

Subject:	Fw: HYDROL63641: Editor's decision	
From:	王玉靖 <wangyujing@lreis.ac.cn>	Feb 14, 2025 7:59:27 PM
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-----原始邮件-----

发件人:"Yuefei Huang" <em@editorialmanager.com>

发送时间:2025-02-14 18:00:49 (星期五)

收件人: "Yujing Wang" <wangyujing@lreis.ac.cn>

抄送:

主题: HYDROL63641: Editor's decision

CC: yuefeihuang@tsinghua.edu.cn

Ref.: "A spatially hybrid hydrological modeling approach based on spatial heterogeneity of watershed characteristics" (Dr. Yujing Wang)

Dear Dr. Wang,

I very much regret to have to tell you that publication entitled, "A spatially hybrid hydrological modeling approach based on spatial heterogeneity of watershed characteristics" (Dr. Yujing Wang) in our journal is not recommended.

We would, however, consider as a new submission for review a substantially revised version of this paper that addresses all of the reviewers' comments. Should you choose to submit such a revised manuscript please refer to the present manuscript number, provide a detailed point-by-point reply to all of the reviewers' comments, and state how the revised manuscript addresses these.

An explanation for this decision is given in the attached review reports (and on <https://www.editorialmanager.com/hydrol/>). I hope that the comments contained therein will be of use to you.

Thank you for your interest in our journal.

With kind regards,

Yuefei Huang, Ph.D.

Editor

Journal of Hydrology

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Important note: If a reviewer has provided a review or other materials as attachments, those items will not be in this letter. Please ensure therefore that you log on to the journal site and check if any attachments have been provided.

COMMENTS FROM EDITORS AND REVIEWERS:

Although your manuscript falls within the aim and scope of this journal, it is being declined due to lack of sufficient novelty. We receive a much larger number of papers than we are able to accept.

Although the problems being addressed are potentially of interest to our readership, your manuscript does not meet the required quality standards to be considered for publication.

The research results reported are too premature for publication. More work is needed to substantiate the conclusions in your manuscript.

Reviewer #1: This paper by Wang et al. is a revision of manuscript HYDROL55981. The authors have made significant revisions to the manuscript and have extensively responded to my previous comments. I appreciate their effort; however, I do not feel that the major concerns I previously raised have been adequately addressed.

One of the central issues remains the definition of "physically-based models." In their response, the authors assert that physically-based models are those that "incorporate solving algorithms simplified from physical principles and adhere to mass and energy balance," whose "many parameters (especially terrain and soil attributes) are derived from field measurements and have clear physical meanings." By this definition, however, virtually all computational models today could be classified as physically-based.

The authors cite TOPMODEL as an example of a physically-based model. While this may have been true when TOPMODEL was introduced 45 years ago, the understanding of "physically-based" modeling has evolved. What was once state-of-the-art is now often regarded as a lumped conceptual model. Beven et al. (2021) in their paper, The history of TOPMODEL, describe it as "a structurally, parametrically, and computationally parsimonious model which gave it advantages over the full implementation of the

physically-based model blueprint set out by Freeze and Harlan (1969)." This suggests that even the original authors, Drs. Beven and Kirby, might not categorize TOPMODEL as a physically-based model by today's standards.

I acknowledge that definitions of "physically-based models" can vary, and while I respectfully disagree with the authors' interpretation, I will defer to the editor's judgment on this matter.

The authors' claim that the hybrid model "demonstrated an overall better performance" compared to the physically-based and conceptual models is also questionable. While the hybrid model performed better during the calibration period, its performance during the validation period was not superior to the "physically-based" model. Specifically, the hybrid model and the "physically-based" model yielded the same NSE and RSR values in the validation period, while the hybrid model exhibited a much higher bias, despite a slightly better R2. I would like to note that R2 is widely recognized as an unreliable metric for assessing model-data agreement in discharge simulations.

An intriguing observation is the disparity in performance between the calibration and validation periods for the hybrid model. The "physically-based" model displayed consistent performance across both periods, whereas the hybrid model's performance noticeably degraded during validation. This raises the possibility that the hybrid model may have been overfit during calibration. The hybrid model's inclusion of both physically-based and conceptual components likely increases its degrees of freedom, enabling it to better match observations during calibration. However, this flexibility might result in overfitting, as evidenced by its diminished performance during validation.

Another issue pertains to the model calibration process, which remains unclear. The authors state that "The objective function for parameter calibration is maximum NSE, minimum absolute values of RSR and PBIAS, with each component given equal weight." Does this imply that the model was calibrated three separate times using distinct objective functions? Or was a composite objective function, such as $a * NSE - b * RSR - c * PBIAS$, employed? Additional clarification would enhance the transparency of the calibration process.

In conclusion, the modeling framework proposed by the authors is intriguing and offers the flexibility to support models of different categories and spatial discretizations within the same simulation. However, based on the results presented in the manuscript, I do not find sufficient evidence to conclude that the hybrid model/approach surpasses the "physically-based" model. Consequently, I feel that the authors' conclusions are not strongly supported by their results.

Reviewer #2: This manuscript proposes an extension for the Spatially Explicit Integrated Modeling System (SEIMS). While it is well-structured study. But I doubted it as a "novel" method as stated by the authors. In hydrological modelling, based on Beven (2012) identifying the goal of modelling and understanding the situation and the process of our watershed is crucial (as perceptual model). And this is the reason that there are some flexible modelling structure frameworks, as discussed in the manuscript.

The authors mentioned two limitations for their study (in the conclusion section). I think this extension can have more limitations especially in adding much uncertainty from conceptual model structure part to the physical model part and the importance of connectivity of hydrological components also needs to be discussed, maybe in future applications.

I agree that this idea can provide more flexibility in modelling, but how can we balance this flexibility with other new arise issues including increasing various sources of uncertainty and finding optimal spatial constitutions for new case studies.

Beven (2012). Rainfall-Runoff Modelling: The Primer. (In one of the chapters, it introduces the 5 steps (or the modelling cycle) in hydrological model development. The first step is called as perceptual model developments, which is some time neglected in hydrological model studies)

Reviewer #3: This manuscript presents a novel spatially hybrid hydrological modeling approach, combining lumped conceptual and distributed physically-based models to address the challenges posed by spatial heterogeneity in watersheds. The authors implemented their method using the SEIMS framework and validated it through a case study of the Heihe River Basin, highlighting the model's ability to outperform consistent modeling approaches. In general, the paper makes a significant contribution to hydrological modeling by introducing a flexible and scalable framework for hybrid modeling.

As the additional reviewer for the second round, I find that the authors have thoroughly and adequately addressed the comments raised by the reviewers, including those that I would have highlighted in my own review process. Therefore, I would like to recommend acceptance of the manuscript, subject to a few minor revisions and suggestions.

Suggestions:

1. Please consider adding a discussion on the potential of adapting the spatially hybrid hydrological modeling approach to advance integrated modeling, particularly given its ability to adapt and incorporate existing structure-varying models for large-scale hydrological applications. Its design enables the reuse of pre-existing hydrological models, facilitating their seamless integration into a unified framework. This adaptability enhances the ability to address large-scale hydrological challenges by leveraging prior modeling efforts, thereby improving efficiency and ensuring consistency across various scales and regions.

2. Consider adding an explanation or some examples to the fourth paragraph to show why fixed model structures have trouble with diverse and complex applications.

Minor comments:

1. Change "primarily" to "usually" and "may" to "can" in line 79.

2. It is not clear what categories the listed models belong to in line 89-95. Please rewrite this sentence to link models to their corresponding categories.
3. Change "was" to "is" in line 134.

AE comments - Thank you for submitting your revised manuscript. Three reviewers reviewed the paper and recommendations ranged from Major revisions to Minor. In particular, reviewer 1 and 2 questioned the novelty of the manuscript and lack of better performance of the hybrid model compared to other approaches implemented here. While authors included additional information in the revised manuscript, those mostly pertained to additional general literature that in some cases convey basic hydrologic science knowledge or details about the modeling code. If your contribution is to highlight the model code, I recommend submitting to journals that publish software. Unfortunately, the current manuscript does not provide any improved understanding of hydrologic sciences or novel methods for modeling. I regret to inform authors that I recommend to reject their manuscript.

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