We thank the reviewer for his/her valuable comments. Here are our responses.

Regarding the research objectives that the Authors want to address, I believe they need to make them more compelling. In particular, some aspects of the objectives have answers even before attempting any analysis. Identifying anthropogenic factors while no information is provided concerning sand-mining, as an example, makes the corresponding results debatable.

We apologise for the misinterpretation here. We gave background information about irrigation and sand-mining in Section 2 because we thought that it would be helpful for readers not familiar with the region to know more about the issues that the region faces. Placing the information at the end of our analyses in the discussion section risks a confusing report for the reader. However, we would be happy to shift them to the back if the Editor also concurs with the reviewer.

We do not understand by what the reviewer means as 'compelling'. Our research covers a critical area of the Mekong basin and highlights observations that have not been previously reported. Beyond listing two anthropogenic responses (which the reviewer disagrees with), our paper also addresses implications for the Tonle Sap Basin and the Vietnamese Mekong Delta as a result of the changes in the flood pulse.

Section 4.1.1, the information and data given in this section can be easily found on the MRC website, so what is the novelty of this section? Additionally, the explanations provided in this section are vague and can be better expressed. For example, lines 174 to 175, where it is "compared to the pre-dam era from 1962-1991, ……." what is observed from figure 3d and e is that the minimum water level seems not to be higher than the minimum water level for pre-dam era for Neak Luong and Chaktomuk stations, at least it is not tangible. I would like to suggest authors revise this part slightly.

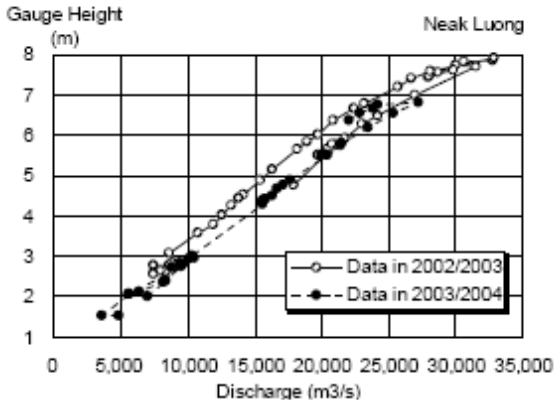
After going to the MRC website, we could not easily find the data and information as mentioned by the reviewer and perhaps the reviewer can provide us with the URL link.

On the MRC website, the latest basin report was in 2019 (MRC, 2019). Even so, the report only used data from 2000-2018. The short study duration cannot represent a historical baseline and cannot differentiate the conditions pre-dam and post-dam. As such, the changes in hydrological parameters within the latest 2010-2019 period as presented in section 4.1.1 is a novel finding. Furthermore, even recent research only used data until 2015 (Binh et al., 2020; Li et al., 2017; Yun et al., 2020) and cannot account for the rapid water infrastructure development post-2010 such as the operationalisation of the Nuozhadu Dam in China.

There is no error in our description. We conducted a Welch's t-test to compare the minimum water level values and found a statistically significant ($p<0.05$) increase of +0.10m at Neak Luong. This data is already presented in the supplement. It would be erroneous to judge the significance of change simply by visual analysis of graphs as what the reviewer suggested.

Additionally, no performance index, i.e., R^2, is provided for Neak Luong's rating curve to see how reliable this rating curve is. It seems the authors extracted the data presented in Fig. II-3-4 at MRC et al. (2004) to develop these rating curves. Although insignificant, this per se can cause errors.

The reviewer assumes that we manipulated data from the graph below:



The allegations are totally untrue and the reviewer should not make such baseless comments.

The R-square value for the said rating curve is 0.979 and is presented in the same section (MRC et al., 2004).

In section 4.2.1, the authors discussed the water exchange at the TSL in various timeframes while they are different from those defined in lines 121 to 132. Please justify.

We apologise for the confusion. The timeframes listed from lines 121 to 132 are indeed phases by which data was divided. In section 4.2.1, the water exchange at the TSL is listed in these divisions: 1962-1972, 1995-2009 and 2010-2018. The years within the three divisions are still subsets within the original phases determined in lines 121 to 132. Therefore, the timeframes used for the TSL exchange are valid.

Additionally, the average start date has been used to compare mega-dam and pre-dam eras; how do the authors justify this, while, according to the MRC, even a few days would cause a relatively significant change in accumulated flow during water exchange, let alone average date; please clarify

We would like to ask the reviewer to provide the exact reference from MRC for this piece of information. We hope that the reviewer can provide the proper source and reference for his/her arguments.

In Figure 4a, the authors presented discharge data for Prek Kdam hydrological station for various time frames as continuous data, while, according to the MRC data portal, there are significant gaps for some of these years (1964-1972 and 2011 to 2018). Would you please explain how the authors get the missing discharge data since what is observed from this figure differs from the existing data?

We obtained Prek Kdam discharge data primarily from the MRC data portal. We then cross-check gaps in the data records with records from the Ministry of Water Resources and Meteorology, Cambodia.

It is not clear what the authors refer to in line 221. According to water level data, the reduction mentioned in this part is not equal year to year. Is what the authors are referring to the “average reduction” in water level during these periods? Presenting a value/percentage for each timeframe seems not interesting as the anthropogenic and natural impacts are monitored year by year.

Please define what is ‘interesting’. Presenting year-on-year data might be interesting to the reviewer, but for others, it is interesting to understand the overall hydrological trend that Tonle Sap Lake has experienced from 1996-2019.

In lines 230 to 235, the results are compared with the published research on maximum and minimum water levels influencing the inundated area; however, those references have addressed the inundation changes as a pattern throughout the investigated periods. Also, what is seen from those references in line 231, is that various periods have been determined, which is different from the present work; how these contradicted results are justified, also a compelling discussion is required here.

Ji et al. (2018) found that both maximum and minimum water levels have decreased from 2000-2014. Lin and Qi (2017) found that both maximum and minimum water levels have decreased from 2001-2015. Wang et al. (2020) found that both maximum and minimum water levels have decreased from 2000-2018. We found that both maximum and minimum water levels have decreased from 1996-2019. While slight differences in study periods exist, the trend reported is clear throughout all 3 papers. We do not understand the reviewer’s claim of contradiction.

Additionally, the reason given for the differences in results with hydrodynamic models is also questionable. How do authors justify it when the mentioned dams have not been operated yet and the research period is somewhat different? For the explanations given in lines 238 to 241, I would also suggest investigating the flood duration year by year over each period and make a comparison between timeframes.

We wrote ‘For instance, reservoirs are being developed at Stung Sreng and Stung Pursat —two key tributaries of the Lake (ADB, 2019a)’ (line 235) This means that the reservoirs at Stung Sreng and Stung Pursat only represent two out of many, not two solely. Even so, construction of water infrastructures at both Stung Pursat and Stung Sreng started in 2011, which would have influenced the hydrology at Tonle Sap Lake during our study period of 1996-2019.

We do not question the validity of the hydrodynamic models presented by the models (Arias et al., 2012, 2014; Kummu and Sarkkula, 2008; Piman et al., 2013). We only wish to present a possible reason why the models have not accurately predicted the changes in the Tonle Sap Lake within the past decade.

While a year-by-year analysis would be interesting for the reviewer, it is unfortunately out of the current scope of the paper. If the reviewer is interested, we may suggest an excellent recent paper by Chen et al. (2021).

For section 5.1, I would also recommend using a more precise definition of variables when a timeframe is addressed. For example, one value is given for the pre-dam era, which seems to refer to the average discharge. Could the authors please confirm and readjust accordingly throughout the manuscript? Additionally, the time series discharge data is available for Stung Treng and Kratie stations for the investigated timeframes, while for other ones, discharge data exist for some years, and the authors have tried to obtain the missing data employing rating curves. Would you please explain how the discharge data were obtained for the mega-dam era for Kompong Cham station as no rating curve has been presented for this station? It seems to the reviewer that the missing discharge data for Chaktomuk and Neak Luong stations have been obtained using two rating curves given in lines 111 and 112, and please justify the reliability of these rating curves to see the accuracy of the results presented in Figure 5. According to the MRC data portal, discharge data exist for limited years for Prek Kdam, so please clarify how data were obtained for this station.

Apologies. the data were calculated from the average of wet-season discharges from June-September in the respective eras.

At Kompong Cham the following rating curve adapted from MRC et al. (2004) was used to calibrate discharge records from 2012-2019:

Kompong Cham (KC): $Q_{KC} = (8.869H_{KC} + 29.811)^2(H_{KC} - H_{CC})^{0.3}$ (R^2=0.998)

We apologies for our mistake and would include this information in our subsequent revisions.

The same concerns regarding lack of compelling discussion and relatively poor data analysis exist for the remaining parts.

We are deeply disappointed that the reviewer did not finish our manuscript based on his/her presumptive judgement that our data analysis was poor. It seems that s/he has incorrectly assumed that we manipulated our data and thenceforth, dismissed the manuscript outright. The reviewer's unsubstantiated criticism of our 'lack of compelling discussion and relatively poor data analysis' also makes it challenging for us to improve our work further.

For the equations presented in section 4.2.1, please provide the performance index, i.e., R^2, same as Kummu et al., 2014.

We thank the reviewer for the suggestion, we will include it in the final revision.

I recommend authors polish the paper as there are many grammatical errors. Some are listed below:

While some edits are helpful, we regret to inform that some of the suggested grammatical changes are in fact, grammatically wrong.

line 30, where it is "that also includes the Cambodian floodplains and the Tonle Sap system," it seems that a pronoun problem exists here. Consider removing it.

The sentence is grammatically correct.

Where it is "floodpulse" throughout the manuscript, please separate the floodpulse into two separate words as flood pulse.

We thank the reviewer for the suggestion.

In lines 34, 44, 58, 84, please correct the grammatical errors related to the used verbs and pronouns.

The sentences in lines 44 and 84 are grammatically correct.

I recommend authors use "past tense" for sentences in the "material and methods section."

We thank the reviewer for the suggestion

Line 127, where it is "with a total storage capacity no less than," the preposition is missing.

The sentence is grammatically correct.

Line 154, where it is " The parameters to characterise reverse flow (RF) where water flow from the Mekong to Tonle Sap Lake ….. , it should be "water flows from …..". Same gramtical error is seen in lines 158 and 159.

The sentence is grammatically correct.

Line 232, it appears that that may be unnecessary in this sentence. Consider removing it.

We thank the reviewer for the suggestion.

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