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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 # 2

Comment 1: 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.

Response 1: Thank you for pointing this out. We agree that it is more appropriate to view the results in terms of the relative change. Even though we have presented results in absolute value (especially in figures) we have discussed the results keeping relative change values. However we will also update the figures with relative change values in the revised version.

Comment 2: 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 basis compared to others. Dispersion diagrams (delP/P,delT) 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.

Response 2: We used only one RCM i.e ECHAM-4 with two scenarios (A2 and B2) because it is being used by Mekong River Commission for climate change studies and this RCM covers the entire Mekong River Basin. But we completely agree that for deriving more reliable conclusions more GCMs should be considered. 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.

Comment 3: 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- soka and Chiew, 2009, J of Hydrometeorology) or quantile mapping (Themeßl et al.,2010, Int. J. Climatology) would have been more appropriate.

Response 3: The delta change method has also been considered as simplest form of downscaling method by scientific community. Yes we agree to reviewer's point that this method is not appropriate with respect to the modification of extremes as it only scales the mean, maxima and minima of climatic variables, which is one of the limitations of this method. One of the reasons for choosing this method for our study is that this method has been used as a downscaling technique by Mekong River Commission. Since the delta change method is inappropriate for extremes we limited our analysis to mean monthly and annual basis only. In the revised version, we will discuss this in a relevant section and also discuss more appropriate downscaling techniques as suggested (e.g. Themeßl et al.,2010, Int. J. Climatology). There is also a recommendation from reviewer # 3 on this aspect.

Comment 4: 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 dowscaling 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 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.

Response 4: We agree that multi-model approach is now recommended and kind of state-of-the-art, although this is certainly also not an optimal solution. However, our study shows one of possible many, more or less equal likely futures (as pointed out by Reviewer #1). But we completely agree that for deriving more reliable conclusions, different GCMs as well as greenhouses gases emission scenarios should be considered. Therefore, to address the issue of multi-model/ multi-scenario 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.

HESSD

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Interactive Comment

Regarding the use of other downscaling technique please kindly refer to Response 3 above.

Regarding the uncertainty that exists with the hydrological model we have conducted uncertainty analysis of SWAT model and discussed the result, but we have not considered the uncertainty in climate change results. As we have already made it clear above, we will add more GCMs in the revised manuscript which will allow us to a large extent to indicate the uncertainty in future projections by GCMs and to add more discussion on this. We will include more detailed discussion on the effect of model uncertainty in impact results in the revised version. We will also include the discussion on how results might be affected when using some other sediment yield models.

Comment 5: Abstract: the abstract does not clearly state that only one climate model was used.

Response 5: Thanks for pointing this out. We will state the number and name of climate models used in abstract of the revised version.

Comment 6: 3345-18: Why not use the entire SCU dataset instead of using two sources.

Response 6: The Santa Clara University (SCU) data are gridded data available at 0.5 deg resolution. Therefore, we believe that the observed (in-situ) data should be used where available. We only used the SCU data to extend the data time series by deriving relationships using the observed data from the available period.

Comment 7: 3346-25: Why the delta-change method? Because it is simple or because it is the most appropriate?

Response 7: Please kindly refer to response 3.

Comment 8: 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?

Response 8: We used SUFI-2 for automatic calibration. In order to run an automatic calibration, the parameters that are to be calibrated (most sensitive ones) and their initial values and ranges needs to be specified. To do this we used manual calibration. So, it is like a pre-calibration or rough calibration manually followed by further refinement through automatic procedure. For the case of sediment, we also followed the similar procedure (i.e. pre-calibration manually followed by automatic calibration), but in this case the automatic calibration could not improve the overall performance than that was obtained by the manual calibration. Therefore, we retained the parameters from the manual calibration. Streamflow and sediment yield was calibrated separately because as required by SWAT the stream flow needs to be calibrated first before calibrating for sediment yield. Of course some of the parameters for stream flow also influence the sediment yield. Therefore while calibrating for sediment we only calibrate the parameters that only influence the sediment yield but not the stream flow. We will clarify this in the revised version.

Comment 9: 3350-14to25: This part was not clear for me. Additional details are likely needed.

Response 9: Here we described how the uncertainty is analyzed in SUFI-2. The initial description of uncertainty analysis by 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). Because SUFI-2 is relatively new method we will add more description in the revised manuscript, which was also suggested by Reviewer 1.

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.

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Interactive Discussion

Discussion Paper

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.

Comments 10: 3355-3to5: not clear. I do not see how uncertainty in the conceptual model can be accounted for.

Response 10: This comment is partly linked to the uncertainly assessment procedure applied, i.e. SUFI-2. We have added some clarification of this method above in Response #9. In SUFI-2, the final uncertainty (or loosely referred to as "total" uncertainly, meaning all sources of uncertainty related to the model and data used) is expressed through two parameters P-factor and R-factor. The P-factor which may vary from 0 to 100% indicates the percentage of observed data falling within the 95 percent prediction uncertainty (95PPU) band calculated at 2.5 and 97.5 percentiles of the model output based on Latin hypercube sampling (Masih et al., 2011). The R-factor is the average width of the 95PPU band divided by the standard deviation of the observed data. In that sense the conceptual model uncertainty is a part of the total uncertainty expressed through these two parameters (P- and R-factors). However, we would like to use the terminology "total" uncertainty loosely and with cautious. Therefore we also understand the reviewer's concern here. We will update our discussion accordingly 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.

Comment 11: 3357-18to19: this is quite expected when using the delta-change method. "Overall, the mean annual sediment cycle follows the trend of the mean annual discharge cycle".

Response 11: Here we are trying to indicate that in general increasing in flow discharge will increase mean annual sediment load, while decrease in flow discharge will decrease mean sediment loads. Such kind of conclusion has also been reported by Phan et al.(2011) where they used LARS-WG and statistical downscaling method for climate change impact study in the same region as our study area. We agree that it is in general expected.

Phan, D. B., Wu, C. C., and Hsieh, S. C.: Impact of climate change on stream discharge and sediment yield in Northern Viet Nam, Water Resour., 38, 827–836, 2011.

Comment 12: 3359-3: I do not see that in the results “Nevertheless, the results indicate that the change of sediment yield and discharge in response to climate change do not always happen in the same direction”.

Response 12: This is based on some differences in some months (particularly Nov and Dec) with respect to +ve and –ve changes observed in the stream discharge and sediment, which are discussed in page 3358 lines 13-23. Looking at these results again (Fig 7a and 8a), we have to say that these differences are relatively small, which could well be the result of the model uncertainty. Moreover, these results might also differ as we add more GCMs for the revised version. We will update this discussion accordingly.

Comment 13: 3374: location of basin cannot be seen in black and white

Response 13: Thanks for pointing this out. We will revise the location of basin in color.

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