Towards a semi-asynchronous method for hydrological modeling in climate change studies

Frédéric Talbot[1], Simon Ricard[3], Jean-Daniel Sylvain[2], Guillaume Drolet[2], Annie Poulin[1], Jean-Luc Martel[1], Richard Arsenault[1]

[1] Hydrology, Climate and Climate Change Laboratory, École de technologie supérieure, Université du Québec, Montréal, H3C 1K3, Canada
[2] Direction de la recherche forestière, Ministère des Ressources naturelles et des Forêts, Québec, G1P 3W8, Canada
[3]Sciences and Engineering, Université Laval, Québec, G1V 0A6, Canada

Correspondence to: Frédéric Talbot (frederic.talbot.2@ens.etsmtl.ca)

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Dear Editor,

The manuscript was well taken care -off and is tidy. However, I think the justification of the used metric for calibrating the hydrological model and not investigating its effect on the outcome I see as major shortcomings of this manuscript.

Review “Towards a semi-asynchronous method for hydrological modeling in climate change studies”

General

The manuscript compares two methods for deriving climate discharge scenarios. One with model calibration based on observed meteorological data + bias correction (conventional) and the other without bias correction and a hydrological model trained on climate model output (asynchronous) and optimized on statical metrics. Its compares the performance of the conventional and asynchronous method in simulating hydrological processes under both current and future climate condition.

We sincerely appreciate RC2 time and effort in reviewing our manuscript. We are glad that RC2 considers that our text was carefully prepared and is well-organized. We have indeed spent much time thinking about how to present our work in the most complete and understandable manner. We appreciate RC2 constructive feedback and acknowledge the concern regarding the justification of the calibration metric and its potential impact on the study’s outcomes. Below, we provide a detailed response and outline the revisions we will make to address this issue.

Main comments

In general the manuscript reads well and setup of the research is clear and and well described. However, It stays unclear what the semi-aynschronous approach is.

The manuscript basically investigates both approaches which makes the additional value (to the available literature) unclear. The main conclusions are defined by the choice of the optimization metric (Kling KGE in this conventional case) and the selected stat. metric (RMSE of the sorted discharges) in the asynchronous method. The choice of this metric is not supported by any reasoning/evidence while it is clear this defines the results and any conclusion. It would be relatively easy to define and include more (better?) statistical metrics (also to make it fairer in comparison with the metric for the conventional approach) that take into account timing, magnitude etc. This needs much more attention/work in the manuscript. Why was this metric selected and not other ones?

We thank the reviewer for this comment, as it reveals that we did not provide a clear enough explanation of the method. The asynchronous method is a relatively new addition to the hydrological modeling field, first introduced in 2019. While it has been applied in a few studies to assess the impacts of climate change, a systematic hydrological variables comparison with the widely used conventional approach, using a physically-based hydrological model, had not been undertaken prior to this work. This study provides a novel contribution by focusing on the representation of hydrological variables under the asynchronous method and cautioning researchers and practitioners about its current limitations. Specifically, while the asynchronous method may achieve seemingly accurate streamflow results, the mechanisms underlying these results often fail to align with hydrological processes, leading to inconsistent representation of variables like snowmelt, which reduces the level of confidence that can be attributed to it under future climate scenarios. These findings underscore the need for significant improvements to the method to make it reliable for climate change impact assessments. Furthermore, the analysis of intermediate variables highlights the issue of synchronicity, which is currently overlooked by the asynchronous method. This underscores the necessity of introducing an objective function that incorporates the concept of synchronicity, thereby paving the way for the development of a semi-synchronous approach. The semi-synchronous approach, as introduced in the discussion section, would retain the distributional benefits of the asynchronous method while addressing the timing discrepancies by aligning hydrological processes more closely with observed seasonal dynamics.

For the comment regarding the calibration metrics, for the conventional method, we employed the widely accepted Kling-Gupta Efficiency (KGE), which evaluates model performance by balancing correlation, variability, and bias. This metric is a standard in hydrological modeling and aligns with best practices for the conventional approach.

For the asynchronous method, we calibrated against the distribution of streamflow using raw GCM data, which inherently lacks temporal synchronization with observed flows. This is the crux of the asynchronous method, as the flows are not synchronized between GCM-driven simulated and observed flows. Therefore, we required an objective function that does not rely on timing. Root Mean Square Error (RMSE) on the distribution of flows was chosen as it is well-established, simple, and excludes correlation in timing, making it suitable for this purpose.

We agree with the reviewer that other metrics excluding correlation could also be considered and might yield different results. However, our goal was to evaluate the current state of the asynchronous method rather than proposing a modified approach. In the discussion section of the manuscript, we propose integrating metrics that incorporate some form of timing (i.e. seasonal) to address issues related to the misrepresentation of hydrological processes.

To further clarify the asynchronous method's workflow, we propose to include the below workflow diagram in Section 2.3.3 of the revised manuscript. This diagram is adapted from Ricard et al. (2023, https://doi.org/10.5194/hess-27-2375-2023).

We will also expand the discussion to explicitly address the implications of using alternative metrics for the asynchronous method.

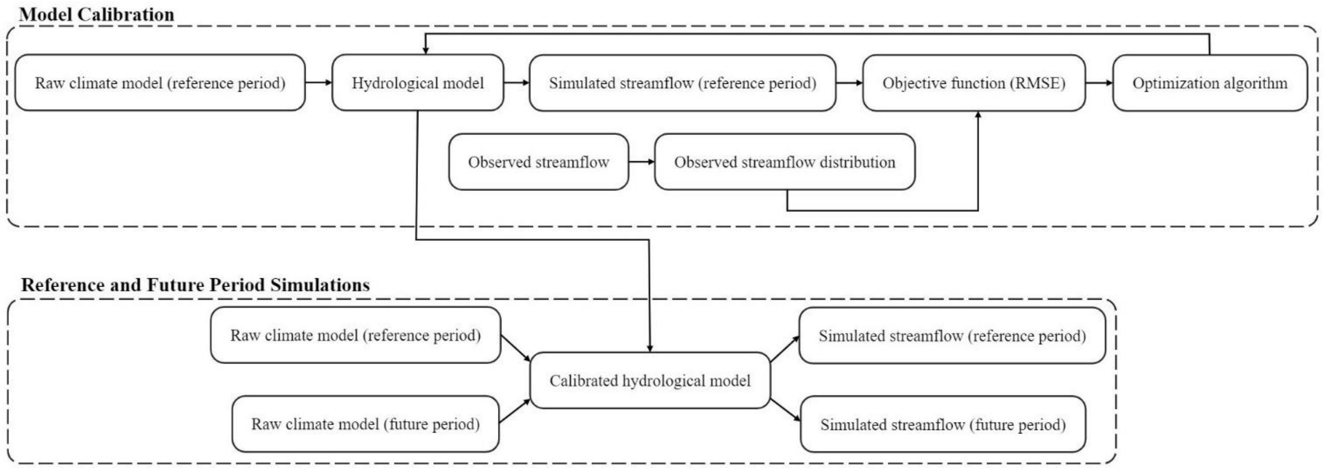

Figure 2. Workflow diagram of the asynchronous method.

Moreover it remains questionable what the value is of calibrating a hydrological model on poor meteorological forcing.

It is important to note that the asynchronous method is an existing approach in hydrological modeling. Its added value lies in its ability to function without requiring meteorological observations or a bias correction step, making it easier to implement compared to the conventional approach.

Additionally, the asynchronous method inherently preserves trends and maintains the physics of the GCM and consistency between simulated climate variables due to the absence of bias correction. This feature ensures that the original GCM signals are retained, which is particularly advantageous for capturing climate change impacts.

The objective of this study is not to propose or advocate for the asynchronous method but to critically evaluate its performance relative to the conventional method and to identify potential solution pathways. By systematically comparing these two existing approaches, this study aims to provide insights into their respective strengths, limitations, and suitability for different hydrological modeling scenarios.

Another approach would have been to use models that tune hyperparameters and are more focused on patterns / seamless parameters like the MPR (Samaniego et al. 2010) or follow a same approach even without calibration (example in this context is in Sperna Weiland et al., 2020). Something that could also have been used for the Wasim model used?

We appreciate the reviewer’s suggestion to explore alternative approaches, such as using models like MPR (Samaniego et al., 2010). These approaches offer valuable insights into parameter transferability across scales and regionalization.

While our study does not focus on these aspects, we agree that applying multiscale parameter approaches to WaSiM could be an interesting direction for future research, and we will add the reference in the discussion. Such an investigation could enhance our understanding of parameter behavior across scales and further improve the robustness of hydrological modeling in this context.

Refs:

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008WR007327

https://www.frontiersin.org/journals/water/articles/10.3389/frwa.2021.713537/full

Specific comments

563, 592 => another or more stat metrics could have minimized this effect. Why isn't this discussed

This is a good point. The potential use of additional statistical metrics to minimize the asynchronous method's challenges with synchronization is addressed in section 4.3 of the manuscript, where we discuss the potential improvement of the asynchronous method through the integration of synchronicity. Specifically, we propose the development of a semi-asynchronous approach that combines the strengths of both conventional and asynchronous methods.

The relevant passage (lines 649–657) states:

"Looking forward, one of the most promising avenues for improving the asynchronous method is the integration of synchronicity, leading to the development of a semi-asynchronous approach. This hybrid method would combine the strengths of both the conventional and asynchronous methods, offering a more balanced solution that mitigates the weaknesses observed in each. By incorporating synchronicity into the calibration process, the semi-asynchronous method would better align the timing of hydrological events, such as snowmelt, with observed data, improving its ability to capture critical seasonal dynamics.

For instance, modifying the objective function to calibrate based on seasonal or monthly data could enhance the model's ability to simulate hydrological processes. This integration of event timing into the calibration process is crucial for addressing the timing discrepancies that currently limit the asynchronous method's performance."

This discussion highlights the importance of exploring alternative metrics, such as those focusing on seasonal or event-specific timing, to improve (or partly integrate) synchronization in the asynchronous method. We will ensure this point is emphasized further in the revised manuscript to address the reviewer's concern.

665=> I see that you mention this here but I would have expected a thorough discussion in the discussion section and more in the material/methods section about this choice.

After reviewing line 665 and the surrounding sentences, we are unsure about which specific choice is being referenced. If the comment pertains to the choice of calibration metrics, modeling approach, or another methodological aspect, we kindly ask for clarification to address the concern more directly.

We appreciate the valuable feedback and believe these revisions will significantly improve the clarity and scientific contribution of our study.

Sincerely,

Frédéric Talbot on behalf of all authors