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Interactive comment on "Uncertainty in climate change projections of discharge for the Mekong River Basin" *by* D. G. Kingston et al.

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Reply to interactive comment by G Lacombe (Referee)

We thank the reviewer for the constructive comments on the paper. Our responses to the specific issues raised are given below.

P 5992, L5: "We quantify uncertainty in these projections associated with GCM structure". The authors did not linked uncertainties to GCM structures but rather assessed the uncertainty through a comparison of projected rainfall and temperature time series. Therefore, I suggest modifying this assertion so that it better reflects analyses that are actually presented in the main text.

- This paper reports on differences in future Mekong discharge between climate scenarios produced by GCMs with different structures. As such, this study provides information on uncertainty associated with GCM structure. However, as this analysis of GCM structure is implicit (rather than explicit) we will modify the phrasing to better reflect this. Additional text will also be added to the introduction to better clarify the experimental structure in this regard.

P 5993 L 16: The reference (Hapuarachchi et al., 2008) is inappropriate here as it is a research paper on hydrological modeling which mentions demographic statistics in the introduction only. L 20: Again, the reference (Costa-Cabral et al., 2008) is not appropriate here as it deals with hydrological modeling rather than the vulnerability of fisheries and other water resources. However, I would have expected to see this reference in another paragraph of the paper (introduction or discussion) as it is one of the few research work that undertook hydrological modeling at the Mekong Basin scale. L 23: Again, the reference (Hapuarachchi et al., 2008) is probably not the most appropriate one to document fishery aspects. (MRC, 2003) cited by (Hapuarachchi et al., 2008) is probably more relevant. L 25: In addition to the 2 dams mentioned by the authors, a third one (Jinghong) was completed in 2008 and a fourth one (Xiaowan) commenced filling in 2009.

- We thank the reviewer for these suggestions and additional information. The references will be modified accordingly and the text will be revised to include reference to the two more recent dams.

P 5994: L 1: (Li and He, 2008) based their analysis on water level measurement only. In order to better document your assertions, you may also mention the following references that looked at sediment loading: Kummu et al. 2010. Basin-wide sediment trapping efficiency of emerging reservoirs along the Mekong. Geomorphology 119 (2010) 181-197 and Wang et al. 2009. Sediment load estimates and variations in the lower Mekong River. River Research and Application.

- We thank the reviewer for drawing our attention to these additional studies which we will make reference to in the revised manuscript.

L 16 to 18. The precipitation elasticity of Mekong River flow is mentioned here but not further discussed. Could you verify whether your results follow this phenomenon?

- Modelled and observed precipitation elasticity of Mekong river flow has now been calculated, with both the value for modelled and observed river flow being 1.23 (the similarity of these two values provides further support for the suitability of the SLURP model of the Mekong). The elasticity value calculated here is different from the Hapaurachchi et al (2008) value of 1.99, which may reflect the different precipitation data used: Hapaurachchi et al (2008) used gauges, with very low numbers of gauges in some basins, whereas a gridded dataset was used here. It is unclear whether the Hapaurachchi et al (2008) value refers to observed or modelled river flow.

- The elasticity analysis indicates that there will be a slightly greater increase in discharge for a given increase in precipitation. However, results show declining discharge for climate scenarios, despite increasing precipitation, indicating that precipitation elasticity cannot be used to describe future Mekong discharge. This reflects the important influence of evapotranspiration in the generation of streamflow.

P 5995: L 6 and 7. This statement could contradict findings from Christensen et al, 2007, mentioned in the first paragraph of the introduction. Need for some clarifications.

- This is not a contradiction, but we agree that clarification is required. For southeast Asia, the middle half (i.e. 25-75%) of the distribution of 21 IPCC AR4 GCMs show changes in precipitation of the same sign (Christensen et al. 2008). However, Chapter 11 of the IPCC AR4 WG1 shows that not all of the 21 GCMs show increasing precipitation (Christensen et al. 2008). This clarification will be incorporated in the revised version of the manuscript.

L 17. Could you briefly explain why did you choose this HADCM3 model to investigate

the hydrological impact of progressive changes in temperature?

- The decision to use HadCM3 to investigate the impact of progressive changes in temperature was made for the entire QUEST-GSI project, and is consistent across the modelling studies submitted to this special issue of HESS (Todd et al. 2010). Whilst it would have been preferable to investigate the impact of progressive changes in temperature for the entire subset of GCMs used here, time constraints and difficulties in running SLURP in ensemble mode prevented this. As such, it was necessary to select a single GCM on which to conduct these analyses. HadCM3 was chosen as it is a well-regarded model that had already undergone extensive analysis. Given the one of the project aims of analysing uncertainty associated with climate model structure, HadCM3 was also sensible choice given the QUMP analyses on uncertainty in HadCM3 structure (Murphy et al. 2004), and the hoped-for inclusion of these runs in future QUEST-GSI analyses.

P 5996: L 5: The reference (Kiem et al., 2005) would be more appropriate here as (Kiem et al., 2008) did not assess the snow melt contribution to the Mekong Flow. They just found that the “elasticity” is not applicable to the northern Mekong River Basin.

- This reference will be changed accordingly.

L6: “34%”. I think this figure is too high. According to Mekong River flow data averaged over the period 1960-2004, the mean annual flow recorded at Chiang Saen (downstream the Lancang sub-basin) represents 27% of the flow recorded at Pakse. Could you please explain how this value (34%) was calculated?

- We thank the reviewer for pointing this out – we have re-done our calculations, and 27% is the correct value.

L 6 to 8: The sub-basins Mekong 1, Mekong 2, lower Lancang, Nam Ou and Nam Ngum were not presented earlier in the text. Why are they studied and how are they defined?

- All sub-basins were defined automatically within the SLURP programme, on the basis of topography. The delineation of sub-basins is identical to that of Kite (2001), with the 13 sub-basins defined within the SLURP model from topographic data in the USGS GTOPO30 public-domain digital elevation model (http://edcwww.cr.usgs.gov/landdaac/gtopo30/gtopo30.html). SLURP uses the algorithms derived for the USGS/University of Saskatchewan TOPAZ system (Garbrecht and Martz, 1997).

- As noted in the manuscript, the Mekong was only modelled as far as the Pakse gauging station (close to the downstream limit of the Mekong 2 sub-basin). Pakse was the lowest point on the Mekong for which sufficient data were available. As such, the model covers the Mekong 2 sub-basin, the Chi/Mun/Chi-Mun sub-basins, and the four other sub-basins noted by the reviewer. All of these sub-basins were included to provide as detailed a representation as possible of discharge variation within the overall Mekong basin.

P 5997 L 3: As station-based daily precipitation and temperature data were used for local calibration of the daily disaggregation procedure, it would be relevant to indicate how many stations did you use, with how many years in the time series?

- Station based daily data were only used to set the coefficient of variation for the generated daily precipitation data, and the standard deviation of daily temperature. The input data used to drive the Kite (2001) SLURP model of the Mekong were used from this. As noted in the manuscript, these were derived from the GSOD dataset. As noted by a number of previous studies (e.g. Hapaurachchi et al. 2008), the availability and distribution of precipitation gauges in the Mekong basin is far from ideal – from this data set, 17 stations were available for the period 1961-1990, with the majority located in China and Thailand.

L 8: The pattern-scaling technique requires that there is a linear relationship between the scaler (global temperature) and the response pattern (i.e. project rainfall or temperature from GCM). Did you verify that this assumption is valid in the case of the GCMs, period and locations that you selected? References indicate that this relationship is generally observed for temperature, but not for rainfall (Cf. Mitchell, 2003. Pattern scaling. An examination of the accuracy of the technique for describing future climates. Climatic Change 60:217-242).

- A global-scale validation of the pattern-scaling scenarios has been undertaken; the results indicate that the assumption of linearity is generally satisfactory, but may not hold for large changes in global mean temperature or where the rate of temperature change slows or even reverses. Note also that Mitchell (2003) also states that “pattern scaling is generally accurate”, and that whilst under some situations non-linearities emerge, the errors that could result from pattern scaling are small compared with many of the other uncertainties associated with the generation of future climate scenarios (see also Warren et al. 2008).

P 5998 L 18: According to your findings, snow melt seems to play an important role in the flow regime change under climate change scenarios. Therefore, could you explain in detail how snow cover, snow precipitation and melting rates are accounted by the SLURP model you are using and which snow data are used as input?

- Although a large part of the upper Mekong basin experiences seasonal snow cover, there is no substantial perennial snow cover – see Kite (2001), their Figure 3. As such, no snow cover data were provided as model input. Precipitation falls as snow when temperatures are below 0 °C and are stored at the ground surface as snow. Accumulated snowmelt was computed on a daily basis using a degree-day method (Rango and Martinec, 1995) including the cold content of the snowpack (St. Laurent, 2003). Daily melt rates varied between the start and end of the melt period for each different land cover. A daily index of snowpack temperature and the cumulative cold content of the snowpack is maintained. The snowpack becomes ripe, and snowmelt occurs, when the cold content becomes zero.

P 6000 L 17: Another cause which could explain the difficulty to calibrate the model over the period 1960-1990 is the instability of the catchment's hydrological behavior. For example, Lacombe et al. 2010. "Conflict, migration and land-cover changes in Indochina: A hydrological assessment", Ecohydrology 3:382-391 have shown that the rainfall-runoff relationship has changed in the Chiang Saen-Vientiane and Mukdahan-Pakse intermediary catchments over this period, in response to broad-scale land-cover changes. You should, at least, mention this possible instability when discussing the model calibration results.

- Agreed – we will acknowledge this issue when revising the paper.

P 6001 L 16: do you mean that UDel does not omit rainfall events in the narrow and topographically complex Lancang section? Please reformulate to avoid or confirm such insinuations.

- This is not what was meant – the sentence will be removed to avoid such confusion.

P 6001 L 24: "A good fit was also obtained for Chiang-Saen (Figs. 2b and 3b)." It is a bit exaggerated. Figure 2b does not show such a good fit between observed and simulated flow at Chiang Saen.

- According to the Henriksen et al. (2008) classification the model fit at Chiang Saen is "very good". However, further text will be added to better acknowledge the differences between modelled and observed Lancang river flow.

Figures 2a, 2b and 2c are difficult to read. Instead of displaying many monthly values, you could show annual variables such as total annual flows, min and max monthly flows.

- We thank the reviewer for pointing this out. We intend to remove Figure 2 from the revised manuscript, to be replaced by a table presenting the statistics suggested.

P 6001 L 25: "Although peak and low season discharges were successfully captured for Ubon". This conclusion is too optimistic. Although multiannual means of peak and

low flow are well captured, as displayed in figure 3c, the year-to-year variations are less well captured (figure 2c). This should be reflected in the text.

- This section will be rephrased as suggested.

P 6002 L 4: "The simulated Pakse discharge compares favourably with previously published models of the Mekong". This conclusion lacks precision. Could you illustrate your findings with some figures and/or add more detailed comments.

- We appreciate that this conclusion is somewhat imprecise – this is due to the difficulty of concisely comparing our results with those of others when a number of different metrics are used to characterise model reliability. Here follows a more complete summary of the studies referenced, an abridged version of which we propose to include in the revised manuscript:

- Differences between observed and modelled monthly flow in Kite (2001) vary between +14% and -37%; the model is described as simulating the Mekong "reasonably well".

- Nash-Sutcliffe coefficients for Hapaurachchi et al. (2008) vary from 70% to 82.5% for different sub-basins; plots of observed and modelled discharge show peak discharges are sometimes substantially overestimated.

- Isihidaira et al. (2008) describe their model as simulating Mekong discharge "quite well", although peak discharge is not always well captured.

- Observed-modelled flow divergence in Pakse and Chiang Saen monthly discharge is also evident in the study of Jaywardene and Mahanama (2002) (their Figure 4), with %RMSE at Chiang Saen of 52.01 (the value for Pakse is not given).

- The R2 value for simulated Kratie (downstream of Pakse) discharge is 0.63 in the Vastila et al. (2010) study, but with some problems of timing of the seasonal cycle of river flow (their Figure 4).

- Therefore the SLURP model presented in this manuscript, whilst not perfect, performs similarly to a number of previous models.

P 6002 L 7: “The performance of the model varies little between the calibration and validation periods at Pakse (Fig. 2a)”. This observation is not obvious in Fig2a, b and c as we don’t know the values of the Nash-Sutcliffe coefficients over the validation period. It would be more relevant to provide these values directly in the text, as it was done for the calibration period.

- Validation period Nash-Sutcliffe values are 0.77 for Pakse, 0.81 for Chiang Saen, and 0.64 for Ubon (although this latter value is only for the 1991-1993 period). These compare to 0.89, 0.77 and 0.44 (respectively) for the calibration period. These values will be included in the text of the revised manuscript.

P 6003 L 7: How this relative difference (1%) was calculated? Through the comparison of multi-year means? Prior to calculate this relative difference, did you adjust the GCM data, based on a comparison of HADCM3 data with UDel data over the base line period? In other words, I am wondering whether this 1% difference originates from long-term temporal changes or reflect a discrepancy between the two data sets that was already manifest over the baseline period?

- The 1% value refers to the difference in mean annual precipitation between the 1961-1990 baseline and the 30-year scenario mean. The scenario precipitation data were generated using a pattern-scaling procedure, which is explained in more detail in Todd et al. (2010), a companion paper submitted to this Special Issue of HESS. This method of scenario generation involves perturbing the baseline data to produce scenario values (i.e. a form of delta-change), meaning that the 1% baseline-scenario difference is reflective of a genuine change in precipitation.

P 6004: L 23 and 24: “Increasing annual runoff in the Lancang sub-basin is driven by increasing early and late season discharge”. You did provide possible explanations for the increase in early season discharge. Could you provide comments on the increase in late season discharge too?

- The late season increase in discharge in the Lancang sub-basin is thought to be associated with increasing precipitation. This is supported by this increase being present in the precipitation-only scenario (Figures 5d and 7d), but not in the temperature-only scenario (Figures 5c and 7c). This explanation for the increase in the late season discharge will be included in the revised manuscript.

P 6004: L 27: To validate your hypothesis on the role of snow melting in the early season discharge increase, I suggest that you re-run your model without the snow component and verify that there is no early season discharge increase.

- We believe that running the model with climate change scenarios but holding precipitation and temperature (in turn) constant is sufficient validation of this hypothesis. We do not quite understand the referee’s request with respect to running the model without a snow component, as this does not seem physically possible – e.g. what would happen when precipitation occurs in sub-zero temperatures? It would not be possible to run the model without a snow component; assuming all precipitation to be rain is not reasonable.

P 6007: L 15: The parameters that you selected to undertake the sensitivity analysis do not include any parameters related to the snow cover. This is surprising as snow melting seems to play an important role in the flow regime changes. Can you better explain your choices of investigated parameters and why snow-related parameters were not selected?

- Snowmelt rates were based on WMO (1986) with cold penetration factors from St Laurent (2003). There are no other parameters used in the degree-day method. The model is operated such that all precipitation that falls as snow is converted to runoff or sublimation on a daily basis. The snowmelt on any given day depends on the accumulated snowpack, the assigned snowmelt rate (mm/day) and the accumulated snow coldness. The snowmelt rates are physically based (WMO, 1986) and are not varied. As the rates are held constant when running the model, any differences in snowmelt volumes and timing are due to the different climate variables.

P 6009 L 10: The sum of linear trends should result in a linear trend as "addition" and "multiplication by a scaler" are linear transformations. In your analysis, how can you explain that summing monthly linear trends results in non-linear annual trend?

- To clarify, linear changes occur in both precipitation and temperature with rising global mean temperature, but temperature does not have linear association with river flow (river flow increases with increasing snowmelt, but decreases with increasing evapotranspiration).

P 6010 L 10: Another important source of uncertainty, which is not discussed in the paper, is the effect of climate change (changes in precipitation, temperature, PET and increase of greenhouse gas concentration) on vegetation cover and then on runoff production and actual ET. Such effects cannot be eluded as they are expected to alter basin water yields in the future (cf. Peel. 2009. Hydrology: catchment vegetation and runoff. Progress in Physical Geography 33(6): 837–844)

- We agree that land cover change is an important issue. Given data on how vegetation patterns would change we could have included this in SLURP. However, we have adopted the same approach of holding vegetation constant as employed in all the catchment modelling studies undertaken as part of the QUEST-GSI project. This is in part due to the difficulty of constructing land-use scenarios that were consistent with the prescribed-change climate scenarios. An acknowledgment that potential changes in vegetation cover provide additional sources of uncertainty will be provided in the revised manuscript.

P 6017, Figure 1: I could not find the definition of the sub-basins in Kite (2001). Therefore, you should define them in your paper.

- We thank the reviewer for pointing this out – we will provide further information on the sub-basins in the revised manuscript (as noted in response to previous comment).

Technical corrections:

P 5992, L 1: "comprises" is generally followed by a list of entities, not only "a key regional..."

OK – the text will be modified accordingly.

P 5996 L 10: Do you mean the Lower Mekong Basin?

Yes.

L 19: "from June to November"

OK.

P 5998, L 23: what "(initially)" means?

This alludes to the subsequent replacement of CRU rainfall data with UDel. We will clarify this in the text.

P 5999, L 11: "the combined Chi, Mun and Chi-mun sub-basins": No need to repeat the names of Chi and Mun twice.

Note that Chi-Mun is a separate sub-basin to the Chi and Mun basins (Figure 1). Chi-Mun is the name of the sub-basin into which both the Chi and the Mun flow.

P6001, L 27: R2 generally refers to the coefficient of determination and not to the Nash Sutcliffe coefficient.

OK.

Additional references

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