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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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Review of Shresta et al. 2012 HESSD – "Impact of climate change on sediment yield in the Mekong River Basin: a case study of the Nam Ou Basin, Lao PDR"

The manuscript presents a case study of assessing climate change impacts on sediment yield in a sub-catchment of the Mekong basin. In general the manuscript is technically sound in terms of techniques used and conclusions drawn on the presented results. However, there are three points I would ask the authors to put their attention to. Two of them concern technical/scientific aspects of the presented work, and the

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final one the limitations of the study.

1. Temperature correction

The derivation of the daily maximum and minimum temperatures is not clearly presented. If I understand it correctly, you 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.

2. Parameter and uncertainty estimation

I would urge the authors to explain the calibration and uncertainty estimation procedure in more detail, because a) the method (SUFI-2) is not that well known as e.g. GLUE, and b) the results as they are presented give rise to a number of questions, in particular Table 1: âĂć 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. âĂć What is the meaning of the percentage values of the fitted parameters,

that are 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. âĂć Are the parameters with fitted single values less sensitive that the ones with ranges and thus fixed? But since you list these parameters as the most sensitive, I assume this is not the case. So, why have they single values? âĂć 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 possible model parameterizations according to Table 1 shown in the figures? âĂć Are the uncertainty estimates derived from model runs, sampling parameter values from the given ranges? How many model runs were performed anyway? Some clarifications are needed in order to enhance the readability of the manuscripts and to understand/interpret the results.

3. Use of a 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.

In the attached annotated manuscript these points are marked with red comments. In addition there are some other comments (in yellow) the authors should consider.

Please also note the supplement to this comment: http://www.hydrol-earth-syst-sci-discuss.net/9/C1384/2012/hessd-9-C1384-2012-supplement.pdf

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