Comments on flood pulse paper by Chua et al.

Dear authors,

I reviewed the paper, the Referees' comments and your responses carefully. I believe the Referees' comments can stimulate more depth thinking and will ultimately improve the quality of your work substantially. I generally agree with the comments and believe the paper need a major revision before its potential publications on HESS.

In addition to the Referees' comments, I have my own comments as follows.

General comments: the issue of drastic decline of floodpulse in the Cambodian floodplains has received a lot of attention due to its potential great impact on the ecosystem, fishery, livelihood in the Mekong floodplain and delta regions. This paper collated a large amount of data to quantify such decline and tries to figure out its driving factors. The findings are of significance for proper water resources management and transboundary cooperation. However, the quantification and attribution are complex for such a large scale and data shortage region. The following points should be addressed to consolidate the conclusions:

1) The major finding of the paper is local (Cambodian) anthropogenic factors are likely the main reason for the drastic decline of floodpulse. But the paper introduces the problem from upstream dams and the three study periods were divided based on upstream dam construction. This would be very misleading. When the editor read the paper, I was always looking for your evidence of impacts from upstream dams on the floodpulse because the term "mega-dam period" always reminded me to do so. Furthermore, as the local anthropogenic factors are likely the main reason, so the authors should introduce the local anthropogenic activities in more detail. I understand the investigation data may be very rare, the descriptive materials are still helpful. Also, the study period division should also consider both upstream and local factors. The period division is very important for attributing studies, a lot of studies adopted trend and abrupt changing point analysis methods. So more explanation is required for the period division.
2) Data quality is of a big concern in the Mekong studies. The Referees' comments also elaborate this issue. The authors should find some way to demonstrate it. For example, Figure 5 presents the wet season discharge on the Cambodian floodplains during the two study periods, which is very useful and indicative. Is that possible to show the annual discharge as well and use them to conduct a water balance analysis to verify the data quality? Water inflow and outflow should be balanced at a longer time scale. Or the authors can utilize the lake water storage change results at the annual scale if possible.
3) Data sharing: As the authors collated a lot of data from MRC and other sources and make efforts to clean them. Is that possible to share the collated data in some way (so the followers can easily replicate the results and go beyond)?

Minor comments:

1) The abstract can be more conclusive. The current version contains quite a few numbers but lose the informative conclusion. Also, the main reason for decline of floodpulse is not clearly stated.
2) Figure 2: is that a typical annual water level / discharge figure or a virtual one? Please check.
3) P10L184, the authors claim more area of Cambodian floodplains are now permanently inundated during the dry season. But as I can see from the Figure 3, the dry season water level is well below flood threshold (dashed line). Why the authors say "permanently inundated" when water level is not higher

than flood threshold. Please explain more.

4) P17L311, this paragraph is confusing. I cannot understand why the authors mention running dry canals when talk about the impacts of water regulation. More meaning discussion should be the storage capacity of reservoirs, the area of expanded paddy field, the water demand and irrigation amount for these expanded paddy field, etc.
5) P18L328, the authors state that water from the receding floodwaters will be diverted for irrigation in anticipation of the dry season. This statement is more like an assumption. Any evidence to support it?
6) P20L373, the authors concluded that the Tonle Sap Lake has released about 6.2 km^3 more water annually to the Mekong during 2010-2019 as compared to 1962-1972. Is that possible to find the data (e.g., GRACE) to validate these results?

Hope all the comments can help you improve the manuscript. Please be noted, the revised manuscript will be subject to another round of reviewing, which will not guarantee its final publication on HESS.

Kind regards,
Fuqiang