We thank the reviewer for his/her comments and have responded to them accordingly. The revised portions are in red. We hope that the revised version is now much clearer to the reader.

1. This is an important transboundary issue in Eastern and Southeastern Asia that is suited for the special issue. However, I do not recommend the authors publishing the paper in the present form. The hydrological analysis, while likely time consuming, lacks hydrological rigour and ignores the findings of much past work showing that new insights of this complex system can only be gained by greater consideration of the 3 dimensional hydrological balance (e.g., Kummu et al., 2014), which was recently reiterated at by Kallio and Kummu (2021) in pointing out the limitations of the recent Wang et al (2020) analysis (and the reply of Wang et al (2021).

In Kallio and Kummu (2021), the authors compared modelled discharge (without dam operations) to actual observed discharge at Stung Treng and found an increasing trend during the dry season and a decreasing trend during the wet season. Thereafter, the authors concluded that anthropogenic changes are a better indicator of the Tonle Sap Lake (TSL) hydrology as compared to precipitation changes.

The subsequent reply by Wang et al. (2021) challenges the aforementioned conclusion. Specifically, they pointed out that the observed discharge at Stung Treng were much larger than that at Chaing Saen. Also, the water level at Kratie showed greater correlation with the inundation of TSL than at any other stations along the Mekong. These evidences presented showed that it is unlikely that Chinese dams are fully responsible for changes in the TSL.

Indeed, these studies have shed light on the complexity of the hydrology of TSL and we have acknowledged their findings and contributions to literature accordingly. Nonetheless, there are differences between these papers and our work here.

- These papers only considered hydrology at the TSL and have not considered the influence of the surrounding Cambodian floodplains. Given the close connection between the TSL and the Cambodian floodplains, it is important to consider the broader region as a whole. Focusing only on the TSL might lead to missing out of key elements in the local geography – that local factors in the form of irrigation and channel incision are important factors affecting the floodpulse in the region.
- In a way, our study imparts an additional layer of insight onto these existing works. For example, we showed through an alternative method that water infrastructures (dams, irrigation diversion etc.) upstream of Stung Treng are unlikely to be the main agent of hydrological change (Section 5.1), further extending the arguments by Wang et al. (2021) that the impacts of Chinese dams within the region is over-estimated. Furthermore, we considered hydrological records from 8 stations, more than that in Kallio and Kummu (2021) or Wang et

al. (2021). **Thus, our methods can be viewed as empirical tests to the model-driven approaches of the two papers.**

- **We stress that our objective was never to construct a hydrological balance.** To do so would require data on evapotranspiration, groundwater movement and overland flow – data that is almost impossible to obtain. **Instead, our research is solely focussed upon the quantification of floodpulse along the Cambodian floodplains using 60 years of data from 1960-2019**. Creating a hydrological balance with the dearth of data would require formulation of numerous assumptions, which would challenge the validity of the balance itself.

2. The study considered is arguably limited too, as it only considers very few spatial measurements of water depth and (sometimes potentially flawed) discharge. Further it includes rainfall from only locations. The calculations based on this limited set of information likely do not allow a full interpretation of the myriad flow processes affecting this complicated hydrological setting. In the end, the main findings are basically the same from those published by Kummu et al (2014) that uncertainty in the hydrology of the tributaries of the Tonle Sap largely prevent closing the water balance of this complex system. The other aspect preventing closure of the budget is uncertainty in the groundwater dynamics.

In our study, we considered over 60 years of water levels and discharge records across 8 different stations within the Cambodian floodplains. We hope that the reviewer can clarify what s/he means by "potentially flawed discharge". For our precipitation data, we obtained observed readings from Kompong Cham and Chaktomuk which are stations located centrally within the Cambodian floodplains. As the records are actual physical measurements, the readings are arguably not less reliable than model-based approaches such as TerraClimate or GMFD that might defer from ground truths.

Kummu et al. (2014) found that 53.5% of water in the TSL originates from the Mekong, 34% from its tributaries and 12.5% from precipitation. **Our main findings – that the observed decline in floodpulse in the Cambodian floodplain is caused mainly by local anthropogenic factors – had not been mentioned within Kummu et al. (2014) at all.** Straightaway, our geographical coverage of the entire Cambodian floodplains from Stung Treng to Neak Luong is much more extensive than the study area of Kummu et al. (2014) – just the TSL. **Therefore, our research is novel and not a repetition of Kummu et al. (2014) as what the reviewer suggests.** We reiterate that it was never our aim to close the hydrological budget and this is out of the scope of our research paper.

3. Further, the findings do not go farther than echoing the findings in the most recent works (Wang et al., 2020; 2021; Kallio an Kummu, 2021, Ng and Park, 2021). Thus, the bulk of the

conclusions are largely the information that is already alluded to in the introduction (or should be alluded to with careful inclusion of the 2021 papers). I am struggling therefore to see what the main contribution of this new analysis is other than "contributing to the discussion", but in a quality that is not quite at the level expected for HESS. Better would be if the authors invested much effort in addressing the theme of the special issue by using their "estimates" to re-enforce the issue of the complexity in determining the lake water balance and possible drivers, which are both local and transboundary in nature.

Ng and Park (2021) argues that sand-mining at Phnom Penn have caused water levels at Phnom Penn Port to decrease, and subsequently caused the TSL to shrink. Indeed, the four papers listed by the reviewer contributes greatly to discussion on the TSL. However, their focus on the TSL means that the hydrology of the larger surrounding Cambodian floodplains has been neglected. Thus, the main difference between these works and our study is that we have considered the hydrology and various anthropogenic drivers across the broader Cambodian floodplains, not just the TSL as these papers have done.

To reiterate our arguments, our study has several novel contributions:

- We quantified the decrease in floodpulse within the Cambodian floodplains (from Kratie to Neak Luong) in terms of flood parameters
- We identified from actual observed records that the reverse flow along Tonle Sap River has been decreasing from 1962-2018.
- We argue that these changes cannot be fully explained by upstream water infrastructures – local drivers of irrigation and channel incision are also important factors to be considered.

We are confident that we have not simply copied the conclusions of the works that the reviewer cited. We use a very holistic dataset and our findings are novel and of interest to the scientific community.

We disagree with the reviewer's recommendation to "determine the lake water balance" because, as we stated earlier, this is not the scope of our research topic. Again, we must stress that we are not aiming to craft a hydrological budget for the TSL as what the reviewer has suggested.

4. Also, to be publishable in this journal, much greater care is needed in telling the comprehensive story, as well as addressing the limitations of the calculations at hand--and perhaps the uncertainties in those of other studies. Currently, the message that the reader is left with is that too much attention has been focused on the role of upstream dams in the past, but again, even this focused topic is not addressed in a meaningful discourse that aligns with the specific theme of the special issue. Largely, incomplete explanations of processes related

to the other important drivers are given (e.g, how exactly downstream sand mining affects upstream hydrology).

We disagree with the reviewer that we have focussed "too much attention … on the role of upstream dams in the past". Even though our paper argues that local factors are important drivers of hydrological changes, **we must view these changes within the context of the wider Mekong Basin.** Therefore, upstream dams should be part of the discourse given its capabilities to regulate flow downstream. **A discussion without considering the impacts of upstream water infrastructure would be missing the elephant in the room – arguments on local impacts must be compared against the competing influences of upstream factors**.

We are unsure of what the reviewer mean by "incomplete explanation". The three anthropogenic factors that we investigated – upstream dams, water withdrawal and channel incision – affects the downstream hydrology of the Cambodian floodplains. Their relationships with respect to each other are not within the scope of this paper. Like the reviewer, we are also unsure how downstream sand-mining at Neak Luong can possibly affect dam operations in China.

5. Finally, I have questions regarding some of the methods, which are addressed in the points below. Importantly, one question relates to uncorrecting the corrected stream rating curve at a critical location that informs on the changes in discharge to the area in question, and on the role of dams on reducing flows. In practice one adjusts a rating curve when the old one is no longer valid. By adopting the old curve (ignoring the new curve) for the new calculations in this paper, one wonders if the authors are corrupting their calculations. Even if explained elsewhere, sufficient details are needed here for the reader to have confidence in the calculations. Nevertheless, this issue of measurement uncertainty relates to prior calls (e.g., Kummu et al 2014 and likely others) for better and more comprehensive measurements of hydrological phenomena needed to study the Tonle Sap water balance with accuracy. In conclusion, I am hoping this is a very rough draft submitted hurriedly to make the initial deadline of the special issue and that the authors are already undergoing a much more comprehensive assessment to provide an engaging, objective story regarding the issue of Tonle Sap, which is increasingly tragic.

We apologise for the incomplete documentation of our methods and will further clarify our methods in the later sections. Like the reviewer (and many within the scientific community), we support the call for better hydrological data coverage for the Mekong.

While we thank the reviewer for his/her valuable comments, we would like to reiterate that our study is not focussed on only the TSL, as the reviewer has suggested. Our study area is the entire Cambodian floodplains from Stung Treng to Neak Luong. Tonle Sap Lake (and Tonle Sap River) only represents part of the floodplain. Therefore, there is much more novel insights to be gleaned from our study as compared to other papers that only focus on the TSL.

6. The introduction seems a bit dated, not really discussing the issue based totally on what is known (or debated): a) Mekong flows are reduced and certainly have an effect on the the water levels in Tonle Sap Lake; (b) Climate has had some influence on the hydrology of the entire region (especially the recent "dry" conditions in the region); (c) intense sand mining downstream of the lake is likely a culprit in changing the flow regime of the Mekong, potentially contributing to lake level changes; (d) anthropogenic changes in tributaries above the lake affect inflow to the lake (e) dams and diversion on other mid and lower stream tributaries of the Mekong affect flows in the river as well as other rivers on the Cambodian floodplain; and (f) and agriculture intensification in the floodplains may also affect flows. If these things are "known" how can they be conclusions to the paper? What is not known are the contributions and their combined effects. Importantly, the authors need to emphasize the novelty of their findings.

We structured our introduction with the aim of setting the stage for scholars unfamiliar with the region. We agree that the issues raised by the reviewer has been known and debated by other scholars. However, full coverage of all of these issues and their impacts of the Mekong basin is out of the scope of the paper. Literature reviews such as Hecht et al. (2019); Pokhrel et al. (2018) have reviewed some of these issues and their consequent impacts.

Within the scope of our paper – the Cambodian floodplains – we recognised the various competing drivers of upstream dams, water withdrawal and channel incision and have provided additional information in Section 2 and 5.

Our key areas of novelty are presented in Line 53 to 57:

"we offer novelty in two ways. First, we studied the Cambodian floodplains in its entirety, as compared to other authors who only investigated the Tonle Sap system (Chen et al., 2021; Kummu and Sarkkula, 2008) or the Mekong system (Binh et al.,2020). Second, we synthesised knowledge of the various anthropogenic drivers in the Cambodian floodplains and associated them with observed hydro-geomorphological impacts. In so doing, we present the implications of current human activities on the Cambodian floodplains and the wider region."

7. I think once the finalized message of the paper is determined, the title and abstract can be tuned to reflect that story. Is it simply the decline in the flood pulse (two words not one) or is there more to it? The decline in the flood pulse is already known.

We thank you for the suggestion. Currently, we feel that the title/ abstract already reflects the nature of our research. Nonetheless, we are happy to change them if the editor also concurs with the reviewer.

To our knowledge, there has not been any studies that **quantified** the decline of floodpulse in the Cambodian floodplains using a more extensive range of observed data as this paper. To the stakeholders of the region, it is insufficient to say that flooding season has decreased. For example, they would want to know **exactly** how many days the flooding season has decreased by. In this aspect, this paper has provided the exact quantity the stakeholders and other scientists can take reference from.

8. The estimate of the "water withdrawal rate" from the floodplain is not believable with the simple, indirect methodology.

We are not sure what the reviewer means by “believable”. A simple methodology might not be a bad methodology and vice versa. Again, we want address the water balance is not the main focus of this paper.

9. Lines 35 to 40. The brief inclusion of the studies elsewhere are not needed here as they distract from information that is needed to explain the Tonle Sap issue.

As mentioned, our study encapsulates the wider Cambodian floodplain region, not just the TSL. We feel that inclusion of this paragraph allows our reader to better understand the importance of the floodpulse within the floodplains and wider Mekong basin. Nevertheless, we will remove this section if the editor also concurs with the reviewer that this section is a distraction.

10. Line 46. The spatial (and temporal) extent of the Cambodian floodplain area, above and below the lake, should be defined; and an explanation of the hydrological processes operating on this area is needed (what are the boundary conditions?). Make sure to refer to the map.

Indeed, the delineation of the Cambodian floodplains has been done in Section 2 and the corresponding map in Figure 1. The floodplains are the light green sections as indicated in the legend.

Again, we like to remind that the Cambodian floodplains extend beyond the TSL.

11. Line 48. A map is needed showing the Lancang dams, as well as any other dams and features referred to in the paper. I was unable to follow the story without opening Google Maps.

Thank you for the recommendation. We updated Figure 1 to show the entire Mekong Basin.

12. Line 53: The "surface hydrology" of the floodplain system was studie, but only through flow on two rivers and one lake depth. This is not comprehensive.

Sorry, we cannot find the mention of "surface hydrology" as cited. In fact, we did not use the term in the entire paper. As our intention is not the construction of water budget, our extensive coverage of the study area using surface hydrological data is sufficiently comprehensive.

13. Line 55: The "synthesis" amounts to a cursory description, but lacking support data and critical consideration.

Our discussion of anthropogenic drivers – upstream dams, water withdrawal and channel incision – is built upon data and reasoning (Section 5.1 to Section 5.3). We do not understand what the reviewer meant by "cursory" and "lacking support data and critical consideration".

14. Figure 1 Caption. Please provide more details and descriptions of important information.

Thank you for the recommendation. We have added in more details and descriptions.

15. Figure 1. Where are the areas of intense sandming and irrigation (See Ng and Park, 2021).

We have indicated possible irrigation canals in Figure 1 (grey lines) and referred to them in the map legend.

For sand mining, there is a lack of official documentation on its activities. Therefore, there is much uncertainty on the exact location of sand mining operations. Even though Ng and Park (2021) has published a map of possible mining locations (Figure A), their map do not correspond to those published by Hackney et al. (2021) (Figure B).

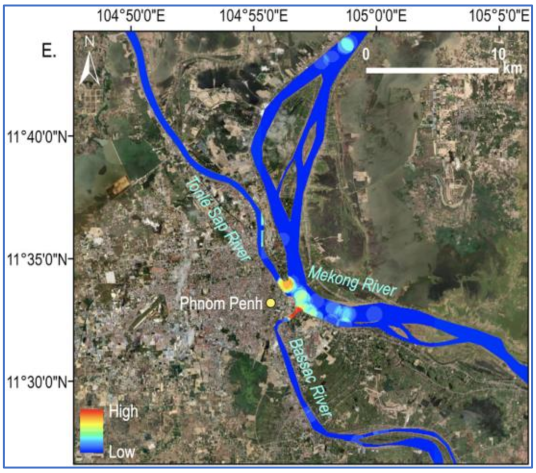

Figure A. Location of sand mining as per Ng and Park (2021)

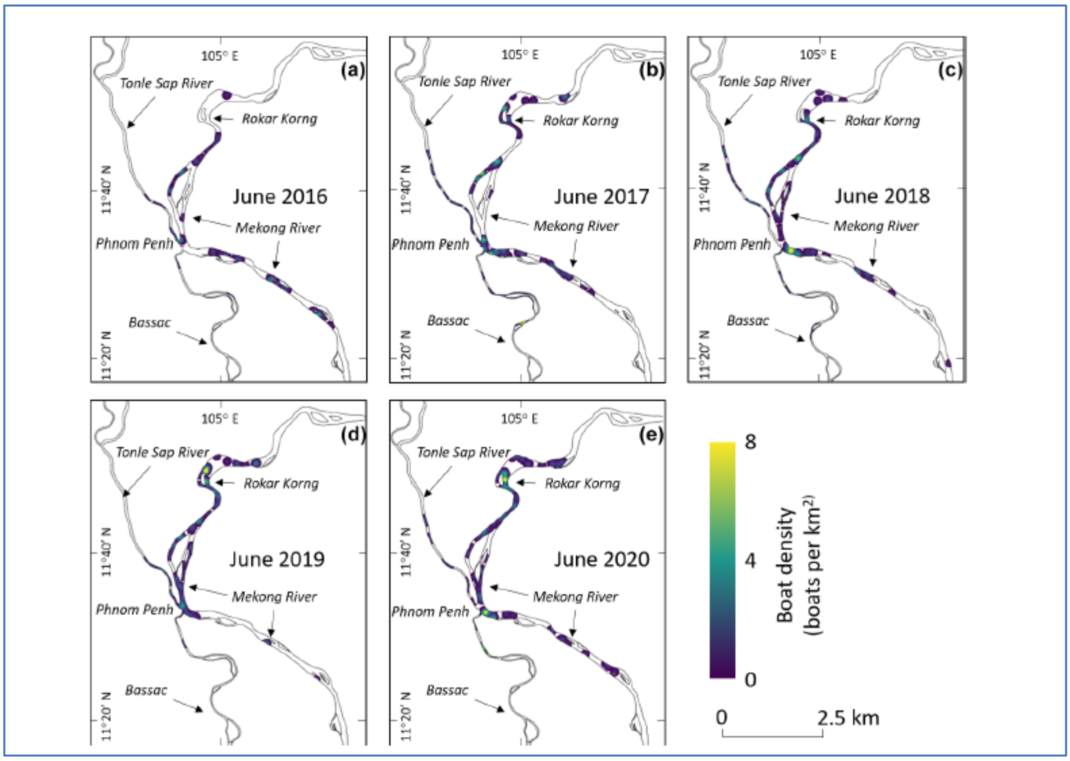

Figure B. Location of sand mining as per Hackney et al. (2021)

For instance, Ng and Park (2021) claimed that mining operations along the Tonle Sap River is minimal while Hackney et al. (2021) asserts that the Tonle Sap River is a hotspot for sand mining. These differing views make it hard to establish a consensus on the exact sand mining spot within the Cambodian floodplains.

16. One issue regarding understandability of the paper is the tendency of using "upstream" in reference to tributaries to the lake, tributaries to the Mekong in the vicinity, locations far above. Please be exact and descriptive and provide reference on maps.

Thank you for your suggestions. We meant “upstream” to be regions of the Mekong above the Cambodian floodplains, aka above Stung Treng. We have provided additional clarification in the main text.

17. Lines 79-82. Where exactly is the Chi River with respect to the lake and what are the known effects (use data)? What is meant near the floodplain and how is that more relevant than being far away when river discharge is considered? Please show the Chi and the other S3 dams on a map. What are the details regarding these dams?

Thank you for your suggestions. We have included their locations in the updated Figure 1. More details on their hydrological impacts can be found in works by other scholars (Arias et al., 2014; Cochrane et al., 2014; Piman et al., 2013).

18. Lines 83-92. It is important to show where this area is in relation to the lake and rivers, as up to 31% of the low season flow of the Bassac and Mekong Rivers could be consumed. How can this estimate simply be glossed over and not explored?

We have already indicated possible irrigation canals in Figure 1 (grey lines) and referred to them in the map legend.

19. Lines 95-100. Check out the new Ng and Park (2021) paper and rewrite accordingly.

Thank you for your suggestion. We have written a citation to Ng and Park (2021) as requested.

20. Section 3.1 Rewrite for clarity.

Thank you for your suggestion. We have rewritten for clarity.

21. Line 110. Regarding rainfall from only 2 stations: How is this representative of a huge area? Others have estimated rainfall for the region using much more data (e.g, Wang et al 2020).

While others have estimated rainfall for the region using much more data, the data are afterall, estimates. For example, the data used in Wang et al. (2020) is derived from GMFD, which in turn is based on TRMM rainfall data. While comprehensive, these data also needs to be validated against ground truth. In this aspect, our rainfall data are entirely obtained from ground stations and thus could be regarded as the most accurate source of data in the region.

The purpose of showing rainfall data is to merely illustrate the rainfall changes have not been extreme during the study period. We are not using the data for modelling or budget purpose. Even if we include additional stations, the trend is still the same as the two stations referenced in our paper- that rainfall patterns have been mostly constant from 1960-2019 (Figure C). We selected Chaktomuk and Kompong Cham stations because of two main reasons. One, they are located centrally within the Cambodian floodplains. Two, that their rainfall records are of better quality than the other stations.

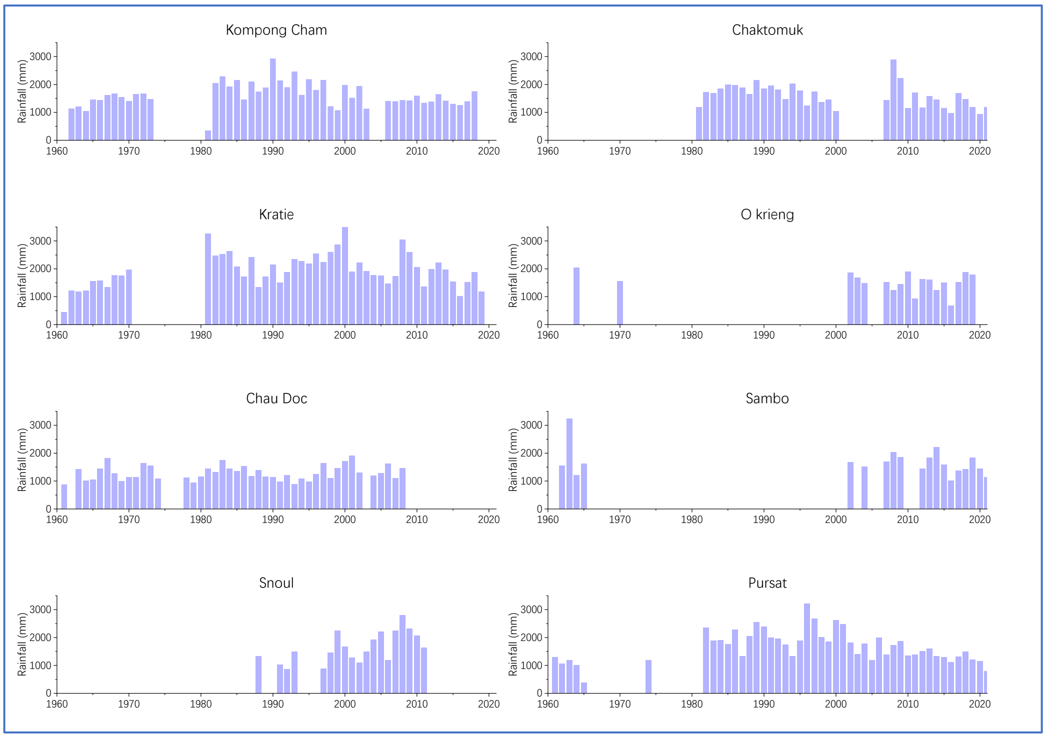

Figure C. Yearly Rainfall at 8 stations located within the Cambodian floodplains.

22. Lines 115. Please check the accuracy of the equations. Also, HESS uses numbering on equations correct? Better check. Why are the R2 values so high? What is the timing of the data (daily, monthly, yearly)?

Thank you for pointing out our mistakes. We have added numbering on our equations. The data are in daily intervals.

23. Lines 124-130. Steung Treng is the primary station used to judge flows as affected by the dams, yet the authors alter the rating curve, which makes the most recent flows higher. Why assume the curve wasn't adjusted by the operators because it was wrong and predicting too high of values for a long time?. This "adjustment" affects the validity of the assessment. THIS IS A MAJOR ISSUE that must be addressed in this issue. I didn't read the prior paper, but discussion is needed here to ensure the reader that this is not egregious data manipulation that just so happens to support the story.

At Stung Treng, a hitherto unreported rating curve was adopted on 1 January 2005, with a sudden drop of 623 cms in discharge reading from 31 December 2004 to 1 January 2005 without the corresponding drop in water levels (Figure D). Without further calibration of discharge readings from 2005 onwards, it is erroneous to compare these readings to prior data.

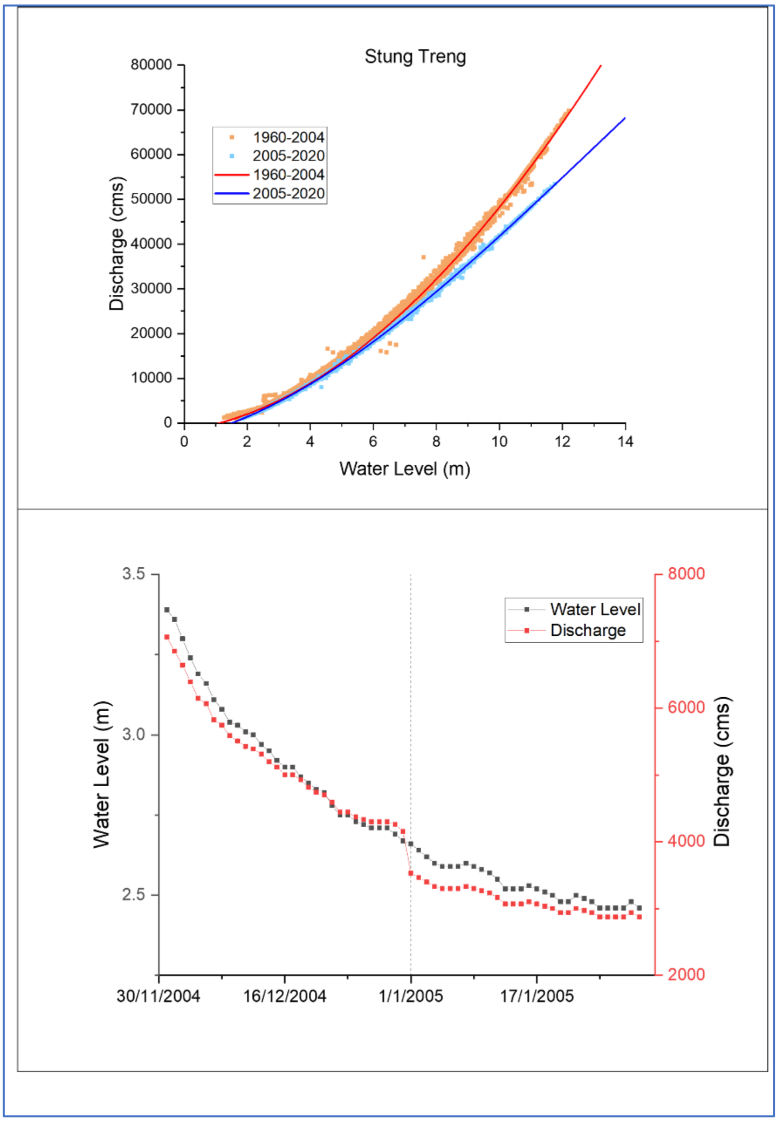

Figure D. The top graph showed a fundamentally different rating curve being employed at Stung Treng from 2005 onwards. This large change in rating curve cannot be the result of natural processes such as channel change as the timing of the change is too abrupt. The bottom graph further shows how discharge changed without a corresponding change in water level. These evidences point to the presence of potential irregularities in the official records so a calibration was undertaken. Graph source: Lu and Chua (2021).

24. Figure 2 is useful, but all the mentioned locations need to be shown on a map; and here, something is needed to identify what water body they affect and where.

The purpose of Figure 2 is to show that most of the water infrastructure developments (be it dams or irrigation projects) occur during the mega-dam era from 2010 to 2019. We have mapped the location of the dams as seen in Figure 1. However, to quantify exactly the hydrological impact of each mentioned location would be out of the scope of this paper.

25. Section 3.2.1. Give the equations numbers please. Are both THRESHOLD and FT needed? Put the units in "( )" not with "/" to make it more clear to the reader.

Thank you for the suggestions. We have made the necessary changes.

26. Figure 3. Rise rate and fall rate are not accurately displayed. What is shown are the depths. You need to show the slope of the line from the start date to the max (for rise rate) for example (e.g a 45 degree angle on the figure).

Thank you for the suggestions. We have changed Figure 3 accordingly.

27. Regarding STATISTICS. Is the use of "validated" a misprint? Have a read again on statistical inference testing for wordiing. I assume it is meant that the Welch test was used because it doesn't assume equal variances; and it was used to assess if the two samples had statistically different means.

Thank you for pointing out our mistake. We meant "conducted".

28. Line 173. Right after the PARAMETRIC Welch test is mentioned, the NONPARAMETRIC Mann-Whitney test is introduced. Why not simply only use nonparametric tests for everything?

We used parametric Welch test because we want to compare the mean of the various distribution. In contrast, Mann-Whitney test was used only because we cannot determine the "mean" of a date range.

29. Lines 176-177. Equation numbers again, and put the units in (). Actually check what HESS recommends.

Thank you for your suggestion. We have rectified our labels accordingly.

30. Section 3.2.3. I would redo this section very carefully and look at net discharge for more combinations than only 2 months. When I look at the various calculations of discharges and water depths over time I am not sure I see a consistent pattern. I would spend some time on thinking this analysis through carefully. Problematic is that this analysis only considers surface flow in three large channels, but I am guessing water is moving with more complexity. THIS IS A MAJOR ISSUE TO INVESTIGATE FOR ACCURACY.

It is wrong to say we only look at net discharge for "only 2 months". We considered the whole wet season from June to September. We are not sure what the reviewer meant by "consistent pattern". At the annual scale, the fluctuation of discharge across the dry and wet season is expected and common knowledge. What we are trying to do here is to quantify how much water is gained/lost at the Cambodian floodplains.

We think that the reviewer has misunderstood our aims. As mentioned, we are not creating a hydrological budget of the Cambodian floodplains. To do so would require much more data and calculations. Neither are we trying to model surface flow across the Cambodian floodplains. Our objective here is simply to estimate the amount of water loss/gain by considering the difference in discharge at the entrance at the floodplain (Stung Treng) versus that at the exit of the floodplain (Neak Luong/ Chaktomuk).

31. Line 207. Where exactly are the water infrastructure developments located?

The water infrastructure developments are located within the Cambodian floodplains. According to our results, their impacts on hydrology from Kratie to Neak Luong/Chaktomuk indicated their large spatial extent across the region. The locations of possible irrigation canals are shown in Figure 1.

32. Figure 3. Why only show water level and not discharge, if even in the supplement? Does one arrive at a different result if Q is used?

We showed only water level because our flood pulse parameters (Section 3.2.1) are based on water level data. Furthermore, the water level data are actual observations from ground records, ensuring that the data quality is high and reliable. Given the tight coupling between water level and discharge, we would reasonably hypothesise that similar results will be obtained if Q is used. However, when we consider changes to the channel, then the two variables will represent different meanings, e.g. Q does not indicates any channel change. .

33. Line 208-210. Reference someone who showed the extent of changes -- or make a new map.

Line 208-210 describes one of our novel findings – that the annual flood extent within the Cambodian floodplains is reduced – so we are not able to oblige the reviewer's request to add a citation here.

We have not amassed sufficient information to precisely plot the exact extent of changes across the Cambodian floodplains. Such an attempt is out of the scope of the paper. Alternatively, there are remote sensing-based works that have showed the change in flood extent within the TSL, should the reviewer be interested (Ji et al., 2018; Lin and Qi, 2017; Wang et al., 2020).

34. Lines 225 -230. Provide more details and show on maps if possible.

The irrigation canals and infrastructure are shown in Figure 1.

35. Line 235. This "hint" has been demonstrated by others and they should be cited.

Line 232 to 236 contain another one of our novel findings – that rise rates and fall rates have increased within the Cambodian floodplains – so we are not able to oblige the reviewer's request to add a citation here.

Instead, we changed the word 'hint' to 'point' to reduce ambiguity. Thank you for your suggestion.

36. Line 250. Figure 4b (maybe 5b, I have two versions of this draft). Maybe make the shaded area here light blue, as it is not the same period as the light gray one in plot a. Improves readability.

Thank you for your suggestion. We have modified the graph accordingly.

37. Lines 265 to 271. This information needs to be carefully presented. How do dams that are "being developed" affect the past flows analyzed in this paper? The dam at Stung Prasat was only 80-90% finished in May 2021. Where is Stung Sreng? All these places need to be shown on the maps. This paragraph is highly speculative.

We apologise for the miscommunication. We cited Stung Sreng and Stung Pursat as examples of water infrastructure developments along the TSL's tributaries. We do not mean that these reservoirs are responsible for the hydrological changes in the lake. As such, we have edited this section to reduce ambiguity for our readers.

38. Figure 6. Schematic of wet season discharge on the Cambodian floodplains during the pre-dam and mega-dam era. Across all stations, there is a reduction of discharge during the mega-dam era of 2010-2019. This is interesting, but the black arrows "admit" to the uncertainty of the flows and the difficulty in interpreting a few stage and Q measurements. I don't think the Q values listed agree entirely with your Qdiff values from before. Do they tell the same story? Please take a look and comment if needed.

Thank you for the concern. We cross checked the values in Figure 6, 7, 8 and Table 2 and found the readings consistent.

The Q measurements in Figure 6 are not exactly commensurate with Qdiff values – they tell different aspects of the story. Figure 6 showed how the reduction of wet-season discharge (already established in Section 4.1) varies across the Cambodian floodplains. We found that the developments upstream of Stung Treng (be it Chinese or Laotian dams) cannot fully account for the reduction in flows further downstream.

The calculation of Qdiff in Figure 7 and 8 then further supports this narrative by showing that there is an increase in water lost through the floodplains. Further analysis then pinpoints the source of this loss to be local anthropogenic factors, which we argue to be water withdrawal in the region.

39. Figure 6. What does the annual flux look like (put in SI)?

Thank you for your suggestion. We will add in the unit accordingly in Figure 6. We are not considering annual flux but only wet-season flux from June to September.

40. Section 5.2. To fully understand this potential impact the reader needs to know the location of the rice fields, and they need to know more about the rainfall: the droughts of 2015 to 2018 were prolonged and extensive. Your rainfall analysis is lacking in rigorousness as it includes only two stations and two months considered. IMPORTANT LIMITATION.

The locations of the canals have already been marked on in Figure 1. We are unsure of the reviewer's claim of a prolonged drought from 2015 to 2018. We could not find any reports that refer to it. Perhaps the reviewer meant the drought from June 2019 to August 2021 afflicting the region. Nonetheless, the 2019-2021 drought is not within the study period of our study.

For our rainfall analysis, we included daily data from 1960-2019 plotted in quarterly intervals, not "two months" as the reviewer has suggested. As mentioned, our precipitation analysis is not for the purpose of budgeting or modelling, but only to illustrate the point that there has not been major changes to rainfall trends in the Cambodian floodplains from 1960-2019. Furthermore, our analysis based on ground measurements mirrors the result by other studies that used other sensing methods (Raghavan et al., 2018; Singh and Qin, 2020; Thoeun, 2015).

41. Section 5.3. Check out the Ng and Park (2021) paper, then rewrite this section. You will need to explain in better detail the processes by which sand mining below the lake is affecting flows, then relating this to observed discharges of water into the lake.

Thank you for the suggestion.

In Ng and Park (2021), the authors described sand mining at Phnom Penh, not "below the lake" as the reviewer has alleged. Thus, the link between sand mining and TSL discharge is not what the reviewer has presumed.

Furthermore, the findings of Ng and Park (2021) differ from another similar (and reputable) research paper by Hackney et al. (2021). Thus, the exact quantification of sand mining within the Cambodian floodplains is still in debate and we feel that we should not favour the results of any author without proper considerations of its methods and analyses.

42. Section 5.4. Unless you are willing to go into detail the reference to the other systems is not informative. The attempt to look at wider implications is ok, though somewhat speculative. I think it is important to compare your finding(s) with the work of others, but it seems to me they basically reinforce what is largely known. You should discuss limitations of your approach with respect to errors and limitations.

Thank you for your suggestions. We disagree with the reviewer's allegation that our findings are "largely known". For example, to our best knowledge, our quantification of the reduction of reverse flow to the TSL has not been replicated by any studies prior.

We have added in an additional Section 5.5 that discuss the limitations and direction of future studies within the Cambodian floodplains.

43. Regarding the nature of the special issue on transboundary issues, you may want to make a small section on this aspect to add value to the paper. In particular, insights regarding social aspects of this transboundary issue is greatly missing, given the expertise on the Tonle Sap in Singapore, both present and past.

Thank you for your suggestions. Indeed, we have discussed the transboundary nature of the Cambodian floodplains. In Section 5.1, we discussed the impacts of upstream dams from China or Laos on the flood pulse in the floodplains. Through our analysis, we found that the reduction of wet-season discharge in the Cambodian floodplains cannot be attributed fully to these transboundary drivers.

In Section 5.4, we further discussed the potential impact to the Vietnamese Mekong Delta, located downstream of the Cambodian floodplains. Hopefully, through our study, we hope that readers can derive a scientific and objective view of hydrological processes occurring within the floodplains, instead of opinions commonly purported by the mass media.

44. Conclusion. Make sure to highlight new findings (explain novelty), not just summarize results. I like the attempt in the last paragraph to emphasize the importance of the loss of the flood pulse, regardless of the reason(s). I am not sure what "water harvesting" means; is it in reference to irrigation?

Thank you for your suggestion. We have rewritten the section for clarity. “Harvesting” in this sense here refers to the usage of Mekong, be it for its water for irrigation, its fisheries, its sediment or for hydropower uses.

We thank the reviewer for time taken to read our manuscript.

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