

Hoang et al. Mekong River flow and hydrological extremes under climate change

Responses to short comments from students group

By Long Phi Hoang

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We appreciate the students' interest to take our manuscript for their peer-review exercise and to provide their short comments. We have revised the manuscript following the relevant comments and suggestions, as described below.

HESS manuscript evaluation criteria

Comment#1. Does the paper address relevant scientific questions within the scope of HESS? Yes, the study of flow regimes in the Mekong basin and their response under climate change is within the scope of HESS. The scope of this journal is comprised of three major aspects, and this study falls exactly under the third category: the study of the interactions with human activity of all the processes, budgets, fluxes, and pathways as outlined above, and the options for influencing them in a sustainable manner, particularly in relation to floods, droughts, desertification, land degradation, eutrophication, and other aspects of global change.

Response: Response not needed.

Comment#2. Does the paper present novel concepts, ideas, tools or data? This paper does not present new concepts or ideas, but it does present new data. This paper repeats work that has been done before, that is, executing a hydrological impact assessment based on predictive climate change data (Västilä et al., 2010) and (Lauri et al., 2006; 2012), but the authors use the most recent CMIP5 Climate change scenarios to complete this assessment. In their use of the latest data to complete their study, they update the current understanding of the Mekong basin's behavior under climate change. While this new data could be valuable to those looking to manage the water resources in the Mekong Basin, this study is really more of a work of engineering, because new data are analyzed by existing methods. However, this article is novel in its focus on hydrological extremes, since most previous studies focused only on changes at a monthly or seasonal timescale.

Response: We agree with the students that one key novelty of our research is its focus on hydrological extremes under climate change. Furthermore, we have revised the manuscript to highlight that the study, although taking a similar approach as other impact assessment studies, does convey new ideas and messages concerning understanding and managing hydrological extremes in the Mekong basin. As mentioned throughout the abstract, results, discussion and conclusion sessions, the study focused strongly on hydrological extremes and reported robust evidences of substantial increases in both high flow and low flow conditions. Given these, we also recommend a shift towards a stronger focus on quantifying and managing hydrological extremes in the discussion and conclusion sessions. To our knowledge, such shift in focuses has not explicitly addressed in earlier literatures due to uncertain impact signals from earlier CMIP3-based assessment.

Comment#3. Are substantial conclusions reached? There are two major conclusions reached in the article: The first is that temperature, precipitation, and discharge will all increase under climate change, but the variation between models highlights the need to be prepared for a variety of different scenarios. The second is that it is necessary to use an ensemble approach in hydrological assessments, to correct for the considerable differences in outcomes from the use of different GCMs. Neither of these conclusions is particularly groundbreaking, but it is certainly valuable to verify the behavior of the Mekong basin under climate change using the updated data.

Response: We think that the students seemed to miss one additional major conclusion of this study regarding substantial changes in hydrological extremes under climate change. We have added more text to further highlight this important conclusion in the manuscript.

Comment#4. Are the scientific methods and assumptions valid and clearly outlined? The outline is made very clear in the introduction, and the methodology used in setting up the model was very clearly explained and justified with citations of other similar work, particularly (Lauri et al., 2006; 2012). The use of different climate models was explained and the choice of models was clearly justified in the discussion section.

Response: Response not needed

Comment#5. Are the results sufficient to support the interpretations and conclusions? Yes, but it is worth noting that the conclusions are very broad, discussing general trends in the Mekong watershed and their general implications.

Response: We have further elaborate and sharpen the conclusion section, taking the this comment into account.

Comment#6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? The article explains the methodology in depth, although it relies heavily on citing other studies (Lauri et al. 2006 and 2012) for more precise details of how exactly the model was set up.

Response: Since VMod is a standard, state-of-the-art distributed hydrological model's with detailed technical description in Lauri et al. (2006, 2012), the manuscript focused more on describing its applications to the Mekong Basin case, including substantial information on model calibration and validation.

Comment#7. Do the authors give proper credit to related work and clearly indicate their own /original contribution? Yes. They clearly explained and credited the previous work on which their model was built, and noted what aspects of their work and results were new or different from previous studies.

Response: Response not needed.

Comment#8. Does the title clearly reflect the contents of the papers? Very clearly.

Response: Response not needed.

Comment#9. Does the abstract provide a concise and complete summary? Yes. In general, the abstract was excellent, but it was not clear why the authors chose to note the annual change of +5% and +16%. This is the only numerical data presented in the abstract, but it does not appear to be the most important data in the article, and it doesn't actually add any substance to the abstract. For these reasons it may be preferable to remove this from the abstract.

Response: We thank the students for their comment on the inclusion of the hydrological impact signal range in the abstract. We have added our rationales behind this range in the abstract to illustrate a robust increasing trend in the Mekong's hydrology under climate change. This is an important finding and message that we would like to convey in the abstract, especially when earlier studies typically reported uncertain and contrasting impact signal under the older CMIP3 climate change projection.

Comment#10. Is the overall presentation well structured and clear? Overall, yes, the presentation is clear. One suggestion would be to move the section 2.1, which explains the characteristics of the watershed, into a new section titled "Study Area."

Response: We have moved this section out of Section 2 (Method) and put it in a separate section.

Comment#11. Is the language fluent and precise? Yes, very well written, except on page 11658 line 12, there is a "u" missing in "rain gauge".

Response: Corrected.

Comment#12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes, all formulae, symbols, abbreviations and units are correctly defined and used.

Response: No response needed.

Comment#13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, the introduction could be reduced because it is repetitive in its discussion of water resource scarcity and impacts of climate change on socio-economic development. For example, on page 11653, lines 6-7, the authors say "Both demographic and economic trends imply an increasing importance of water resources for future socio-economic developments. (Pech and Sunada, 2008; Hoanh et al., 2010; Keskinen et al., 2010)." In the next paragraph, lines 11-13, they repeat this sentiment "Socio-economic developments in the Mekong River basin, however, are facing critical challenges relating to water resources, including hydrological changes caused by climate change (Keskinen et al., 2010; MRC, 2010; Västilä et al., 2010)."

Response: We have shortened the introduction session by (1) making the text more concise and (2) removing redundant information from the introduction. Regarding the similar sentences pointed out in the short comment, we have now removed the former sentence.

Comment: Figure 2 could be clarified by adding a time scale to the x-axis.

Response: We have added a timescale to Figure 2

Comment: Table 3 should instead be presented more like Figure 6. In Figure 6 we are able to see what each of the models predict, unlike in Table 3 where we can only see the ensemble mean, and then the minimum and maximum change for each station.

Response: We thank the students their suggestion to modify Table 3, however, we would like to restrain from modifying this table. We use Table 3 to give an overview of projected discharge change and the projection's range. We thus focus on presenting the ensemble means, and the ranges instead of describing the differences amongst individual scenarios and GCMs. We think that these differences are sufficiently described in Figure 6, Figure 7, Figure 8 and associated text.

Comment: Figure 5 is too small, it is very difficult to see and compare the different curves shown in the graph. In addition, showing the relative discharge change as a percentage is misleading because it shows that there are enormous changes taking place between January and April, when in reality, there are just small fluctuations in low flows. It would be better to display this information as absolute change, not as a percentage (or eventually both absolute and relative), because the reader is really interested in knowing where the large amplitude changes are taking place.

Response: We have enlarged this figure to improve readability. By enlarging the figure panels, the lines as well as the projection range are now more visible. Due to limited space, we now moved the percentage change panel to the supplementary note. Regarding data presentation as absolute and percentage change, we have sufficiently report the result in absolute values throughout the text. Additionally, we think that reporting relative discharge change is actually highly relevant, especially in the dry season. A reduction in low flows, which can have serious implications for ecosystems and agricultural activities, could be masked out when reporting in absolute terms since it is too small compared to the absolute changes in the wet season.

Comment: Abstract: In line 5, the authors say that this is “one of the first” hydrological impact assessments. If they say this, they should reference the other studies that completed assessments with CMIP5. However, because they don’t have space (and it would be inappropriate) to reference multiple studies in the abstract, this phrase should not be included.

Response: We are not sure if we fully understand this comment. In this sentence, we would like to stress one of the research’s innovations to use updated climate change projection.

Comment: In line 7, 11652 “(i.e. high and low flow conditions)” is really not necessary because it is explained later in the paper and most readers will understand what “extremes” means.

Response: Since the abstract should efficiently communicate the paper’s essences as a standalone document, we would like to clearly explain what specific hydrological extremes are addressed in the research.

Comment: In line 10, page 11652, the authors present the annual change between +5% and +16%. This is the only numerical data presented in the abstract, and it doesn’t necessarily seem indicative of the overall results of the study. Later in the article the authors seem to focus on seasonal change, but they choose to present the annual change here. This phrase should be removed from the article.

Response: We would like to retain this important piece of information in the abstract to illustrate that the overall trend is an increase in river flow under climate change at all considered mainstream station. This is a new finding (trends remain uncertain with contrasting directions in earlier studies) and we think it needs to be stressed in the abstract.

Comment: In the discussion and conclusion sections, the authors discuss the fact that certain areas show a reducing signal, and they remark that certain GCMs show considerable differences in precipitation changes and measures. While the authors devote considerable space to discussing these differences later in the article, and draw substantial conclusions from these specific results (i.e. saying that an ensemble approach is required for future hydrological assessments), they say very little about the implications of these uncertainties here.

Response: We thank the students for their useful recommendation on further discussing implications of rainfall variability and reduction at some areas in the lower Mekong. We have added more text in the discussion to sufficiently discuss the implications.

Comment#1a. Introduction: The introduction is very repetitive. It does a good job of justifying the need for study by explaining the socio-economic challenges posed by climate change. However the authors repeat their ideas in this section and present more information than is really necessary to explain the motivation for the study.

Response: Please refer to our response to your similar comment above (i.e. comment#13), where we have shortened the introduction section.

Comment#2a. Methodology: Section 2.1 is a description of the study area and the hydrologic characteristics of this watershed. This section should not be in “Methodology.” It should be in its own section or perhaps a subsection under the introduction called “Study Area”.

Response: Please refer to our response to your similar comment above (i.e. comment#10). We have moved the site description to a separate section.

Comment: The hydrological model described in section 2.2 calls for the maximum, minimum, and average air temperatures. However, Figure 3 shows only the projected average change in daily mean temperature. It could be interesting to see the projected minimum and maximum temperatures as well.

Response: We thank the students for their suggestion. However, because we found that projected minimum and maximum temperatures are highly consistent with the average temperature. We therefore think that providing such information would be redundant.

Comment: In section 2.3, it could be interesting to have a figure that shows the locations of the gauges used in the APHRODITE data set.

Response: We would like to restrain from providing such figure to avoid distracting readers from the paper’s main objective: Changes in flow regime and hydrological extremes under climate change.

Comment: Also in section 2.3, page 11660 line 13, the authors state that 2 degrees Celsius is an unrealistic target, but in Figure 3, several of the models show predicted daily mean temperature changes of more than 3 degrees Celsius. Therefore, it seems that 2 degrees Celsius, and the RCP2.6 that they eliminated based on their assessment, should be included as a realistic scenario.

Response: We thank the students for noting the ambiguous sentence. We have revised this sentence to better motivate our selection of the Representative Concentration Pathways.

Comment#3a. Results: In section 3.1 it would be useful to show the equations used to calculate the NSE and associated biases.

Response: We have now added the equations to calculate the Nash-Sutcliffe efficiency and the discharge biases indices in the supplementary material S1.

Comment: In section 3.2, lines 18-19, the temperature patterns are discussed very generally. It would be interesting to know more about the seasonal temperature changes that were observed, or to have more information about temperature changes with different scenarios.

Response: Following the reviewer’s comment, we have added further information on temperature change to the manuscript. In particular, we added more information to compare temperature changes across the GCMs and RCPs, showing that a majority of scenarios project a temperature increase between 1.5°C and 2.5°C.

Comment: On page 11668, line 15, these two sentences could be combined to say “Including other bias-correction methods is out of this paper’s scope because our primary interest is to understand how the Mekong’s hydrology will change under climate change.”

Response: We have revised the text.

Comment: Table 3. This table provides the ensemble mean, and minimum and maximum changes in annual river discharge. However, it would make more sense to present this information in the same format as Figure 6, where we see the prediction from each model, not just the min, mean, and max. Visually, Table 6 is much better at communicating the information and allowing the reader to quickly comprehend the differences between the models.

Response: We have responded to this comment, please refer to our response in the above section.

Comment#4a. Discussion: The discussion is written more like a conclusion. It is natural that following a discussion of data, the authors may draw a conclusion or two within the discussion section. However, in this section, the authors not only draw conclusions, they also discuss the implications of these conclusions. For example, on page 11667, lines 15-19, the authors assess the implications of their results on the safety of hydropower dams. This certainly does not belong in the discussion section.

Response: We would retain the discussion points on implications of the results for water management, as also suggested by Reviewer#1 (Comment#2). We think that it is highly relevant to assess and report the implications of the key findings in the discussion section, especially when our findings are of special importance for water management in practice. Indeed, providing policy and management implications is a standard, common practice seen in a majority of similar peer-reviewed publications.

Comment: This section discusses the many different GCMs that are used in the model. The authors discuss the importance of using many different GCMs, but of the GCMs used in this model, could any of them have been eliminated? Were there any that the authors felt skewed their results in an unrealistic way? For example, in Figure 8, one of the scenarios appears to be an outlier; its values are much lower than those of all the other scenarios. Would eliminating this scenario result in a more representative ensemble mean?

Response: We understand that the students wonder if any of the GCM appears to be an outlier and thus could be removed to improve the ensemble mean. Given our primary objective to quantify hydrological changes caused by climate change, we did not focus on validating the GCMs but rather select them based on model evaluations from Huang et al. (2014), Shabeh uh et al., (2015) and Sillmann et al. (2013). All selected GCMs were evaluated as performing well for the Mekong region and thus we would restrain from removing any of the model. We do agree with the reviewers that one GCM project lower values than the other models. We think it is relevant to include this information and thus added this to the manuscript. However, despite this diversion, the impact signal (i.e. dry season flows increase under climate change) remains consistent across all the GCMs and scenarios. Lastly, we think that removing one GCM would not necessarily improve the ensemble mean, because it is not clear which model has the best projection capability.

Comment: In this section the authors discuss the possible uncertainties and complications inherent in combining multiple different data sets (page 11668). This discussion clearly explains the assumptions that they made when selecting this data and the steps they took to ensure that their data set was complete. However, this discussion may be better placed in section 2.3 where the climate data is

introduced. If the authors want to leave this information in the discussion, it would be wise to at least say something in section 2.3 to the effect of “limitations and potential sources of error will be discussed in section 4.” So that the reader isn’t left questioning the validity of this data throughout the rest of sections 2 and 3.

Response: Following this suggestion, we have added brief explanations and refers to more substantial discussion in Section 5 (Discussion) regarding the combination of historic climate datasets.

Comment: It could be interesting compare the results of this study to other applied simulations using the CMIP5 to know if similar results were found in other watersheds.

Response: We think that comparing impact signals with those found in other watersheds would not be very meaningful because the climatic and hydrological characteristics differ greatly across different watersheds. We decided to dedicate more space to compare our results with earlier studies for the same river basin (i.e. the Mekong). This comparison showed that (1) our results agree with and thus further solidifying the insights and (2) impact signals are more robust compared to earlier CMIP3-based assessments.

Comment#5a. Conclusion: Perhaps because of the conclusive nature of the discussion, the conclusion is very repetitive (for example, lines 10-15 on page 11666 are almost identical to lines 4-7 on page 11669). The authors need to revise the discussion and conclusion sections to better organize their ideas to fit into one section or the other.

Response: We have revised parts of the discussion and conclusion session. In particular, we have (1) shortened the summary of key findings in the discussion; (2) revised the first sentences in the conclusion section. All in all, we believed that the revision resulted in sharper and more concise discussion and conclusion sections.

Comment: The authors should take time in the conclusion to discuss what other types of data collection or modeling would be useful to continue to improve the general understanding of the Mekong watershed. After setting up a model and completing such a detailed analysis of this model and its data inputs, they have a unique ability to identify what sort of studies could be useful to continue this type of work.

Response: We thank the reviewers for their excellent suggestion. Given robust findings about increasing hydrological extremes under climate change, we have added to the conclusion our suggestion to shift focuses in both water management and hydrological researches. Regarding future studies, we added our suggestion to focus on low-probability but high-damage events, in order to better match with the information demand from management and policy domains.