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Hydrology and Earth System Sciences Discussions

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

G Lacombe (Referee)

g.lacombe@cgiar.org

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General comments:

Using 7 GCMs, 9 global mean temperature rise scenarios and the SLURP Hydrological model, this paper investigates different sources of uncertainties in river flow projections at the Mekong Basin scale. It concludes that the major source of uncertainty comes from the GCM projections rather than the parameterization of the hydrological model. The paper indicates that, while changes in precipitation and their impacts on river flow remain uncertain, the consistent temperature rises projected by all GCM in the Tibetan headwater catchment will induce an earlier snow melting in the year, resulting in increased base flow and reduced peak flow. This paper is innovative. From my knowl-

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edge, there are no other examples of climate change studies in the Mekong region that compared flow simulated from different climate models. I think the article is suitable for publication in "Hydrology and Earth System Science" if the authors clarify the points that are listed below.

Although the earlier snow melting attributed to changes in the seasonal distribution of future flow regime seems to be a reasonable explanation, the paper does not describe the “snow” components of the SLURP model. Therefore, it is not possible to accurately assess the reliability of the results and their interpretations. Could you explain how the model accounts for snow volume and melting rates and detail which snow data are used as input. Furthermore, modelling results should be cross checked with a water/snow balance analysis. For instance, additional and reduced runoff observed from March to June and from July to November, respectively (Figure 5c), should be compared with snow volume changes assessed from actual data, even if it is a coarse assessment, given the scarcity of available data. Finally, 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.

Land cover changes that occurred over the base line period (deforestation/forest re-growth) or may happen in the future in response to climate change (vegetation response to change in precipitation, temperature, PET and greenhouse gas concentration) are totally ignored in the text. This is an important issue for two main reasons: i/ land cover changes that occurred in the past may explain the difficulties encountered by the authors while calibrating the hydrological model; ii/future climate-change-induced land cover changes will probably enhance the uncertainties of runoff projections. These two points should be addressed in the text, as mentioned in the specific comments here below.

Future climate scenarios were generated using the pattern scaling technic. The validity of this method for the Mekong Basin should be discussed as it requires that there is a

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linear relationship between the scaler (global temperature) and the response pattern (i.e. project rainfall or temperature from GCM). Did you verify that this property is observed in the Mekong Basin, especially for precipitation?

All along the paper, linear and non-linear changes in precipitation, temperature and flow are mentioned and discussed. An additional figure illustrating this contrast between linear and non-linear changes versus spatial and temporal scales would certainly help convincing the reader about the points raised.

Some references, especially in the introductions (cf. detailed comments), are not used adequately as they marginally document the comments of the authors. I suggest the authors to identify more appropriate references which, in most of the cases, are cited in the referenced papers.

Specific comments:

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.

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

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the authors, a third one (Jinghong) was completed in 2008 and a fourth one (Xiaowan) commenced filling in 2009.

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. 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? 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. L 17. Could you briefly explain why did you choose this HADCM3 model to investigate the hydrological impact of progressive changes in temperature?

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. 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? 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 studded and how are they defined?

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? 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

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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).

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?

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.

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. 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. 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. L 25: "Although peak and low season discharges were successfully captured for Ubon". This conclusion is too optimistic. Although multi-annual 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.

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Interactive Comment

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. 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.

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?

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? 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.

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?

P 6009 L 10: The sum of linear trends should result in a linear trend as “addition” and

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"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?

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)

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.

Technical corrections:

P 5992, L 1: "comprises" is generally followed by a list of entities, not only "a key regional. . ." P 5996 L 10: Do you mean the Lower Mekong Basin? L 19: "from June to November" P 5998, L 23: what "(initially)" means? P 5999, L 11: "the combined Chi, Mun and Chi-mun sub-basins": No need to repeat the names of Chi and Mun twice. P 6001, L 27: R2 generally refers to the coefficient of determination and not to the Nash Sutcliffe coefficient.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5991, 2010.