

HYDROL48272 - Editor decision - revise

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发给 朱良君

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发件人:

em.hydrol.0.7fcb74.fb082bc1 <em.hydrol.0.7fcb74.fb082bc1@editorialmanager.com> 代表

Journal of Hydrology <em@editorialmanager.com>

收件人: 朱良君 <zlj@lreis.ac.cn>

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Dear Dr. Zhu,

I can now inform you that the reviewers and editor have evaluated the manuscript "Identification of watershed priority management areas based on landscape positions: an implementation using SWAT+" (Dr. Liang-Jun Zhu). As you will see from the comments below and on <https://www.editorialmanager.com/hydrol/>, publication in its present form is not recommended, and major revision is being requested.

Please consider the reviews to see if revision would be feasible. To facilitate further review, add line numbers in the text of your manuscript. Should you wish to resubmit you should explain how and where (i.e. by giving line numbers) each point of the reviewers' comments has been incorporated. For this, use submission item "Revision Notes" when uploading your revision. Also, indicate the changes in an annotated version of the revised manuscript (submission item "Revision, changes marked"). Please follow the order "Revision Notes", "Revision, changes marked", "Manuscript", etc.

Should you disagree with any part of the reviews, please explain why.

Please strictly follow the formatting requirements as presented in the Guide for Authors. When submitting your revised manuscript, please ensure that you upload the source files (e.g. Word). Uploading a PDF file at this stage will create delays should your manuscript be finally accepted for publication. If your revised submission does not include the source files, we will contact you to request them.

Any new version should be returned within one month, as any resubmittal received after this time may, at the editor's discretion, be considered as a new paper.

To submit a revision, go to <https://www.editorialmanager.com/hydrol/> and log in as an author. You will find your submission record under Submission(s) Needing Revision.

When resubmitting, please present any figures, tables etc. as separate files. See the Artwork Guidelines on the home page right menu for further file naming conventions and format issues.

I hope that you will find the comments to be of use to you.

Research Elements (optional)

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With kind regards,

Nandita Basu
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.

Note: While submitting the revised manuscript, please double check the author names provided in the submission so that authorship related changes are made in the revision stage. If your manuscript is accepted, any authorship change will involve approval from co-authors and respective editor handling the submission and this may cause a significant delay in publishing your manuscript.

COMMENTS FROM EDITORS AND REVIEWERS

Dear authors, we have now received comments from three referees of your resubmitted article. While all three find the article to be of potential interest to Journal readers and a substantially revised version of the original manuscript, all three recommend additional major revisions before the manuscript can be considered for publication. Please carefully review the major suggestions provided by each of the three reviewers which include more clarification of definitions and methods employed and consideration of the improvement of model efficiency relative to accepted standards.

Reviewer #1: General comments:

This study proposed adopting landscape positions along the hillslope as identification units of PMAs, so as to balance the general applicability and the representation degree of spatial heterogeneity. The logic of this paper is excellent, and I think the result can absolutely prove the advantage of using landscape position as identification units. However, the SWAT+ and Markov method are both not new so the authors should show what is new for this paper in a clearer way. Besides, I think this paper needs more evaluation data to support the modeling result, maybe it will more convincing.

Specific comments:

L29-L59 The disadvantages of "Subbasins, Artificial geographic and grid cells" have been introduced, but I think you should also list the main advantage of those identification units to compare with your idea.

L188-L190 The reason why using a retention coefficient capacity of pollutants to simplify the complicated channel routing process should be explained.

L227-229 We generally believed that inorganic nitrogen include ammonia nitrogen, nitrite nitrogen and nitrate nitrogen, and in most areas the ammonia nitrogen account for a large proportion. I think maybe you should explain why the nitrate nitrogen is only considered in inorganic nitrogen.

L241-248 What kind of classification methods are used in this study?

L258 I think the information of landscape should be introduced.

L294-295 only 5-day monitoring data to calibrate total nitrogen the results if reliable?

Reviewer #2: There is some improvement in this version of the manuscript. Here are a few questions still causing uncertainty.

- a. The mathematics / modelling seem fine, but the key metric, the landscape position unit (LSU), isn't clearly defined. I think it is a metric that separates uplands and lowlands or specifically floodplains, but whatever it is, it doesn't have a clear definition and then description of how it was used to refine the subbasin scale.
- b. Where are the N and P values across the spatial units to show how they are used as inputs?
- c. There seems to be second objective which is an introduced methodology within the SWAT process - "improved Markov chain-based PMA identification method can be regarded as a method framework". If correct, then introducing two changes in a model (in this case scale and method herein) seems to undermine the attempt to demonstrate a change with the scale adjustment (LSU metric). For example, if something changes in the output, then is it the result of the method or the scale alteration?
- d. Figure 5 suggests there could be a further reduction in scale to HRU. This appears to 'improve' the results beyond LSU scale change. Why not present that HRU output and then discuss why the LSU is a better approach?
- e. Regarding PMA designations - for the China watershed the difference is small and for the USA watershed it is much larger. This suggests that other/many watershed characteristics effect the model outputs, i.e., other factors may be overriding the impact of the chosen LSU metric. These types of uncertainty undermine your LSU conclusions (an example why many more watershed would help determine if the LSU is doing what you wish).

Reviewer #3: This paper provides interesting results documenting how reducing the scale for Priority Management Area (PMA) derivation from subbasins to landscape position units can better focus the targeting of areas for application of best management practices. As such, this paper provides an interesting contribution to the environmental/pollution management community. Also, whereas I was not a reviewer of the original submission, I can see that the authors have made several substantial improvements to the paper in response to the earlier review comments. With respect to Reviewer 2's comments the revised paper has:

- A) Added a second example in response to Comment 1.
- B) In response to Comment 2, the English in the revised paper is generally clear and understandable, and I have offered a few suggestions on the marked manuscript to further improve the English in this paper.
- C) In response to Comment 3, the paper now includes the subsections recommended by the reviewer.

With respect to Reviewer 1's Comment 2, the authors have given a justification for using the area-based approach. I agree with Reviewer 1 that it would be preferable to have a more physically based approach to describe the transition from upland to floodplain to channel, but I also agree with the authors that such physical details are difficult to derive and the area-based approach is supported in the literature (e.g., Biegner et al., 2019).

In summary, I feel that the authors have done a reasonable job in trying to respond to the major comments of the original reviewers and have prepared a paper that may eventually be acceptable for publication in the Journal of Hydrology. The final acceptance of this paper for publication in the

Journal of Hydrology depends on the authors revisions to this paper in response to the following Major and Minor comments.

Major Comments

1) Lines 303 and 304 state "calibrated SWAT+ models have approximately satisfactory performance proposed by Moriasi et al. (2007)", and, thus, the authors conclude on Lines 307 and 308 "both calibrated models are applicable for the validation of the proposed PMA identification method in this study." However, for satisfactory models Moriasi et al. (2007) indicate NSE should be greater than 0.5 and RSR should be less than 0.7, other than the flow validation for the Zhongtianshe Watershed none of the model results meet satisfactory levels. The PBIAS values for the Zhongtianshe Watershed are acceptable, but those for the Willow River Watershed are not. The nitrogen PBIAS value for calibration on the Willow River Watershed appears "good", but, in fact, a "good" nitrogen concentration result with a poor hydrologic simulation yields poor nitrogen load prediction. Thus, in my experience with dozens of watershed models I would not categorize either SWAT+ model as satisfactorily simulating flow or total nitrogen. The results of the current study would be more convincing if the SWAT+ models were actually satisfactory. For example, since the Markov retention coefficients are derived from the internal loading information from the SWAT+ model, the current coefficients, derived from unsatisfactory models, are unreliable.

On the other hand, the current paper is not designed to guide watershed managers in the design and implementation programs for best management practices in the study watershed, but rather this is a "proof of concept" study on the value of landscape unit scale PMA determination. Thus, truly reliable modeling results may not be necessary. However, I think the paper would be more convincing if the authors could improve the calibration and validation of the SWAT+ models, and rederive the PMAs.

2) Lines 315 and 316 state that the "natural break method was utilized to classify the nitrogen load contribution of the spatial units". The paper should include details on this method and cite appropriate references when giving these details.

Minor Comments

1) Lines 51 and 52 state (note, text slightly revised): "they are not easily generalized as generally applicable identification units and are widely applied." This statement seems contradictory, that is, if artificial geographic entities are not easily generalized as applicable identification units, then why are they widely applied? Please re-write and clarify.

2) Numerous editorial suggestions are made throughout the marked manuscript which the authors should consider when preparing a revised version of this paper for re-review.

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