We would like to thank both of the reviewers for supplying such thoughtful insights and comments on our article. In order to address these concerns, we have made major revisions to our original manuscript, completing a total re-analysis using a novel modelling setup and data supplied from Hoang et al (2019) and Triet et al (2020). We have now employed a different structure to our scenarios and added additional climate change components, utilising an ensemble of 5 GCMs for both RCP 4.5 and RCP 8.5 projections.

In addition to these broad alterations to our analysis, we have also addressed each of the specific comments that the reviewers have identified, the details of which we have given below.

Thank you again for your time and consideration, and we look forward to your decision.

Response to reviewer 1 general comments:

Comment 1.1: The article mixed the impact of Cambodian or Mekong floodplain and Mekong Delta. It does not seem clear which area refer to this or both are the same.

> Reply1.1: We have now made this distinction clear in the study area section of the methods (lines 112-117) and used this terminology throughout the article.
>
> "*The study area is located in the downstream part of the Cambodian Mekong River Basin (excluding the Tonle Sap Lake region), also known as the Cambodian Mekong floodplain. The area is about 27,760 km2 and extends along the Mekong mainstream from Kratie province to the Cambodia-Vietnam border*."

C1.2: Check the definition of flood season and wet season. Or refer to the same season?

> R1.2: We have now standardised our language to only refer to a wet and dry season.

C1.3: Check the definition of the wet and dry season – from which month to which month?

> R1.3: We have now added the following text in section 2.1 (study area)...
>
> "*The wet season runs from June to October, and the dry season runs from November to May*."
>
> Where we refer to months outside of these ranges, we have stated the months in parentheses afterwards.

C1.4: The research paper seems to miss the discussion of the results and propose solution and mitigation measure to overcome.

> R1.4: We have included a fairly comprehensive discussion of the results and their implications in the discussion section, which we have now revised to reflect the change in our re-analysis. We don't feel the scope of this paper is wide enough to talk about mitigation or solutions to the drivers we investigate (climate change, hydropower development, or irrigation).

C1.5: The baseline period (1985-2008) is a bit old - consider extending it to more recent years.

> R1.5: Thanks for raising the baseline period -issue. We reflected the used baseline on literature and basin development, and decided to actually use an older time period as the baseline (1971-2000) for our re-analysis. . We assert that this longer time period is better

suited to represent a time relatively free of hydrological alterations by large infrastructure and irrigations projects. Though there were a few dams constructed by the late 1960's, most of the Mekong basin remained relatively unaffected by large hydropower through to the late 1990's as it was hampered by the cold war conflict era (Soukhaphon et al., 2021). The ecosystem and people's livelihoods have become accustomed to the natural flood cycle conditions (i.e. pre-2000) over the centuries. Thus, it is justified to have that period as a baseline. Further, using this baseline of 1971-2000 also enables us to make direct comparisons with other studies that have used the same baseline in the region (Hoang et al., 2019; Triet et al., 2020).

The justification for the used baseline is now given in the revised manuscript (page 10, lines 198-201).

"*The use of 1971-2000 as our baseline represents well the hydrological state of the basin before major alterations were introduced (Soukhaphon et al., 2021). Including years after 2000 in our baseline would introduce significant hydrological and irrigation influences that would prohibit a thorough examination of these in isolation as part of our simulations.*"

C1.6: It is not clear how the selection of climate change dataset to apply in this study. This would lead to uncertainty of result analysis and interpretation.

R1.6: We agree that this was not clear. In the revised analysis, we now use an ensemble of 5 GCMs that have been shown to be applicable in this region.

C1.7: An accurate description of general areas/places is sometimes confusing. This happens in many places throughout the article. Particular attention should be paid when revising the article.

R1.7: As part of the re-analysis we have now updated our description of the results and tried to pay attention to reducing this confusion.

Specific comments of reviewer 1

C1.8: Line 93: Add "Hydrological" condition...

R1.8: We have now added 'Hydrological' (line 121).

C1.9: Line 97: Remove "extreme"

R1.9: We have now removed 'extreme' (line 129).

C1.10: Line 99: "catchment" annual average temperature refers to the catchment of the Model or study area? Rewrite this sentence to make it clearer.

R1.10: We have now clarified that this refers to the study area (lines 130-134).

"...the annual average temperature across the study area varies from 26.9°C to 28.2°C, with mean monthly temperatures between 30°C during the hottest months (April and/or May), and 26°C in the coldest month (January)."

C1.11: Line 102&103: What is the wettest month and driest month?

R1.11: We have now added this data (line 135).

C1.12: Table 1: Add year of Topography.

R1.12: We have now added the year.

C1.13: Table 1: What is Geospatial data? Year?

R1.13: This is no longer applicable with the re-analysis, so we removed it from the table.

C1.14: Line 135-139: These indices are not common - therefore good to describe more comprehensively.

R1.14: We have now used precision and recall indices to assess the performance of the flood extent model, which we detail in the text (lines 214-218).

"*Recall evaluates what proportion (0-1) of the flood derived from remote sensing images are identified by the simulation. Precision evaluates what proportion of the simulated extent agrees with the remote sensing.* "

C1.15: Line 162-188: This section can be summarised with a table. This would improve the readability of the section. Hence, it is not sure if Table 2 is really useful.

R1.15: We have re-written this section to describe the scenarios in our re-analysis.

C1.16: Line 226-252: It is hard to read this section. Please consider rewriting it.

R1.16: We have re-written this section to describe the scenarios in our re-analysis.

C1.17: Table 3: This table may need to reconceptualise for better information visualisation.

R1.17: we changed the formatting of the table; hopefully it reads better now.

C1.18: Line 253-294: Finding in Section 3.3 looks really interesting – however a simpler presentation of the findings would help improve readability and convey key messages.

R1.18: We have now revised the entire section as well as simplified the figure to boxplot representations.

C1.19: Line 295-316: Not sure if the description of Cambodian provinces in this section is important for the international context of this journal.

R1.19: Whilst we acknowledge that this is a valid concern, we have included the provincial analysis to highlight the complexity and heterogeneity of the region, which further shows that this type of localised floodplain study is necessary in addition to the large scale basin-wide undertakings.

C1.20: Line 316: It is more appropriate to mention that your model outperformed others – but it does not mean your model is more accurate than others. Consider revising this sentence. This also applies to other parts of this section.

R1.20: This is a very good point and we have now refrained from discussing accuracy and instead compare the relevant model performance metrics (lines 390-399).

"*The model performance metrics achieved by our hydrological simulation of water discharge and water level for the baseline period of 1971–2000 at all four monitoring stations (Kratie, Kampong Cham, Chroy Changvar and Neak Loeung) exceed existing studies within the same region…*"

C1.21: Line332: Uncertainty may cause by the climate change dataset used in this study – but it may not come from the simulation of climate change of this study. Consider rephrasing this sentence.

R1.21: We have re-written this section to reflect our re-analysis.

C1.22: Line 425-426: Water-Energy-Food Nexus could not just look at the Cambodian Mekong floodplain alone. Consider revising this sentence.

R1.22: We have amended this to reflect the wider reach of the Water-Energy-Food Nexus (lines 498-502).

Response to reviewer 2 general comments:

Comment 2.1: The study is very ambitious in that it considers a large range of factors, sectors and drivers of change; also, the methodology encompasses a large set of advanced modeling tools.

C2.2: The title, abstract and motivation of the paper feature climate change prominently, along only another driver of the changes analysed. Still, the way climate change is treated in the study framework is less than optimal. Regarding the climate model used to simulate future changes in climate: one single model was used, which doesn't allow to address the relative uncertainty; the model chosen is dated (CMIP5 generation); it is not clear that the use of its results is validated through comparison with observations; GCMs are generally considered inadequate for to study hydrological processes at such fine sale over a small domain, where Regional Climate Models are more appropriate and overcome mostly shortcomings that are not negligible when looking at precipitation extremes in a monsoonal climate.

R2.2: We agree that this was the main shortcoming of our original analysis, and have undertaken a complete re-analysis in order to address this concern. Our re-analysis now includes an ensemble of 5 GCMs and two RCP levels that have a precedent for use and suitability in the region. The chosen 5 GCMs were selected as they performed best in the region (Hoang et al. 2019).

Whilst we acknowledge that the models we use are from the dated CMIP5 generation, we now justify their use. We have included an analysis in our supplementary (Table S1) that compares the mean precipitation and temperature for the wet and dry season across our study area between an ensemble of six CMIP5 models and the equivalent models from the CMIP6 generation and show that the differences are very small.

The use of newer CMIP6 generation models would require us to fully rebuild the basin-wide scenarios, which is beyond the scope of the work. And as the difference between the CMIP5 and CMIP6 scenarios is very small, it would not change the main findings nor our conclusions. Further, by using the existing basin-side scenarios, allows us to compare our results to basin-wide studies as well as studies done for Vietnamese part of the delta. Therefore, we chose to use the CMIP5 scenarios for this study but, as stated above, we now use an ensemble of GCMs.

C2.3: It seems that the paper presents the results of an advanced framework that integrates multiple types of models and uses a large variety of datasets. It is not possible to me to judge, however, whether the setup is appropriate, due to a lacking explanation of the experimental setup. In particular, it is hard to understand how each model in the set of those adopted relates to each other.

R2.3: We agree that the explanation was inadequate; apologies. We fully revised the model set-up and procedure for the updated analyses. In short, we use now a combination of three models: Mekong basin-wide distributed hydrological model (IWRM-VMmod,) whole Mekong delta 1-D flood propagation model MIKE-11 (boundary conditions from IWRM-VMod and sea level in South China sea, including detail modelling of the Vietnamese part of the delta with its canals and sluice gate) and 2-D flood duration and extent model IWRM-Sub for Cambodian Mekong floodplains, that enables detail floodplain modelling.

Also, we have taken extra care in ensuring a more thorough explanation of how each component adds to the end result (revised Section 2.2), including a conceptual diagram of the model system that we have included in the main text (Fig. 2).

C2.4: The setup of scenarios and their explanation are lacking. Mainly: the use of one single future climate scenario, a practice that is strongly discouraged in the field; and the lack of a plan to understand the effects of climate change on each of the scenarios of infrastructure development. Further, scenarios of socio-economic development (their present IPCC iteration being the Shared-Socioeconomic Pathways, SSPs) seem to matter in the analysis included, for what concerns land use and agriculture, water use, irrigation etc.

R2.4: We agree with the reviewer that the description of the scenarios was not clear enough. Having now changed the entire model setup, it also allowed us to update the scenario setting. We have given a more comprehensive description of each scenario, as well as included references to more details from the original sources (Section 2.4).

C2.5: A notoriously biased and inaccurate elevation dataset is used for the flood modeling, where improved datasets exists that are even included in other parts of the work.

R2.5: We agree that there is a mismatch between the datasets. The flood extent and duration model IWRM-Sub we used, is based on older elevation model and combined with detail bathymetry survey data. Due to how the model is constructed, partly because of the combination of SRTM and detail bathymetry data, unfortunately we cannot change the underlying DEM. As we use these 90 m resolution datasets at the aggregated 1 km resolution, any differences are unlikely to impact our results dramatically.

C2.6: Whereas the concept of transboundary water management has gradually gain firm footing in the last years and decades, this study stands in stark contrast with such universally preferred practice in that the situation downstream of the national boundary is neglected. It seems reasonable that focusing on one country, Cambodia, enables a more detailed and focused analysis, allows to neglect the effects of coastal processes and sea level rise, and may be also justified on grounds of dataset availability; but in the context of the lower reaches of the Mekong river it seems arbitrary to cut the modeling and analysis domain at the boundary with Vietnam. I do not ask the authors to repeat their analysis on a larger domain, but I suggest that this aspect should receive (concise) attention, and that the implications of the study, including any policy recommendations the authors may choose to draw, reflect recognition of this limitation. It would indeed be unwise to recommend policy based on knowledge of effects for only one of the countries in the lower Mekong, before quantifying the effects onto other territories downstream.

R2.6: This is another very good point, and one that we have tried to address in our re-analysis. With our novel setup of three models (see our reply R2.3) we are now able to take into account the sea level rise as well as the complex setup of canals and sluice gates in Vietnamese Mekong Delta. This was done by using boundary conditions to the Cambodian part model (IWRM-Sub) from MIKE 11 model (see Fig 2). MIKE 11 is set-up for the area by Triet et al. (2020) who focuses on the impact of climate change on the Vietnamese Mekong delta. Through their model application to the entire delta, the downstream effects of coastal processes and sea level rise are implicitly incorporated into our re-analysis, though we have not enlarged our focused study area nor made inferences about the impacts of our results on the wider delta region.

Specific comments of reviewer 2

Abstract

C2.7: I suggest that the first three sentences could be condensed so that the abstract can soon reach the core of the paper at hand.

R2.7: We have now condensed the first three sentences as suggested and it reads much better.

C2.8: When in the fourth sentence you mention impacts, it is not clear what these impacts refer to: which is the impacting phenomenon? I suggest you take the occasion to explain that you are scientifically assessing the implications of planned interventions in a context of changing climate (or some similar formulation).

R2.8: We have done as you suggested and rephrased this section (lines 6-9).

C2.9: In the following sentence you mention modeling, but you have not explained what type of modeling: please use some words to lay out the methods of the paper.

R2.9: We have now added this (page 2, lines 11-12):

'*distributed hydrological (IWRM-Vmod) and flood propagation (MIKE-11 and IWRM-SUB) modelling analysis*'.

C2.10: Following sentence: isn't there overlap between 'monthly' and 'sub-seasonal'?

R2.10: We agree with this comment, and now just say monthly and seasonal (lines 14-15).

C2.11: How come a 'scenario', i.e., a formulation of the future (as implied also by the use of the future tense), refers to year 2020, which is in the past?

R2.11: We have now revised our scenarios and no longer have a 2020 scenario.

C2.12: What concretely is altered, in the 'hydrological regimes': is it discharge?

R2.12: We have now clarified that this refers to 'regimes (discharges, water levels, and flood dynamics)' (line 15).

C2.13: What is 'hydropower mitigation investment'?

R2.13: This is no longer part of our scenario structure.

C2.14: I think the last sentence of the abstract is vague and not connect to the results of the study.

R2.14: We have now amended the last sentence to better reflect our findings (lines 25-27)

'*Our findings highlight the hydrological complexity and heterogeneity of this region, and demonstrate the substantial changes that planned infrastructural development will have on these ecologically fragile floodplains.*'.

Introduction

C2.15: The part on the benefits of annual (seasonal?) flooding is very important, and it's essential that it be well explained since the notion and key concept don't receive much focus in the literature. I suggest adding a sentence to explain how annual flooding improves water availability in the dry season, as this is not obvious.

R2.15: We have now expanded this section to explain about aquifer recharge (lines 45-47).

"...floodwaters play an important role in the recharging of aquifers and ensuring the hydrological connectivity of the floodplain, which is essential to maintaining ground water resources for use during the dry season…"

C2.16: Please, make reference to Fig. 1 when pointing to locations in the study area, so the reader can better follow the explanation.

R2.16: We now refer to fig. 1 in several parts during the introduction.

C2.17: Line 33: substitute comma for semicolon.

R2.17: We have done this (line 55).

C2.18: L 39: please rephrase sentence starting with 'moreover' (add a verb). Also, explain what you mean by 'energy' here.

R2.18: We have changed the beginning to 'In addition' and have removed mention of energy. (lines 54-57).

C2.19: L 41: 'Hydropower' doesn't seem a phenomenon of concept that can impact the water cycle: I think you should rather talk of 'hydropower dams/reservoirs/infrastructure' here.

R2.19: We added infrastructure (line 53).

C2.20: L 41: you mention future projections here, but don't add any detail about e.g. which climate scenario and which future time horizon these refer to: please explain better. Also, which study do these come from? I suspect they don't come from both of the studies mentioned. In the following sentences these aspects are treated more systematically, but please try to be more specific also there, e.g. in terms of scenarios and time horizons.

R2.20: We have now clarified the future projections in question for each of the examples given in the text (lines 57-74).

C2.21: L 55: you mention 'in opposition to climate change', but the reader should know what is the expected effect of climate change on the metrics treated here.

R2.21: We have removed this as it is indeed ambiguous, and the precise impact of climate change hasn't yet been clarified to the reader.

C2.22: L 59 on: is 'peak inundation area' the same as 'flood extent'? if so, please use the same term to avoid confusing the reader. Also, you express change in terms of percentage and also in terms of surface area, which prevents understanding the difference between such changes.

R2.22: We have now changed this to 'flood extent' (line 81).

C2.23: Please check that it is clear to the reader how this study goes beyond the one of Try et al. 2020a. I understand that that study's limitation is that infrastructure development was not considered?

R2.23: We have now stated that Try et al., 2020a considered only climate change in isolation, and at a different time scale (2075-2099) (lines 76-77).

C2.24: L 80: I'd leave 'important' out from this sentence, as it is a subjective judgement. Also, in the following lines, I'd avoid mentioning 'global sustainable development goals' as it seems to only serve to aggrandize the study. But this is only my opinion, please judge for yourself.

R2.24: We have removed 'important' and have changed the last sentence to read '*The results of this study may contribute to formulating adaptation and mitigation strategies to flood-*

prone areas that balance the need for flood prevention and water resource allocation against the ecological functioning of the floodplain.' (lines 108-110).

C2.25: Please keep abbreviations to the minimum necessary. E.g. UMB and MRB are almost never used.

R2.25: We agree that these are redundant and have replaced them.

C2.26: Is it warranted to dedicate virtually all attention in the introduction to the problem of flooding, and not e.g. to the problem of scarcity of water for agriculture?

R2.26: Whilst we agree that the issue of water scarcity is an important one to the region, and for that reason we have discussed it briefly in both the introduction and the discussion sections, the focus of this analysis is very much on the flooding aspects of these expected hydrological changes. Therefore we feel justified in making flooding the main topic of the introduction.

Materials and methods

C2.27: Explain MRC. Don't use abbreviations HR, TR and NE.

R2.27: We have explained MRC and changed the metrics we use for the flood extent verification, so no longer use the abbreviations HR, TR.

C2.28: It is a bit disappointing that the elevation model used is the SRTM 90 m resolution one, whose vertical inaccuracies are known. Was it not possible to access lidar data for this relatively small area, or datasets that improve on SRTM, e.g. MERIT (Yamazaki et al. 2917) which you do use in the remote sensing part of this work. Please mention this in the methods. Also, the land-use map is quite dated (is it from 2003): can you justify the (necessary?) choice?

R2.28: As we replied to Comment C2.5 , the DEM is an embedded component of the model that is combined with the survey based bathymetric data, which wouldn't be possible to separate at this stage. However, as we amalgamate the 90 m SRTM data to a 1 km scale, the difference between the updated MERIT dataset would be negligible.

C2.29: L 109 on: Please explain more clearly how each model stands in relation to the other, and what each model uses as input and what output is analysed and further used. There is mention of hydrological model IWRM-VMod, floodplain (hydrodynamic?) model IWRM-Sub, SWAT, IQQM and ISIS. Unlike stated, Table 1 doesn't describe models. Fig. S1 does a far better job at this, but the key points of the methodology should be clear without opening the supplementary material.

R2.29: As we now changed the entire modelling structure, we have included much more description of the models used, how they link together, and the driving data that is required for each, all of which is now summarised in Fig 2 as well as within the main text (Section 2.2. *Modelling structure and datasets*).

C2.30: Table 1: what is 'climate change' here?

R2.30: This has been altered to 'climate change projections of temperature and precipitation'.

C2.31: L 130: can you please explain the choice of these four stations: are these the only available? It would have seemed reasonable to have selected also a station in the tributary and distributary towards Tonle Sap, due to the complex and seasonal behavior of this river trait.

R2.31: We did not select a station along the Tonle Sap for comparison within the small scale floodplain model as the Tonle Sap discharges are a boundary condition from MIKE 11, fed to the hydrodynamic model and so are not simulated in the IWRM-Sub model.

C2.32: L 132: 'For the range adopted for performance rating see ASABE (2017).' Please provide further explanation for this. E.g., what 'range'?

R2.32: We have removed this reference as it was unclear.

C2.33: L 138: please explain better what NE is.

R2.33: We no longer report NE, instead we report recall, precision, and the ratio of the flooded extents.

C2.34: L 149 on: I cannot understand the explanation of the satellite-based images. See following points:

What are these stacks composed of, daily flood maps? Please clarify the explanation of the percentile maps: what percentiles did you take, what do they represent? 'permanent' and 'temporary' is not clear, do you mean permanent water bodies and flood waters? Why 'default' values? What is the water index? What threshold values? If the explanation is too technical for the non-expert in remote sensing (like me) to follow, please provide a simplified, though understandable, broad explanation in the main, and add technical, though clear, details in the supplementary. Also, please add necessary explanation in the caption of fig. S2, for the abbreviations and each step in the data processing.

R2.34: We have reduced and clarified the description of the flood extent map generation (lines 206-212) and clarified the abbreviations used in the Figure S2 (now S1).

"The SWMT is a Google Appspot based online application developed by Donchyts et al. (2016). A stack of Landsat (4 and 5) data was generated using SWMT from 1984 - 2000. This stack of images was then used to generate a water index map using the Modified Normalized Difference Water Index (MNDWI) (Xu, 2006) to distinguish between water and non-water areas, which were then adjusted to account for dark vegetation and hill shadows using a Height Above Nearest Drainage (HAND) map (Rennó et al., 2008)."

C2.35: L 165: Please re-think the explanation of the scenarios to see if you can make it more straightforward. Also, please explain the overall thinking behind the formulation of the scenarios: what overall questions are you trying to address with such study setup? You explain some of this in the Results, e.g., at lines 248 on, but it would seem necessary to explain this in the Methods. Some more specific points follow. What do you mean by 'definite'? It is highly confusing to the reader that you define year 2020 as a future scenario. I cannot see the reasons behind this choice: please either provide clear reasons or modify the definitions. Why didn't you add the effect of climate change to all scenarios of future infrastructure? Or, even better, why didn't you plan to simulate all future infrastructure scenarios both with and without climate change? I understand this would multiply work and results and complicate their presentation, but please discuss whether this is a warranted simplification. Are dams of the central panel in fig. S3 already realized, or are these 'plans'?

R2.35: Our reanalysis now has a different scenario setup which we have been careful to go into much more detail about (see Section 2.4 and Table 2), and have referred to the original scenario formulations where appropriate.

C2.36: L 173: there is no Pla2020 scenario in table 2.

R2.36: There is no longer a Pla2020 scenario.

C2.37: L 174: are LMB 2020 dams only two, in addition to those of 2007 in scenario BASE? If so, don't mention 'Xayaburi and Don Sahong only', or it will seem like an arbitrary choice to include only two. Further, when describing e.g. scenario Pla2020 just outline the differences from the BASE scenario, without mentioning everything that is included again. Also, clarify how factors like agriculture, land use irrigation change across years 2007, 2020, 2040: what are the sources of these datasets and what drivers and socio-economic scenarios do they presuppose? How are these factors included in the modeling, how are they parametrized?

> R2.37: These comments have been addressed by changing our definition of the scenarios, which now clearly distinguish between baseline and future hydropower and irrigation expansion in isolation as well as in combination.

C2.38: L 179: 'IPSL-CM5A-MR under RCP4.5' this requires explanation. Define (and cite?) IPCC, GCM and RCP4.5. I don't think that one model can 'represents the range of uncertainty inherent in the GCM climate change projections'. Do you mean that its results are representative of the IPCC ensemble of GCMs because they fall around the mean/median of the ensemble ones? What do you mean by 'covers monsoon seasonality': does the model successfully capture the seasonal variations in precipitation that characterize the local summer monsoon? Did you only use the results of the GCM for the scenario(s) of year 2040, or did you use them to simulate the situation at year 2007 and 2020, to assess biases and differences with the same simulations forced with observed temperature and rainfall? Was the output of the model bias corrected? What is the reference for this GCM's setup?

> R2.38: We have now changed our future climate projections used in the analysis to address these concerns. We now use an ensemble of 5 GCMs and 2 RCP levels that have been shown to be suitable for use in this region. See more at our reply R2.2.

C2.39: L 184: what does 'mitigation' here refer to? What do these plans try to mitigate?

> R2.39: We no longer use this term here are our description of the scenarios has now changed.

C2.40: Some questions I believe where not addressed: How long were the simulations of scenarios? What are the past meteorological data based on? What spatial resolution? How where the precipitation data from the GCM downscaled? How is the effect of dams/reservoirs/hydropower stations included in the simulations? What dam operation decisions and principles have been included, what is the level of confidence about these?

> R2.40: These concerns have all been addressed in the formulation of our new scenarios, with a brief description of how each represents a future development component given in the text along with a more thorough description given in the original source of the scenarios, which has been referenced extensively in the main text.

Results

C2.41: Table 3: please explain abbreviations.

> R2.41: We have now expanded the abbreviations in the caption.

C2.42: L 200: stations cannot overestimate: the model either over- or underestimates.

> R2.42: We have re-written most of this section to reflect the results of our new scenario formulations.

C2.43: L 206: what floods are presented and discussed here? Because this was not explained in the methods, the reader is not sure that this is the maximum flood extent in the 1985-2008 period, or a specific return period, or something else.

R2.43: These scenarios and what is being reported are now better described to make this clear (lines 338-343)

"*We compared year to year the impact of each development scenario against the S1_baseline (1971-2000) on the total flooded area across the study area (Fig. 5). Scenarios S2-S4 use the same driving climate data as the baseline scenario (S1), and so the variability in the impact shown is significantly reduced to produce consistent impacts for all years. Whereas scenarios S5-S12 are driven by future climate data projections, so that the variability in comparing year to year is significant.*"

C2.44: L 208: please remind the reader that the SWMT data represent the (proxy for) flood observations. Also in the figure, please from which models and datasets the images come from.

R2.44: We have re-written this section and the figure as the results have changed significantly (lines 265-272 and Fig. 3).

C2.45: L 209: it is indeed interesting that the overestimation could be (partly) an artifact in the strong difference in the resolution of the two datasets. Is there a way to test this, potentially by aggregating the higher-resolution data to a coarser resolution (in different ways) and looking at how the comparison then looks? The degree of agreement between observed and modeled floods is in any case remarkable.

R2.45: In our re-analysis this no longer seems to be the case. The modelled extent areas now match very well indeed in terms of magnitude, but there is still some discrepancy between the observed and simulated extents in some regions, though the overall coverage is a slight improvement from the previous analysis.

C2.46: L 210: what do you mean by 'scattering in the flood extent', and how would this affect the comparison with the modeled extent?

R2.46: We no longer mention this as it is not relevant any more.

C2.47: L 2020: I am not sure 'flow duration curves' are what is displayed in fig. S6, where the discharge is plotted versus exceedance probability. These are indeed often called 'exceedance probability' curves.

R2.47: We no longer include these as they were indeed vague and unnecessary.

C2.48: L 222: fig 3 is an effective way to summarize differences. But are these percentage changes with respect to the BASE scenario? Please explain.

R2.48: Yes, these are indeed percentage changes with respect to the baseline, which we have made much clearer both in the text, and the figure (Section 3.2 and fig. 4).

C2.49: L 235: this is not 'independent of climate variability'. It shows the impacts of planned developments if anthropogenic climate change were not to occur. Climate variability is a different concept. On another note, with respect to the GCM results: could you briefly specify somewhere how temperature and precipitation change in the simulated future climate with respect to the 1985-2008 reference period?

R2.49: This no longer appears in the text as it refers to scenarios that are not used any more.

C2.50: L 237: 'severely impact the hydrological functioning of the Mekong main channel' seems unwarranted wording. You should explain what 'function' the main channel has, and why the

modeled homogenization of the seasonal flows should be seen as a 'severe impact'. Maybe these sort of reasoning (also present in other parts of the Results) should be moved to the Discussion, where additional explanations and concepts can be added?

R2.50: We appreciate that this sort of reasoning would be better suited to the discussion, and whilst it no longer appears in the manuscript, we have taken this comment on board and tried to ensure that we just present the results in this section.

C2.51: L 251: I suggest to leave judgement of what is the most sustainable course of action for decision making to the Discussion or other section. Results are not the place to add these. Why would it be preferable to minimize hydrological alterations? This requires arguing that doesn't fit here.

R2.51: This section has now been completely re-written, but we agree that such discussion does not belong in this section.

C2.52: L 255: sentence staring with 'The comparisons' is unclear, please rephrase.

R2.52: This no longer appears in the text due to the section being completely re-written.

C2.53: L 257: what does this range refer to , annual maximum flood values across the period?

R2.53: This no longer appears in the text due to the section being completely re-written.

C2.54: L 260: readers will be familiar with flood being a peril and a problem that needs mitigating and reducing, so wording flood reduction as 'exacerbated' will at minimum read odd to many. I understand that in this context floods have both advantages and disadvantages, so I suggest a solution would be to exclude wording that expresses a value judgment and use more neutral and descriptive terms.

R2.54: This no longer appears in the text due to the section being completely re-written.

C2.55: L 425: why is the 'water-energy-food nexus' mentioned here, with no motivation: what would be the merit of addressing that concept and how would that be possible?

R2.55: This no longer appears in the text due to the section being completely re-written.

C2.56: In general, similar to my recommendation for Section 4.2, I suggest for Section 4.3 to stay closer to the core topics of this study, and avoid embarking in the longest possible list of things interesting about the Cambodian Mekong.

R2.56: Although many of the comments in this section of the reviewers response were not directly applicable in the new draft, we have taken on board comments such as this one and tried to re-write the manuscript with this in mind.

Discussion

C2.57: I suggest to re-think the opening paragraphs to start-off the Discussion less as a listing of ways in which the present study is superior to previous comparable efforts.

R2.57: Whilst we appreciate the reviewers thoughts, we feel it important to establish the credibility of our model simulations before we go on to report our results as something that the reader should have confidence in. However, we have reduced this section so that we can more quickly progress to discussing the results.

C2.58: L 319: I do not think here much can be said about confidence on future projections, especially not mentioning the 'error inherent within the GCM predations of future climate change', as only one climate scenario was simulated by only one model.

> R2.58: We have removed this text from the draft. Further, as stated above we now use an ensemble of GCMs.

C2.59: L 323: how have you compared the performance of your setup for a single event to the performance of Fujii et al, 2003?

> R2.59: We now realise that this is unclear in the text as we meant to imply that our multi-year analysis may not have as good a performance metric as the single event analysis carried out by Fujii et al., 2003. We have clarified this in the new draft (lines 405-407).
>
> "*Whilst there are studies of flood extent within our study area that only focus on a single event rather than a multi-year analysis that slightly surpass our own in terms of performance metrics (Fujii et al., 2003)...*"

C2.60: L 326: 'confidence in our future projections of the Cambodian Mekong floodplain's response to changes in the flood hydrograph.' Please rephrase at it's not clear what response to what you are treating here.

> R2.60: We have now changed this to read… 'confidence in our future projections of the Cambodian Mekong floodplain's hydrological response to planned infrastructural development and future climate changes' (lines 410-412).

C2.61: For the discussion of the implications of the changes in hydrological conditions and flood occurrence and extent, I suggest to stay relatively close to the results of this study. It is interesting to mention the cascading effects of more or less floods on different sectors and aspects of the socio-economy and the environment, but these seem to receive disproportionate space when considering that none of these consequences have been modeled in this study. Section 4.2 is largely a review of the literature of the ramified impacts of hydrological and flood changes, with limited connection to the study at hand.

> R2.61: We appreciate the reviewer's insight and have now reduced this section to focus more on a discussion of the issues directly relevant to our modelling results.

C2.62: L 411: 'land use change was considered unchanged in the future': this contrast with the Table above that specifies that different land use is used to simulate years 2020 and 2040.

> R2.62: We do indeed vary land use in the future projections as irrigation and agricultural expansion is now a component of many of the scenarios, which is better explained in the methods section.

Conclusions

C2.63: There is in general a large degree of overlap between concepts included here and in the Discussion and also in the Results. Please review each of these sections and try to reduce repetition to the minimum, repeating only the main concepts that you consider essential for the reasoning in each section.

R2.63: We agree that there was some repetition in the conclusions. We aimed to remove this repetition when revising the manuscript.

C2.64: Fig. 1

R2.64: Thanks for the good remarks; see replies below.

C2.64_1: Why in the small map some areas are dark and soma are light green?

R2.64_1: Cambodia is shown as light green in the map.

C2.64_2: Since country boundaries are important in defining the study area, I suggest making more clear from the map where each country lies.

R2.64_2: We now added the country border between Cambodia and Vietnam to the large map; this indeed makes the map more readable.

C2.64_3: The Tonle Sap lake is mentioned many times in the paper but the name doesn't feature in the map, please specify. Also, other names are mentioned in the text that are not reported in the figure, like the tributaries. I suggest to only mention names of places that are functional to understand the key aspects of the study, and to report those in the map for the many readers that will be unfamiliar with the area.

R2.64_3: We now added Tonle Sap as well as the river names. We aimed to make sure that only the relevant place names, such as provinces and station names, were mapped.

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