**Hydrology and
Earth System
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Interactive comment on "Distributed model of hydrological and sediment transport processes in large river basins in Southeast Asia" *by* S. Zuliziana et al.

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Dear Editors and Reviewers,

We highly appreciate the detailed valuable comments of the referees on our manuscript of 'Distributed model of hydrological and sediment transport processes in large river basins in Southeast Asia'. The suggestions are quite helpful for us and we incorporate them in the revised paper.

In response to the raised comments, we have revised and attempted enhancement. And we hope the Reviewers and the Editors will be satisfied with our responses to the

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'comments' and the revisions to the original manuscript. We would be very appreciated if you could consider our manuscript for publication in your journal. We Look forward to hearing your final decision for Hydrological Earth System Science.

Our response to the referee's comments are as follows;

Q1. The author's review of existing sediment models is inadequate. Apart from (R)USLE and SWAT the complete research in sediment modelling is completely ignored. Since the proposal model is completely built on well-known approaches. I cannot see this novelty here. A1. The novelty in this model is the transport and deposition process in main channel which are not considered by many existing models. Furthermore, in my model, overland flow widths of the equivalent channel for each grid was proposed in calculation of deposition. Using the concept of equivalent channels, inter-rill and rill/gullying erosion can be modelled in a more physically based manner.

Q2. The confusion between a model's conception (i.e. physical vs. empirical) and its spatial structure (i.e. lumped vs. distributed) notwithstanding, the study addresses or verifies neither spatially explicit model output nor data in high temporal resolution. A2. Our model concept is a physical based model which are based on the physical equations describing streamflow and sediment generation. The parameters used in physical based model are measurable and known. The parameters cannot be measured in the catchment, was determined through calibration against observed data.

Our model also capable to estimate spatial and temporal distribution of net soil for an entire hillslope or for each point on a hillslope. The authors agree with the referee comments which we are should include the results or discussion related with spatial output. In this manuscript, we only include the temporal output.

Q3. The model evaluation is not very rigorous: Using observed dam outflow as operation rule makes modelling discharge somewhat trival for the downstream gauges. A3. In this manuscript the authors used observed dam outflow as operation rule is because of the limitation of available observed data like observed dam inflow and information

about the operation rule.

Q4. The discussion of the model shortcomings is limited and quite speculative. Other shortcomings of the model (insufficient rain gauge network, river overboarding, unaccounted reservoirs,...) probably deserve more discussion. A4. The authors agree to add more discussion in the manuscript.

Q5. Description of hydrological and sedimentological model lacks clarity and comprises numerous inaccuracies, but is still very lengthy - numerous imprecise formulations that leave the reader with many questions. - reservoir siltation is altogether neglected in the Mekong case - including it is imperative when modelling sediment at this scale. A5. Description on hydrological model already published by Yang, 2000. The authors already mention in the manuscript. For sediment model, the authors agree that the present model still have a limitation and uncertainties in terms of model inputs, parameters and structure which may influenced the simulation results. The authors already mention the limitation of the model and further improvement for the future study in the manuscript. Moreover, the authors agree that is very important to include reservoir siltation in the Mekong case but due to limited availability of dam observation data, the authors decide no estimation for reservoir sedimentation. Therefore, the model still need improvement for better performance.

Q6. Using r as a measure of fit is misleading at best - The performed sensitivity analysis is very crude an restricted to few variations of even fewer parameters. The conclusions drawn from it do not seem well-founded to me. A6. Beside using r, the authors also used Nash-Sutcliffe Efficeincy (NSE) coefficient in evaluation of performance of sensitivity analysis. The r measurement was used to be supported for NSE in sensitivity analysis.

Q7. The authors claim the model be "useful for management and stakeholders". I wonder what information does the model give that cannot be obtained from the available measurements? Especially when publishing in an open access journal, Model avail-

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ability should be explained; ideally, the code should be provided. A7. The model output can give information and useful for management and stakeholders as this model is a grid-based model, it can identify locations of serious sediment dynamics problem which are cannot be identified by available measurements. Moreover, the authors agree with the referee comment that the availability of the model should be carefully explained. The authors also will consider to provide the code.

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