



Interactive comment on “Impact of climate change on sediment yield in the Mekong River Basin: a case study of the Nam Ou Basin, Lao PDR” by B. Shrestha et al.

Anonymous Referee #2

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Review of: Impact of climate change on sediment yield in the Mekong River Basin: a case study of the Nam Ou Basin, Lao PDR

By Shresh et al.

As the title suggests, this paper looks at the potential changes in sediment transport in the Mekong River linked to climate change. The topic is suitable to readers of HESS. I found the paper interesting. It is well written and was easy to follow. Figures and Tables were clear although once printed in black and white, a couple were much harder to decipher (specific comments below). While this is a well-done study, I nevertheless

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find a few important weaknesses as follow:

- Modelling of sediment yield is not very good

As such the paper is more of a tendency in future trends study. Absolute values should be disregarded. This is not emphasized enough in this paper. In fact when looking at future trends, I would remove any absolute values from the paper only to keep relative change values.

- Only one GCM/RCM

While the authors mention that it is a weakness, I strongly feel that this is not enough. Minimally, RCM biases should be ascertained. Does the chosen model exhibit a wet or dry, warm or cold bias compared to others. Dispersion diagrams ($\Delta P/P, \Delta T$) can easily be drawn to improve our knowledge of future trends. While RCM data are probably difficult to get over this region, there is a plethora of GCM data available that could have been used, especially with the delta-change downscaling method. Why use two scenarios when it is widely known that scenario uncertainty is usually dwarfed by model uncertainty? As such the authors could have unknowingly picked the only climate model that predicts precipitation increases (and sediment yield increases) and draw very likely incorrect conclusions from a single data point.

- The use of the delta-change method This is a method that has been used quite a bit in many studies. However, there are now several available empirical downscaling methods which are very likely more appropriate than delta-change, especially with respect to the modification of extremes. I would not worry too much about keeping the same precipitation occurrence series in this case, but I would worry a lot about keeping the same variance, and especially with the high likelihood of underestimating future precipitation extremes. As mentioned in the paper, increased discharge results in an even larger increase in sediment yield. The choice of the delta-change approach, while appropriate for analysing mean values and interannual variability is probably ill-chosen for the problem at hand. Empirical downscaling methods such as daily scaling (Mpela-

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soka and Chiew, 2009, J of Hydrometeorology) or quantile mapping (Thiemeß et al., 2010, Int. J. Climatology) would have been more appropriate.

- Uncertainty not addressed

Most recent papers have addressed uncertainty in multi-model/multi-scenario/multi impact model/multi-calibration approaches. In this case, we have two combinations (1 climate model, 2 scenarios, one impact model, one calibration), compared to several hundred (and several thousands in some cases). This paper does not address uncertainty in any way and drawing conclusions based on this one sample is not appropriate. While we may not expect all study to include all potential sources of uncertainty, a thorough discussion must minimally be included. If such a discussion was to be included using the data available in the paper, the only possible conclusion would be that it is impossible to say anything about future trends.

Accordingly, I must reject the paper in this current form. This is what I think the authors should minimally do to improve the manuscript:

- Use data from several GCM to derive the delta-change factors. If the authors do not want to deal with dozens of curves, they should minimally derive the delta-change factors from the ensemble mean (as suggested by the IPCC) and not by using one model with unknown behaviour over the region of interest. Bracketing the dispersion pattern would be even better.

- Use a climate model with daily data and behaviour close to that of the ensemble mean (on a dispersion diagram) and apply either daily scaling and/or quantile mapping downscaling method. These methods will emphasize potential changes in extremes that play such an important role in sediment transport.

- Discuss uncertainty in a much more detailed way (adding appropriate references, see Wilby and Harris, 2006, Kay et al., 2009. Climatic Change, or Chen et al. 2011 WRR for example), and how it would potentially impacts the model results. In particular, in

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the case of this study, I would wonder how the use of another sediment yield model would impact the result (considering the rather poor modelling results). The authors do not have to use another model, but they have to discuss how results might have been affected when using another model.

Specific comment: Abstract: the abstract does not clearly state that only one climate model was used. 3345-18: Why not use the entire SCU dataset instead of using two sources. 3346-25: Why the delta-change method? Because it is simple or because it is the most appropriate? 3349-23: A bit confusing. Why was automatic AND manual calibration used? Which one was used? Difference in results? Why was only manual done on sediment yield? Was streamflow and sediment yield calibration done together or separately? How does the streamflow calibration influences sediment yield calibration? 3350-14to25: This part was not clear for me. Additional details are likely needed. 3355-3to5: not clear. I do not see how uncertainty in the conceptual model can be accounted for. 3357-18to19: this is quite expected when using the delta-change method. 3359-3: I do not see that in the results 3374: location of basin cannot be seen in black and white

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