Dear Editor,

Thank you for your valuable suggestions. We have deeply revised major aspects of our paper following the comments by the editor and reviewers. The revised portions are in red. Generally, we have consolidated our arguments, clarified our methods and reorganised some sections. Also, we have taken the opportunity to update several sections with recently published literature on the Cambodian floodplains. We hope that the revised version is now much more clearer to readers.

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.

Thank you for the suggestion. We have included additional information on anthropogenic activities that we hope can enhance readers' knowledge of the area. While our paper argues that local factors are likely the main reason, we must still view the hydro-geomorphological changes within the context of the wider Mekong basin. Thus, we thought the separation of the study period by the three eras can allow readers to view the local changes vis-à-vis upstream changes.

Heading your advice, we have included additional justification and information on how upstream and local factors can be viewed in tandem (Section 3.2). In the discussion, we further elaborated on the competing influences of the upstream dams (Section 5.1) versus that of local factors (Section 5.2 and 5.3). Hopefully, this separation allows the reader to better appreciate the various drivers of hydrological changes in the Cambodian floodplains.

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.

We concur that our previous documentation of data sources was confusing. Thus, we have included additional information in 3.1 on how future research can obtain the same data to test our findings. At several stations, we used published rating curves from MRC et al. (2004) to estimate discharge values. Following Reviewer 2's concerns about the quality of the curves, we tested the accuracy of the curves and compared them to actual discharge as documented in Section 3.1. We found that the accuracy is

generally high among all stations (R^2 >0.99). Thus, subsequent analyses using the predicted discharge data should also be accurate.

However, it is not possible to do a water balance analysis as the editor suggested due to insufficient data. For example, we do not have physical information on the exact quantity of overland flow across the floodplains.

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)?

While we will be happy to share our data with any future works, the data that we have obtained from MRC are subjected to their data use licences and copyright. Nonetheless, future authors can register with MRC and obtain the most updated data directly from them. Non-csv data can also be accessed directly in-browser on the MRC data portal at https://portal.mrcmekong.org/home..

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.

We have rewritten the abstract and conclusion and emphasized our main findings: that upstream contributors are not the main reason for the decline of floodpulse, and that local factors should also be considered.

2) Figure 2: is that a typical annual water level / discharge figure or a virtual one? Please check.

Thank you for pointing this out. This is a virtual figure and we have updated the graph label accordingly.

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.

Apologies, we meant that the riverbanks are more permanently inundated. We have edited to as follows: "…more areas of the riverbanks are now permanently inundated during the dry season"

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.

We agree that our mention of water wastage due to poorly designed infrastructure lacks more information and might be misleading. Thus, we have removed the paragraph as it is irrelevant to our central thesis.

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?

We have provided additional information in Section 5.2 about the planting calendar in Cambodia. We drew upon various sources that describes the cropping and irrigation cycles in the Cambodian floodplains (Cramb et al., 2020; MRC, 2009; Phengphaengsy and Okudaira, 2008).

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?

To confirm our result that the Tonle Sap Lake has released more water, we used reconstructed water level data by Guan and Zheng (2021) to predict the Lake volume from 1960-1990. Compared to current lake volumes, we found that water volume has indeed decreased. Therefore, our claim that the Tonle Sap Lake is losing more water to the Mekong is valid. We have also edited 5.4 to include this justification.

References:

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Guan, Y. and Zheng, F.: Alterations in the Water-Level Regime of Tonle Sap Lake, J. Hydrol. Eng., 26(1), 05020045, doi:10.1061/(ASCE)HE.1943-5584.0002013, 2021.

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Phengphaengsy, F. and Okudaira, H.: Assessment of irrigation efficiencies and water productivity in paddy fields in the lower Mekong River Basin, Paddy Water Environ., 6(1), 105–114, doi:10.1007/s10333-008-0108-z, 2008.