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Discussions

Interactive comment on "Mekong River flow and hydrological extremes under climate change" *by* L. P. Hoang et al.

Anonymous Referee #1

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The manuscript in discussion presents an assessment of the impacts of CMIP 5 climate change scenarios on extreme events of river flows through the Mekong River Basin. The study uses an ensemble of 10 scenarios, which were properly downscaled, biased corrected, and used to run a well calibrated and validated hydrological model. Overall, I think that this manuscript represents an important contribution to the understanding of hydrological impacts of climate change in the Mekong, providing also a robust and replicable methodology to be used in river basins elsewhere. As the authors clearly stated, this is one of the first hydrological studies in the Mekong using CMIP5 scenarios and thus of critical value to the hydrology community of this region. Among previous studies of climate change in the Mekong and similar river basins, in fact, this study sets a new bar of standards for comparative studies to follow. In general, the paper follows

a clear outline, its justification is clear, methods well explained, and results sufficiently robust. There are, however, two major points that I suggest the authors clarify and expand on in the manuscript. First, one of the primary justifications for this study –and the use of CMIP5 scenarios–is the large uncertainty associated with previous projections. The hope is that this new set of scenarios could show if CMIP5 models and scenarios have a better agreement among them and thus decrease uncertainty in climate predictions. This aspect, however, remained largely unresolved from the manuscript. I recommend that the authors assess and discuss in more detail if the new set of scenarios are actually alleviating the uncertainty issue in comparison to the previous assessments with CMIP3 scenarios, which is a finding that clearly could bring serious implications and challenges to water managers on the ground. That brings me to the second major issue; while the authors briefly discussed some of the implications for water management, I thought that this discussion was a bit too general and short, given how crucial these findings are for the region. With the exception of a few comments, benefits and impacts to the water-depended sectors that the authors mentioned in the introduction –fisheries, agriculture, and hydropower– were largely ignored from the discussion.

In addition to these general comments, there are a number of more punctual issues that I would like to bring to the attention of the authors:

-Abstract: in the case that the manuscript is updated based on the two major comments above, be sure that those are incorporated in the abstract

-11655 l. 26: Higher precipitation amount per unit area

-11656 l. 15: None of the 3 references provided here actually documented ecosystem and/or agricultural productivity in the lower Mekong. Please correct

-11656 l. 19: Similar to the above, these two references do not relate to economic productivity of the Mekong. In fact, the Lamberts and Koponen (2008) reference will serve well in line 15 mentioned above

-11657 l. 12: What are the soil surface processes and associated methods used in VMOD? Such information was provided with regards to PET, so it is probably good to include here for consistency

-11657 l. 21: A relatively outdated land cover map was used to run the model. Are there any justifications for the use of this map? There have been major land transformations in the Mekong in the past decade and I wonder if the authors carried out any sensitivity test –as they did for precipitation– with regards to this input.

-11658: what is the justification for the calibration/validation period? The simulations were carried out from the early 1970s, time from which there are flow records that could have been used

-11658: Was the calibration done manually only? No systematic/automatic routines?

-11661: Despite the model performing very well in the lower stations, the discharge biased at Chiang Sean concerns me. There are a number of publications in recent years that have shown a significant increase in dry season flows at this exact location as a result of the construction of dams in the upper Mekong in China. Such effect could directly explain the discrepancies shown in the flood duration curves comparison in fig. 2. Based on the published evidence this seems to be a factor that the authors should considered or at least discussed about.

-11663 l. 5: Spatial variability in rainfall is mentioned here. That is a critical point that I suggest is discussed in more detail in the discussion. In particular, a reduction in rainfall in the lower Mekong could have serious implications for rainfed agriculture, which does occur in large areas.

-11664 l.8 : Following up with implications to agriculture, the authors mentioned that one of the only projections for which all scenarios agreed was a reduction in flow in June. Flows during the early wet season are critical for both ecological and agricultural productivity, bringing the first flush of water and sediments critical for growth.

-Fig. 1: For the readers that are not familiar with the Mekong, it would probably be helpful to show country boundaries

-Fig. 5: When printed in regular A4 paper, this figure is very difficult to read. I suggest using a much larger format. In addition, I felt that the ensemble mean lines was not showing a clear message; I recommend removing the mean lines from the discharge graphs in the first two columns, and in the third columns (% change), show also the error bars associated with the monthly difference among GCMs

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