

Interactive comment on “Reducing the basin vulnerability by land management practices under past and future climate: a case study of the Nam Ou River Basin, Lao PDR” by M. Maharjan et al.

Anonymous Referee #2

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In my opinion this study does not provide substantial new ideas and findings compared to the study of Shrestha et al. (2013) published by the same authors (with other co-authors) in the same journal. I doubt whether HESS and its readership would benefit from publishing two papers that are so similar in content, hence my recommendation to reject the paper, but the decision belongs to the editors. My evaluation refers to the point 8 of the HESS General Obligations for Referees: "A referee should be alert to failure of authors to cite relevant work by other scientists. A referee should call to the editor's attention any substantial similarity between the manuscript under consideration and any published paper or any manuscript submitted concurrently to another journal."

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First, let me point the main similarities between Shrestha et al. (2013) and the paper under review. This paper:

- uses exactly the same SWAT model setup as in Shrestha et al. (2013);
- uses the same model calibration and validation results for both discharge and sediment as in Shrestha et al. (2013);
- also discusses the topic of the impact of climate change on discharge and sediment yield quantified using the same SWAT model in both cases;

What is new in the current paper compared to Shrestha et al. (2013)?

-it uses different (presumably more advanced, but this is not discussed) approach for downscaling of GCM climate scenarios; however this does not bring any new insight since, as the authors mention on p. 9878, line 11 "The result of climate projection from this study is comparable with that from Shrestha et al. (2013)...". Both papers admit that the uncertainty of climate projections leads to very uncertain projections of future flows and sediment yields.

-it uses a classification of critical subbasins based on some thresholds on sediment yields taken from literature; there is nothing exciting in it, especially given that the model resolution is so coarse (only 19 subbasins per 30 000 km²);

-it quantifies the efficiency of 5 different land management practices aiming at reduction of sediment yield under current and future climate; this is the only really novel part compared to Shrestha et al. (2013). There have been lots of papers though that evaluated sediment BMP efficiency though (but very few under future climate => the authors should have emphasized this aspect more). The problem is that in my opinion it is too little (of essentially novel aspects) for a paper to be published in HESS.

Otherwise, as a person having expertise in SWAT modelling, I agree with most of the critical comments of the Reviewer 1 concerning the "proper use of SWAT" and too modest Discussion.

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My evaluation would have been different if I hadn't been aware of the Shrestha et al (2013) paper. I would have said that the paper is novel, interesting and worth publishing after minor/major revision. But this is not the case.

References

Shrestha, B., Babel, M. S., Maskey, S., van Griensven, A., Uhlenbrook, S., Green, A., and Akkharath, I.: Impact of climate change on sediment yield in the Mekong River basin: a case 5 study of the Nam Ou basin, Lao PDR, *Hydrol. Earth Syst. Sci.*, 17, 1–20, doi:10.5194/hess-17-1-2013, 2013.

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