Hydrology and Earth System Sciences Discussions

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.

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Responses to the comments by Reviewers and Editor Title: "Impact of climate change on sediment yield in the Mekong River Basin: a case study of the Nam Ou Basin, Lao PDR"

We are thankful to the Reviewers and the Editor for their valuable comments and suggestions on the paper. Below we provide our response to the comments and questions raised.

Reviewer # 1

Comment 1: Temperature correction The derivation of the daily maximum and minimum temperatures is not clearly presented. If I understand it correctly, you use the correlation of the monthly (long term or annual? -> should be specified) means in Tmax and Tmin to correct the daily simulated min and max temperatures? In other words it is assumed that the distribution and bias in the monthly means is identical to the daily variations. This is a strong assumption! And certainly requires more explanation and justification. Show e.g. the empirical distribution of the daily and monthly data and the scatter plots that underlie the regressions. Also, why is the correction based on monthly means at all? As I understand it, you use the gridded daily observation data of SCU (http://hydro.engr.scu.edu/files/gridded_obs/global/daily/), and not the simulated monthly data. This would enable a direct comparison of the gridded data with the daily observations from the study area. Or is the temperature data from the study area monthly? As an alternative to proving the applicability of using the monthly bias correction to daily data, you should conduct a sensitivity analysis of the discharge and sediment yield simulations on these input parameters, you need these parameters for the hydrological modelling (evapotranspiration). The sensitivity results should then find their way into the discussion of the results.

Response 1: The correlations are stronger at the monthly time scale and results in reduced bias, but we fully agree that this overlooks the variability that exists within a month. The observed and SCU data are both available in daily time step. Therefore, we will check how good relationship can be established at a daily time step. If it appears, based on sensitivity analysis as suggested, to be better than using the monthly correlation, we will revise accordingly in the revised version.

Comment 2: Parameter and uncertainty estimation I would urge the authors to explain the calibration and uncertainty estimation procedure in more detail, because the method (SUFI-2) is not that well known as e.g. GLUE.

Response 2: The initial description of the SUFI-2 method is available in Abbaspour et al. (2004) and Abbaspour et al. (2007). These references are included in the

manuscript. The method is available as a tool box to be used with SWAT. A number of successful applications of the method have been reported in the literature including Masih et al. (2011a & b). We agree that it is a relatively new method and less know than GLUE, for example. Therefore we will add more description of SUFI-2 method in the revised version.

Masih I, Maskey S, Uhlenbrook S and Smakhtin V (2011a). Impact of upstream changes in rain-fed agricultural water management on downstream flows in a semi-arid basin. Agric. Water Manage. 100 (2011): 36-44, DOI:10.1016/j.agwat.2011.08.013.

Masih I, Maskey S, Uhlenbrook S and Smakhtin V (2011b). Assessing the impact of areal precipitation input on streamflow simulations using the SWAT model. J. American Water Resources Association (JWARA), 47(1):179-195. DOI: 10.1111/j.1752-1688.2010.00502.x.

Further cComments on "Parameter and uncertainty estimation":

(i) Why have some parameters initial values and some ranges? As I understand the calibration procedure, one assumes a physical/plausible range for all parameters. Apparently this is not the case and some explanation for this should be given.

Response (i): Thanks for pointing this out. Actually the ranges mentioned in the table are also initial values but for different subbasins, which are specified based on land use and soil types. As you rightly pointed out all the parameters used for calibration have ranges within which optimal parameter sets are searched. We agree that mentioning ranges for the initial values can be misleading, which we will revise/clarify in the revised version.

(ii) What is the meaning of the percentage values of the fitted parameters that is apparently given for those parameters that have an initial range specified? I assume that this is the percentage change relative to the initial value range, but this is not clear and needs to be explained.

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Response (ii): Yes in SUFI-2 there are two ways to change parameter values during calibration: one by directly changing the absolute value of the parameter, and another by changing the parameter value relative to the initial value specified for the parameter. We will clarify this in the revised version.

(iii) In case a range is given for an individual parameter, what values are used to obtain the simulation results shown in Fig. 3+4? Are the results equifinal or is it the median from different model parameterizations shown in the figures?

Response (iii): As we clarified in our response (1.3) above, the ranges mentioned in the table are also initial values, which we will clarify in the revised manuscript. The result presented is not the median from different model parameterizations but based on the best fitted values.

(iv) Are the uncertainty estimates derived from model runs sampling parameter values from the given ranges? How many model runs were performed anyway?

Response (iv): Yes the uncertainty estimation is based on model runs from sampling parameters. In this study 750 runs were used.

Comment 3: Use of Single GCM.

I believe that the authors are well aware that climate change impact studies are highly uncertain, mainly because of the uncertainties in GCM predictions. As many authors have pointed out already, the use of just a single GCM/RCM to assess climate change impacts is problematic, because use of another GCM and even RCM or a different downscaling method is likely to produce different results. This is particularly true for a region as the Mekong basin, where two different monsoon systems are active, as e.g. the cited Kingston et al 2011 illustrate. Thus a multi-model approach is now recommended and kind of state-of-the-art, although this is certainly also not an optimal solution, but the best there is right now. And this puts some limitations on the conclusions drawn from this study: it shows one of possible many, more or less equal likely

futures. This doesn't mean that the study has no value, but in the conclusion and discussion you have to take this into consideration and soften your statements under the limitation "as predicted by ECHAM5+PRECIS". The study surely gives indications of future developments and is as such useful for basin management, but in order to be a somewhat reliable basis for planning, it needs to be corroborated by driving SWAT with different GCM products.

Response 3: We agree that the use of only one GCM is indeed a limitation of this study. This has also been pointed out by the second reviewer. Therefore, we decided to add a number of other GCM results. We are considering two or more from CCMA_CGCM3, CNRM_CM3, NCAR_CCSM3, MIROC3.2Hires, GISS_AOM, MPI_ECHAM5. We will update the manuscript accordingly.

Comments inserted directly in the manuscript (supplemental document)

Comment 4: Abstract The GCM/RCM combination used for the assessment of climate change impacts should be named prior to the results, even in the abstract.

Response 4: We agree that GCM/RCM combination used should be mentioned prior to results. We will update the abstract accordingly and also specify this in an appropriate section before the results.

Comment 5: Introduction and GCMs!

Response 5: Yes the estimated increase in global mean atmospheric temperature is based on various GCM's and greenhouse gas emissions scenarios. We will add GCMs in relevant text in the revised version.

Comments 6: Introduction Page 3343 That's Setegn 2010 in the reference

Response 6: Yes the publication year should be 2010. Thank you for pointing this out. We will update the citation.

Comment 7: Observed data It should be mentioned that these data are observed, not

GCM modeled data, in order to avoid confusion. This is the case, isn't it? On the cited web page simulated as well as observed gridded data are available. If you use the observed gridded data, you should add the reference to Adam and Lettenmaier, 2003

Response 7: Yes the data are observed not GCM modeled data. The reference to Adam and Lettenmaier (2003) will be provided in the relevant section of the revised manuscript and will be added in the reference list.

Adam, J.C., and Lettenmaier, D.P. : Adjustment of global gridded precipitation for systematic bias. Journal Of Geophysical Research,108, 4257-4271, 2003.

Comment 8: Observed data Is there any reference to MQUAD the method? What is the advantage to standard Thiessen polygons? This appears to be a critical step and should be elaborated a bit more.

Response 8: MQUAD is described in the report "Decision Support Framework User Guides DSF211 MQUAD" by Halcrow (2004) for Mekong River Commission. The is freely available from MRC in a pdf form.

MQUAD is based on multi quadric analysis developed by Hardy (1971) in which the surface is represented as summation of many individual quadric surface. For real rainfall data from an irregular network of rain guage stations, multi quadric analysis is a most practicable and efficient method for determining areal rainfall (Shaw and Lynn, 1972). The advantage over Thiessen polygons is described in more detail by Shaw and Lynn (1992) (who supplied the algorithms used in MQUAD). We have mentioned about MQUAD method however we will elaborate more about MQUAD in the revised paper.

Shaw, E.H and Lynn, P.P. Areal rainfall using two surface fitting techniques. Bulletin of the International Association and Hydrological Science XVII, 4 12, 1972.

Comment 9: Observed data Luang Prabang doesn't seems to work that well. 3.7°C difference in mean Tmax and 2.8°C in mean Tmin is not negligible, is it?

Response 9: Yes, we agree that the result for this station is not as good as that for the other stations. The SCU is a gridded (1/2 degree) data source. In the present result the selection of the SCU values is based on the nearest-neighbourhood interpolation for deriving the relationship. It might be that the relationship may be improved if other interpolation methods are used (e.g. bilinear interpolation). We will explore these alternatives to see if the relationship can be improved.

Comment 10: Observed data Page 3346 Is discharge really measured? most of the discharge data from the mekong basin I know is derived from water levels and rating curves. I asked because this has implications on the uncertainty inherent in the data and consequently the assessment of the model results and performance.

Response 10: Yes most of the discharge data from the Mekong basin is derived from water levels and rating curves that are based on intermittent discharge measurements. We agree that this has implication on the assessment of the model results and performance which we have also mentioned in section 4.2. This was also noticed by Rossi et al. (2009). As a matter of principle the rating curves should be regularly validated and updated. For the revised manuscript, we will try and add some information on how often the rating curves are updated for the data used in this study.

Comment 11: Regional climate model outputs I would not count the delta change method as a downscaling method. It is a method of assessing climate change impacts without relying on GCM results alone. It preserves the observed variability and adds relative simulated changes to it.

Response 11: Delta change method has also been considered as in simplest form of downscaling method by scientific community. We would like to refer to one paper here : "Fowler et al., 2007 Linking climate change modelling to impacts studies: recent advances in downscaling techniques for hydrological modeling. Int. J. Climatol. 27: 1547–1578. "

Yes we agree to your point that this method only scales the mean, maxima and minima

of climatic variables, ignoring change in variability and assuming the spatial pattern of climate will remain constant, which is indeed a limitation of this method.

Comment 12: Page 3349

Some more information of the data sources should be given, e.g. is the DEM based on remote sensing or interpolated topographic maps, source of land use and soil maps, etc. The MRC typically collects data, rather than producing it.

Response 12: DEM used was provided by MRC which is based on interpolated topographic maps. Since majority of data used were provided by MRC we indicated MRC as our source for data, however we will provide some more information of the data sources in the revised manuscript.

Comment 13: Model evaluation and uncertainty analysis only for mean behaviour, not extremes!

Response 13:Noted. We will rephrase appropriately. Comment 14: Page 3351 Simulated = the PRECIS data? pls. clarify.

Response 14: Thank you for noticing this. The observed temperature and downscaled temperature was compared not simulated. We will clarify this in the revised manuscript.

Comment 15: Page 3352 What rainfall product was used in the calibration? I assume the extrapolated observed rainfall? Or the precipitation from the gridded obervation data set? Or from PRECIS? This should be explicitly specified, either here or in the data section. Best in both.

Response 15: Yes extrapolated observed rainfall was used in the calibration. We will specify this in data section in the revised manuscript.

Comment 16: Page 3352 What is the objective function the model was calibrated against? pls. specify.

Response 16: The Nash Sutcliff (NS) was used as objective function to calibrate model.

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We will specify this in the revised manuscript.

Comment 17: Page 3353 Yes that may be a cause, but not the only one. As it seems the model simulates the mean sediment loads well, but not the extremes during the wet season. Adding more data points, which are typically points in the average range, would increase the R2 and NS, but not necessarily the performance in the higher sediment yields. It might well be that the model does not consider the processes causing erosion during high rainfall events properly, as you mention later on. However, you can still argue that you capture the average system behaviour well and that this is most relevant for this study. This is expressed in the PBIAS. Also, the high sediment yields during the wet season may be caused by effects that cannot be captured by the model, e.g. heavy rainfall induced landslides, river bank collapses or human activities. This would be an additional argument for the validity of the simulation results.

Response 17: Yes the model was not able to capture the extremes as good as the mean values during the wet season. By more data points we mean more data points covering more of the extreme values. That would have allowed better calibration of the model for the extreme values. But the problem of data particularly for the extreme values is well understood. We also agree that the model may be able to represent some of the processes associated with the extreme values well as you have rightly pointed out. We will update the discussion accordingly in the revised version.

Comment 18: Page 3357

This needs some addition: ..HadCM3 GCM and assuming a stepwise change in global temperature rise fom 0.5°C to 6°C Kingston et al. also performed a multi-GCM evaluation of climate change impacts yielding very contrasting results with regard to future streamflow changes.

Response 18: Thanks for pointing this out. We will update this information in the revised manuscript.

Comment 19: Page 3357 can this contrasting behaviour be attributed? What can cause this behavior, resp. is causing it?

Response 19: We have explained the possible reason for contrasting behavior of changes in discharge and sediment in the following paragraphs in the same section. Please kindly refer to page 3358 line 11-20.

Comment 20: Page 3358 How? by your parameter estimation method?

Response 20: Yes it is by our parameter estimation method.

Comment 21: Page 3358 This whole paragraph is difficult to read, because it is not always clear what is infered from this study and what are citations. It is also not clear if the arguments are based on model results or general process considerations. This should be improved.

Response 21: Here we are trying to justify our model result with general process consideration. Our model results indicates that although there was decrease in rainfall the sediment still increase which might be due to increase in temperature. Study by Zhu et al., 2008; Li et al., 2011 indicated that increased temperature may aggravate the soil erosion rate, and consequently increase sediment flux through its influence on vegetation and weathering. We will clarify and improve this in updated manuscript.

Comment 22: Page 3359 you should explicitly mention that the estimations do NOT consider the sediment trapping in the (planned) reservoirs. I.e. the reduction in suspended sediment by reservoirs is not considered in this study.

Response 22: Yes in this paper we have not considered the trapping of sediment by reservoir instead we are trying to show how sediment yield will change due to climate changes in possible locations for planned reservoirs. We will clarify this in the updated manuscript.

Comment 23: Page 3360 which is somewhat contradicting to the results of Kingston 2011. However, since Kingston et al. do not show the results of the Nam Ou basin

explicitely, a direct comparison cannot be done, unfortunately. But this should be adressed in the discussion/conclusions. And maybe contact Kingston for the results of the Nam Ou basin for comparison?? But this is a suggestion only.

Response 23: The results might be contrasting because of difference in GCM/RCM as these results are likely to be GCM/RCM dependent. The spatial and temporal variation of climatic change within the basin is another possible reason. However in order to present and discuss our results in more concrete way we will use some more GCM's and discuss the results also referring to the results obtained by Kingston (2011) where relevant in updated manuscript.

Comment 24: Page 3374 please also show the locations of the rainfall gauges used for interpolating spatial rainfall.

Response 24: Thanks for your suggestion. We will update the figure with the locations of the existing rainfall gauges that are used for interpolating spatial rainfall in the revised manuscript.

Comment 25: Page 3379 it would be more consistent if red is used for observed and blue for simulated values, as in the previous figure

Response 25: Noted and will be updated.

Comment 26: Page 3379 Why? That is not explained

Response 26: Number of days used for calculating average sediment yield varies from 2 to 21 because of sporadic nature of sediment measurements which is already mentioned in data and methodology section. However for more clarity we will add this in other relevant sections of the revised manuscript.

Comment 27: Page 3381 I would appreciate horizontal grid lines as in the previous figure. Otherwise the actual values are hard to read from the graphs.

Response 27: Noted with thanks and will be updated.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 3339, 2012.

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