputs and calibrating the hydrologic model.

Since climate models are not designed to simulate the observed sequence of meteorological events, we expect the resulting simulated hydrologic response to also be out-of-phase relative to streamflow observations. We thus expect the use correlation-based calibration metrics (such as NSE, KGE) to mislead the identification of sound and representative calibrated parameters within the asynchronous modelling framework. A paragraph has been added to the

manuscript [lines 146- 153, see also 161- 163]. Finally, we believe it is acceptable for two parametric solutions, issued by two distinct modelling frameworks, to differ in their corresponding simulated hydrologic response.

3.Another issue I am concerned is that, the comparison between the new and traditional methods (Figure 8 and Table 4). Here, the author said that the new method tend to have smaller uncertainties, but I note that the samples are different from the new and traditional approaches during the comparison. It is meaningful? In addition, some samples have large uncertainties in new method (e.g., crx4 in Site 4) but do not occur in traditional approach. Is this not the advantage of the traditional approach? Moreover, given that the new method shows clear weakness in representing the streamflow seasonal cycle compared to the traditional method, whether it is better in the future projection is doubtful. I do not think a lower uncertainties necessarily indicate a better projection especially when we do not have observations and its lower performance during historical period.

We wish to clarify that we did not explicitly refer to the notion of uncertainty when describing and discussing Figure 8 and Table 4. We rather described the distributions of the projected change values, using the standard deviation to describe the spread of the corresponding distributions. We would argue that the spread affecting projected change values should theoretically include both uncertainties related the implementation of the modelling change and the naturally variability of the hydroclimatic system. We agree with Reviewer #3 that lower uncertainties do not necessarily translate into better projections. Also, we did not conduct a formal analysis comparing statistical distributions, we rather described how an impact modeler would interpret and communicate the projected change of the hydrologic regime. Our main intention was to determine if the corresponding conclusions would differ from a modelling framework to another. The proposed analysis is thus rated based on expert-based judgment rather than statistical significance.

Outlying change values are an obvious drawback of the proposed asynchronous modelling framework. We agree with Reviewer #3 (as discussed in the manuscript) that this drawback should be carefully taken into account using the asynchronous framework. We however demonstrated that the outlying change values are related to a poor representation of the annual hydrograph simulated over the reference period. In the manuscript, corresponding climate simulations have been formally excluded of the impact analysis, without affecting conclusions. For these reasons, we would not fully agree with Reviewer #3 that weaknesses in representing the streamflow seasonal cycle

affect the reliability of the projected change values issued by the asynchronous framework. Note that outlying change values can also be generated using the conventional approach (Appendix B).

4.On the "quantile perturbation". Figure 10 shows the future projections based on quantile perturbation. I think it is reasonable using this method when the hydrological regimes remain stable in the future. However, the "tipping points" has been received much attention in recent years which is an important issue in future projection. Is the "quantile perturbation" suitable when the hydrological regimes changes? For example, a shift in seasonal cycle?

We acknowledge that the quantile perturbation assumes a comparison between two stationary periods (reference vs future) and does not consider potential rupture in future trends. We believe shifts in the seasonal cycle could theoretically be more precisely assessed by applying sub-annual (monthly) perturbation factors. The point raised by Reviewer #3 highlights the fact that the asynchronous approach, as designed and presented in the manuscript, could rather be considered as an (hybrid-like) approach to assess vulnerability of water resource systems, instead of a pure top-down predictive assessment tool. Comments have been added to the discussion [lines 501-503 and 541-543].