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Discussions

Interactive comment on "Historical impact of water infrastructure on water levels of the Mekong River and the Tonle Sap System" *by* T. A. Cochrane et al.

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The paper aims to quantify the hydrological alteration occurred in the Mekong River mainstream water levels as well as that of Tonle Sap system. Water level data of 7 gauging stations for the period 1960 to 2010 is used for the analysis. Range of variation approach is used whereby 33 IHA indicators are estimated for the pre-and-post 1991 periods. The subject of the paper is very relevant for wide range of audience such as scientists, water managers, decision makers and donors. However, the results and discussion and conclusions are not well substantiated at the moment and require substantial revisions before the manuscript could be recommended for publication in the HESS journal. The comments and suggestions are noted below, which author

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might consider while revising their manuscript.

Major comments:

The pre-period is composed of 30 years (1960-1990) whereas post period is composed of 20 years (1991-2010). Most of the dams were constructed after 2006 period. Therefore the present analysis of the post period is inherently constrained by two factors: (i) the two records are not well comparable because of unequal temporal division and lack of data in the post period, (ii) the considerable impacts of dams, if any, is most likely in the last 10 years of data. Furthermore, these two factors are not independent from each other, which further complicate a straightforward analysis and description of the results. Therefore, a proper analysis of these two factors is essential in order to put the results in a proper context. At the moment analysis on climate is missing. Authors give section 4.2 where issue of climate variability and change is discussed from previous studies, but, this section does not give any clear explanation of the presented results. Therefore, it is recommended that authors should conduct an analysis of climate for the corresponding study period (1960-2010 including an analysis of pre-and-post 1991 periods). Ideally this should be done using areal precipitation for each catchment upstream of the studied gauges. However, given the limitations, at least, this should be done for few climatic stations across the Mekong basin where long-term records are available. This analysis should at least cover monthly precipitation, if possible other parameters like peak rainfall, dry spells etc. The analysis on temperature records will shed light on snow flow and snow melt processes as well as changes in the evapotranspiration. Monthly precipitation assessment for the pre-and-post 1991 period is inevitable to explain the results given in Table 3 and Figure 3. For instance, mean monthly flows in the post period (1991-2010) are consistently higher for all months compared to pre-period (1960-90). This is not expected result of regulation by dams as well as abstractions for the irrigation. Similarly, high flow (1, 3, 7, 30 and 90 days) values for the post-period (1991-2010) are higher than pre-period (1960-90), which is also not expected with reservoir operations alone. Authors have attempted to explain

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increase in dry season flow attributed to dams, which seems logical. But the discussion on increase in wet season flows (monthly and floods) is missing. This warrants proper investigation of climate for both periods. It is likely that period 1991-2010 is wet compared to 1960-90, which resulted in increase in mean monthly flows during wet season as well as high flows. This is an important aspect, which should be properly analyzed and included in this paper in order to substantiate or modify the conclusions on the hydrological alterations in the Mekong mainstream and the Tonle Sap system.

Another major comment is related to the storage capacity and flow regime. Authors should explain in the results section 3.1 how the increase in storage capacity relates to the flow regime. For instance, by estimating mean annual flows from Table 1 (this column could be added to Table 1) and then comparing it with the storage capacity upstream of these gauges, it is evident that the storage is very less, especially before 2005, compared to the flow volumes. Only during 2006-2010, the substantial increase in the storage capacity could be noted from Table 2. Another important factor is to discuss the residence time of water in the dams. The operational strategies of the dams should also be discussed, especially in relation to meeting peak electricity demand, which might have high influence on altering the flow regime (e.g. rise/fall rates, reversals).

Minor Comments:

In the introduction, several studies are referred and many of them with similar subject. Authors should briefly review some of them and clarify the limitations of the related studies and what additional contribution this paper is making.

Authors attempt to link the studied IHA indicators to river ecology and fisheries. But these statements are not well elaborated. Please consider improving these parts.

The conclusions may be different after addressing the review comments. In either case, please consider adding quantitative figures on the key indicators. At the moment only percentage changes are given for some indicators. It would be valuable if changes

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in water levels are also indicated (e.g. in centimeters). This can also be done in the results and discussion section.

As indicated previously, please consider adding a column in Table 1 which gives the mean annual flow for each of the study gauge (e.g. in Billion cubic meters, BCM). It seems that Table 2 is not correct for LP and VT. Please re-check the given storage values.

In Table 4, percentage values could be rounded to integer values.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 4403, 2014.