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Hydrology and Earth System Sciences
Discussions

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 #1

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General Comments

The manuscript describes the identification of areas within the Ou River Basin that are more susceptible to erosion under historical climatic conditions and quantifies how sediment yields within the river basin can vary under differing climatic and land management scenarios into the future. SWAT was utilized to simulate sediment transport under historical and future conditions with the future climate characteristics that were incorporated into SWAT being three downscaled GCM projections that were each forced under three established greenhouse gas emission scenarios. Subbasins were classified with respect to sediment yields that then allowed the identification of five subbasins that

are more susceptible to erosion. Sets of land management practices that aimed to reduce erosion potential were incorporated into SWAT simulations to quantify reductions in sediment yields. Modeling the land management practices in conjunction with the climate scenarios enables the authors to make recommendations of the most effective erosion control practices that can be used to inform land management.

The question (main objective) addressed by the manuscript is interesting and framed appropriately to be within the scope of HESS, but there is some confusion regarding what vulnerability means. Vulnerability should be clearly defined in the context of soil erosion and sediment yield to make the main objective less ambiguous. Additionally, the authors fail to explain the significance of soil erosion and why a high susceptibility to soil erosion within the Ou River Basin should be a concern. Mentions of planned hydroelectric projects are made throughout the manuscript that could serve as a starting point for highlighting the significance of soil erosion and the manuscript in general.

I do not have any experience with downscaling GCM output and cannot comment on the appropriateness of the methodology employed to that regard, but I do have expertise in SWAT model calibration/validation, use, and the incorporation of different land management practices into SWAT simulations that I can comment on. Conceptually, the approach utilized seems appropriate for the objectives outlined at the end of the introduction, but there are aspects of the SWAT modeling that draw some concern. Details regarding why SWAT modeling was conducted at a spatial resolution of 250 meters remains unclear; along with aspects of SWAT calibration (time-step used in computing goodness-of-fit indicators (GOFI) is not provided; GOFI values for calibration of sediment yield are vague; warm-up/calibration/validation time-periods seem too short for projections made so far into the future; and more GOFIs can be provided). The temporal scale at which SWAT was calibrated and inclusion of more GOFIs will provide insight on how to classify the degree of model calibration (see Moriasi et al. 2007). Additionally, the land use that was utilized in SWAT simulations (Figure 2) contains vague class names (e.g. “deciduous” and “mosaic”) and contains the class “clouds” that does

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not belong in a SWAT simulation. Furthermore, more details are needed regarding how the erosion control practices were incorporated into SWAT simulations because it seems like the incorporation only involved changing MUSLE parameters without specification of how these values were determined. Outside of concerns regarding SWAT simulations, I found the classification scheme used for depicting the vulnerability of a subbasin to erosion interesting, but the scheme originates from a conference preceding and I wonder if there are other similar classification schemes in the peer-reviewed literature that could be used. Finally, the authors mention the large degree of uncertainty in the GCM projections and SWAT parameterization, but no qualitative or quantitative description of the uncertainty is provided; thus the uncertainty should be a focus of discussion to build confidence in the results.

Due to concerns I have regarding the proper use of SWAT and reporting of necessary results, it is difficult to determine the relevance of the conclusions made in the manuscript. Of greatest concern is the fact that no discussion or details on simulated surface water flows is provided. Surface water flows are the principal mechanism in transporting sediment and thus should not be neglected. Additionally, no discussion of how the spatial variation in precipitation or surface water flows (historical and future) could have influenced the results is provided.

In its current form, I do not think the manuscript is publishable within HESS. The lack of information regarding simulated surface water flows and concerns over proper application of SWAT makes it difficult to assess relevance of the results and the conclusions. I would be happy to review the manuscript again if the authors address my concerns regarding the methodology and provision of information.

References

Moriasi, D. N., Arnold, J. G., Van Liew, M. W., Bingner, R. L., Harmel, R. D., and Veith, T. L.: Model evaluation guidelines for systematic quantification of accuracy in watershed simulations, Transactions of the ASABE, 50, 885-900, 2007.

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