We would like to sincerely thank the reviewer for their kind words and for once again providing such detailed and thoughtful comments; these have highly improved our manuscript. Below you will find responses to each of the reviewer's comments outlining how we have addressed them in the revised manuscript.

Response to reviewers' comments

Reviewer 1

The manuscript has been substantially revised and the modelling structure and integration of various models were changed.

Overall, it is hard to closely examine the results and discussion in the paper as I strongly feel that the central questions to interpretation of the results and analysis rely on the proposed methodology and input datasets. Please see my general comments/suggestions below:

Reply: Many thanks for your comments and suggestions on the revised version of the manuscript. We have now clarified the issues raised by the reviewer, as detailed below in our replies.

1. It is difficult to judge or verify the results from this large model integration as errors from one model could propagate to another, different time-steps used in hydrological (daily for V-Mod) and hydrodynamic model (sub-daily for MIKE11 and IWRM-Sub model).

Reply 1: we do understand the reviewer's concern. However, while the validation results of the hydrological model (IWRM-VMod) and flood propagation model (MIKE 11) are presented in the cited documents (Triet et al 2017, Hoang et al 2019), we were able to validate the results also at the last step, i.e. the results from flood extent and duration model (IWRM-Sub). All the three model validations, after each step of the modelling setup, show that simulated results agree well with the observed ones.

VMod: the NSE at the boundary to MIKE 11 is 0.68, i.e. high agreement with observed discharge (see Table 2 in Hoang et al 2019).

MIKE 11: the NSE in Cambodian floodplain stations varies from 0.84 to 0.98, i.e. very high agreement with the observed water levels (see Table 1 in Triet et al 2017)

IWRM-Sub: the NSE in Cambodian floodplain stations (excluding the upper boundary Kratie, that is not within the floodplain) for discharge varies from 0.80 to 0.81 and for water level from 0.85 to 0.87, i.e. very high agreement with the observed discharge and water levels, respectively (see our Table 3). For Kratie the NSE for discharge is 0.79 and water level 0.69, equally very high or high agreement with observations.

We thus believe that the propagation of errors is not an issue in our article. We now communicate this better in the text by adding a comment on the possibility of compounding errors (lines 288-290).

"Using multiple models in succession can have the negative effect of compounding errors, however these results demonstrate that this has not unduly impacted our methodology as our estimations closely match the observations of flood extent."

2. A workflow of modelling structure should be considered to add to this manuscript. This would be a better way to explain the modelling approach of this paper.

Reply 2: the schematic illustration of the modelling setup is given in Fig 2 of the manuscript. We believe that this gives enough information about our modelling approach.

3. There is no explicit mention of the selection of GCMs, climate change datasets, and no detailed description of bias-correction or downscaling for the Mekong. What is the suitability of the selected climate change datasets for applying to this study?

Reply 3: thank you for pointing this out; indeed, we failed to communicate this properly. The bias-correction and downscaling was done in Hoang et al (2019). This is explained in detail in Hoang et al (2016; 2019).

The GCMs (ACCESS-1.0 (ACCESS); CCSM4 (CCSM); CSIRO-Mk3.6.0 (CSIRO); HadGEM2-ES (HadGEM); and MPI-ESM-LR (MPI)) were selected based on their performance regarding historic temperature reproduction (Huang et al., 2014), seasonal precipitation (Hasson et al., 2016) and climate extremes (Sillmann et al., 2013). The GCM data were downscaled using bilinear interpolation to a 0.5° × 0.5° spatial resolution and statistically bias corrected through quantile mapping method, following Piani et al. (2010). The adopted techniques for climate data downscaling and bias correction represent standard approaches for hydrological impact assessment studies such as van Vliet et al. (2013) and Yan et al. (2015).

We now summarise this briefly in the revised manuscript (lines 237-240):

"These GCMs were selected based on their performance in reproducing historic temperature, seasonal precipitation, and climate extremes in the Mekong region. The GCM data were downscaled using bilinear interpolation and statistically bias corrected using a quantile mapping method. For full details see Hoang et al (2016; 2019)"

4. Irrigation scenario is based on global projected expansion. It is advised to briefly describe if this global dataset and its variation were applicable for the Mekong.

Reply 4: again, good request for clarification. For the baseline irrigation, data from the MIRCA - "Global Dataset of Monthly Irrigated and Rain-fed Crop Areas around the Year 2000" (Portmann et al., 2010) – was used. The MIRCA data set provides data on irrigated area and cropping calendar for 26 different crops at roughly 10 x 10 km resolution. This data was resampled to VMod resolution (5 km × 5 km). Since irrigated rice is the most dominant crop in the Mekong basin (accounting for over 80% of the total irrigated land) the focus in the scenarios was on irrigated rice. Although this is a global dataset, the figure pertaining to irrigated rice that we use refers to the Mekong region specifically as that is where the vast majority of irrigated rice is cultivated. Therefore it is indeed valid for use in this case.

5. One of the biggest challenges in modelling flow for the Mekong is a representative of dam operation rules (in both mainstream and tributaries). This paper did not mention much about the type of dams (Run-of-River, small/large reservoir, high/low dams). Each of these has different rules.

Reply 5: and this too, a relevant issue to be clarified.The main role of nearly all of the dams in the basin is hydropower production (except few in Thailand, whose main purpose is to provide water for irrigation). The hydropower dam operation rules were developed by Lauri et al (2012), and those aim to maximise power production. The simulated dam operation impact on discharge was confirmed to be rather accurate by Räsänen et al (2017) who compared the simulated discharge with dam operation rules in place to observed ones in the upper basin. And as reviewer points out, each of the dams has different rules, depending on the active volume of the dam, input discharge, operation of upstream dams, etc. The method used is based on an optimisation algorithm that can take all these aspects into account. See more at the papers cited here.

We have briefly introduced this on the revised manuscript (lines 248-251):

"Dam simulation was based on the optimisation scheme developed by Lauri et al. (2012), which calculates each dam's operating rules separately in a cascade, aiming to maximise productive outflows (i.e., outflows through the turbines), thus maximising hydro-power production."

6. Hydrological station at Neak Loeung is impacted by the tide, particularly during the dry season. Similarly, Station at Chruy Changvar is influenced by the reversed flow of the Tonle Sap Lake and tide. It is good to explain how the modelling approach of this paper overcomes these issues. Moreover, the paper should elaborate on the relationship between water level and discharge at these stations.

Reply 6: The used flood propagation model MIKE 11 has a lower boundary at South China Sea and Gulf of Thailand is thus able to take into account the tidal impact (Table S1 in the Supplement in Triet et al 2017). The same model includes Tonle Sap Lake and thus the reversed flow was accounted for with the model. When looking at the validation results of the MIKE 11 (Table 1 in Triet et al 2017), the water level agrees very well in Prek Kdam (not far from Chruy Changvar) at Tonle Sap River to the observed water levels (NSE is 0.95, i.e. very high agreement with the observed water levels (see Table 1 in Triet et al 2017) and Neak Luong (NSE is 0.98). Therefore, we can conclude that both the tide and Tonle Sap reverse flow are well represented in MIKE 11.

The water results from the IWRM-Sub model, the last in chain, line well with the observed ones too. NSE for Chroy Changvar and Neak Loeung was 0.86 and 0.85, respectively (see Table 3). The NSE for discharge in these two stations was 0.80 and 0.81, respectively, indicating very good agreement also for discharge.

After careful consideration, we came to a conclusion that the current validation analyses are enough to show that our modelling scheme is able to represent the dynamics in the system (in terms of both water level and discharge) and decided not to do any further analysis to elaborate the relationship between water level and discharge. Further, this kind of extra analysis, on top of the already rather packed article, would not add anything new towards meeting the key goals of the article.

Finally, it is just a minor comment. The spelling of the Cambodian provinces should be advised by the Cambodian co-authors. The spelling looks a bit odd, especially in Fig 7. Any changes to these names should be applied throughout the paper.

Reply 7: thanks for the note. We now harmonised the spelling of the province names as given in Fig 1.

Specific comments:

- Line 137 (Fig. 1): Boundary of the provinces cannot be clearly seen.

Reply SC1: Our colleague who produced figure 1 is no longer working on the project and has proven difficult to contact, therefore we have been unable to alter the boundaries. If the editor feels this is necessary, then we can try again to contact him to make the alterations before final publication.

- Line 172 (Fig. 2): There is no legend and scale bar for the maps. It is hard to understand the different colours of the lines, points, arrows, etc. Furthermore, the names of the key stations should be added to the maps.

Reply SC2: This figure is used as a schematic outline to show the methodology and not intended for close scrutiny or to convey information about the study area. For this, we have provided Figure 1. For clarity, we provided some more explanations on boundary condition arrows. We also cite the documents in the caption from where more information is available on tiles A and B.

- Line 243 (Table 2): for better readability, a brief description of the datasets should be provided here.

Reply SC3: all of these datasets are described in Section 2.4. If we added a description of each of the dataset to the table, it would, in our opinion, lower the readability instead of increasing it. This is, because the same dataset is used in multiple scenarios and thus, there would be a lot of repetition. We now refer to the section in the text in table caption.

- Line 276 (Table 3): Not clear the model performance was evaluated with daily or monthly values or others?

Reply SC4: The model performance was evaluated with daily values. This clarification is now added to the caption.

• Lines 291-296: Results for other stations should be included in supplementary material if they could not be presented in the paper.

Reply SC5: The results from Kratie, Kampong Cham, and Chroy Changvar are virtually indistinguishable as they are all 50 -75 km downstream of each other without any major confluences or divergences. The difference in discharge between these points as a proportion of the total is so small as to make no impact on the modelling results. We do explain this in the text.

• Line 327 (Fig. 4): It looks like there is an inconsistency of changes in water level and discharge for Jun-Sep at Neak Loeung. This could be the impact of the tide in discharge estimate? Would you please help explain this?

Reply SC6: we did not find any inconsistency in the results, as tiles g (absolute change) and h (relative change) agree on the direction and magnitude.

• Line 327 (Fig. 4): It is not easy to read this figure as colour bars are tiny and close to each other. It may be impossible to read when printing on monochrome. One way to tackle this issue is to reconsider a different way of visualization, i.e. separate sector development (irrigation and hydropower), climate change and cumulative change (sector and climate).

Reply SC7: thanks for the comment; we agree. We modified the figure so that there is considerably more space for each tile and thus, we think that it is now more accessible.

• Lines 783-787 (Fig. S2 and Fig. S3): The model consistently underestimated water level at Kratie, especially low flow period – 1-1.5 m. However, the model performed well for discharge. Any explanation?

Reply SC8: this is the upper boundary and thus, the water level might be off due to a somewhat inaccurate representation of the river morphology there. However, this does not impact on the floodplain results, as Kratie is outside the floodplain and key is that the discharge is correct, and as Table 3 shows, the NSE for discharge at Kratie indicates very good agreement with the used boundary condition and observed discharge. Also, the water levels in the floodplain agree very well with observations (see our reply 3 above).

Reviewer 2

I commend the authors on the review work. The suggestions of both referees have been considered with utmost care, and all points are addressed. This includes expanding the modeling framework to include the 1D flood modeling step, therefore capturing also the effects at the seawards boundary, much expanding the scenarios contemplated, and including many more simulations. It is quite impressive that the authors have been able to carry out and document so much work in a short time. The explanation of the methods is far clearer, and Fig. 2 is excellent. Calibration and validation of model results is high quality, and figures are generally far superior now. The new version is a much more solid and superior study in many respects, and conclusions/implications can be drawn with much more confidence. What authors could not implement in the revision, e.g., a more accurate topography in parts of the analysis, is satisfactorily motivated. I recommend publishing the article, pending some minor suggestions that the authors should consider, focusing mostly on the general presentation of the study and on the methods.

Reply: Many thanks for your positive comments on the revised version of the manuscript.

Comment 1: (L. 13) scenarios are mentioned here as though it was already clear that any scenarios were included, whereas this is not the case.

Reply 1: We have now made it clear that we run scenarios by adding text to the introduction (line 15).

"We then ran scenarios to approximate possible conditions expected by around 2050"

Comment 2: (L. 14-18) In those sentence there is repetition that could be substituted by some indications of in which direction climate change and hydrological development, respectively, alter discharge. That will be an obvious question still in the mind of the reader after having read the abstract.

Reply 2: We have now taken out the unnecessary repetition and added text describing the expected impact of climate change (lines 19-21).

"Projected climate change impacts are expected to decrease dry season flows and increase wet season flows, which is opposition to the expected alterations under development scenarios that consider both hydropower and irrigation."

Comment 3: (L. 25) the first part of the closing sentence of the abstract seems not very informative. So far the reader has not received any indication about the heterogeneity or complexity of the region, and does not have the chance to learn anything meaningful here. Similarly for the ecological fragility: it's mentioned here for the first time and not much is said about it.

Reply 3: We have now changed the abstract text to reflect these comments, and amending the last sentence (lines 28-31).

"Our findings demonstrate the substantial changes that planned infrastructural development will have on the area, potentially impacting important ecosystems and people's livelihoods, calling for actions to mitigate these changes as well as planning potential adaptation strategies."

Comment 4: (L 39) flooding creates damage even if short-lived. Also, please check punctuation (also on line 49).

Reply 4: We have now removed the work 'prolonged' so that this applies to all floods. We have also changed the semi-colon to a comma in both instances.

Comment 5: (L 59) since most of those papers will be explained individually in the following, it's probably not necessary to cite them all together in that line. Also later in the paper, a bunch of studies are cited repeatedly, mostly needlessly.

Reply 5: We have removed references to Hoang 2016, Hoang 2019, and Lauri 2012 as each of these is described in the following passages. We have also removed a number of unnecessary references, mainly to Hoang et al 2016 and 2019 from the discussion.

Comment 6: (L 60) Hoang et al 2016 present results for several stations of the Mekong. To which does this result refer?

Reply 6: We have now stated that this result refers to stations Stung Treng and Chiang Saen.

Comment 7: (L 73) it doesn't seem obvious to the reader that "These hydrological alterations are likely to intensify when considered cumulatively". In the previous sentence you report opposing outcomes on dry season flows, so that one expects alternations to compensate each other.

Reply 7: We agree with the reviewer that these two sentences contradict one another and so have removed the latter sentence.

Comment 8: Please check that whenever a results from previous studies is reported that evokes climate change, the scenario to which it is associated is also reported here, so the reader can evaluate if any discrepancies are attributable to different study set ups or to different scenarios.

Reply 8: Good suggestion, thanks. We have now added that both Hoang et al (2019) and Try et al (2020) use RCP projection 8.5 (lines 74 and 81).

Comment 9: (L 186) the reader is referred to Triet et al. 2020 for the forcings of MIKE11, among which the sea level rise data used in this study. That study seems to only include a 43 cm sea level rise scenario. Is that what is used in this study, and is that appropriate for both climate scenarios included here?

Reply 9: thanks for noting that our explanation for sea level rise was inadequate. Triet et al (2020) refers with sea level rise scenarios to a combination of climate change related sea level rise and the deltaic land subsidence. They used an average of the range estimated by Manh et al (2015), i.e. 22-63 cm. The climate change related sea level rise is taken from IPCC (2014), and is estimated for our study period to be 17-38 cm – covering all the RCP scenarios from RCP2.6 to RCP8.5. There is very little difference between the RCP scenarios (RCP4.5: 19-33 cm; RCP8.5: 22-38 cm), and thus it is justified to use the same estimate for sea level rise + deltaic land subsidence for both climate scenarios.

We now state this in the revised manuscript as follows (line 240-243):

"The seal level boundary condition was adjusted by 43 cm for future scenarios to account for the combined effects of sea level rise and deltaic subsidence, taken as the average of the range estimated by Manh et al (2015) i.e., 22-63 cm. This value was used for both RCP4.5 and RCP8.5 as the climate change component of sea level rise for our study period taken from IPCC (2014) is relatively consistent across RCP scenarios (RCP4.5: 19-33 cm; RCP8.5: 22-38 cm)."

Comment 10: (L 240) It is fine that the reader is referred to the previous study for further details on scenarios, but it would seem important that some more information is included also here on how the effect of the reservoir is included in the simulations. What assumptions are made about the way those 126 dams are operated? It seems plausible that based on that the peak flow lamination and the environmental flows may change massively.

Reply 10: We have now added text that describes the assumptions used when optimising the dam rules (lines 248-251).

"Dam simulation was based on the optimisation scheme developed by Lauri et al. (2012), which calculates each dam's operating rules separately in a cascade, aiming to maximise productive outflows (i.e., outflows through the turbines), thus maximising hydro-power production."

Comment 11: (Table 2) I find the name codes of the scenarios needlessly confusing. E.g., why sometime 'Irrigation_low' is included in the name, and other times 'LI'? why scenarios including climate change sometimes have the notation CC and sometimes not. If it's too much trouble, the authors may leave names as they are.

Reply 11: Scenarios 2-6 consider one development activity or climate change projection in isolation, and so have expanded naming. Scenarios 7-12 combine more than one element, and so are shortened to save space. We did it this way to include as much information in the earlier (2-6) names as possible.

Comment 12: (Fig. 6) Another puzzling choice is to have the two baseline maps on a different scale than the rest of the maps here. This does not have to be changed, but I wanted to point it out in case the authors agree that this is bizarre and does not facilitate visual comparison.

Reply 12: We thought to include slightly larger baseline maps as these convey the data that all scenarios are then judged against. But we have now amended the figure so that all the maps are the same scale.

References

Hasson, Su, Pascale, S., Lucarini, V., Böhner, J., 2016. Seasonal cycle of precipitation over major river basins in South and Southeast Asia: a review of the CMIP5 climate models data for present climate and future climate projections. Atmos. Res. 180, 42–63. https://doi.org/10.1016/j.atmosres.2016.05.008.

Hoang, L.P., Lauri, H., Kummu, M., Koponen, J., van Vliet, M.T.H., Supit, I., Leemans, R., Kabat, P. & Ludwig, F., 2016. Mekong River flow and hydrological extremes under climate change. Hydrol. Earth Syst. Sci. 20, 3027-3041. https://doi.org/10.5194/hess-20-3027-2016.

Hoang, L.P., van Vliet, M.T.H., Kummu, M., Lauri, H., Koponen, J., Supit, I., Leemans, R., Kabat, P. & Ludwig, F., 2019. The Mekong's future flows under multiple drivers: How climate change, hydropower developments and irrigation expansions drive hydrological changes. Sci. Total Environ. 649, 601-609.

Huang, Y., Wang, F., Li, Y., Cai, T., 2014. Multi-model ensemble simulation and projection in the climate change in the Mekong River Basin. Part I: temperature. Environ. Monit. Assess. 186, 7513–7523.

Lauri, H., Moel, H.d., Ward, P., Räsänen, T., Keskinen, M. & Kummu, M., 2012. Future

changes in Mekong River hydrology: impact of climate change and reservoir operation on discharge. Hydrol. Earth Syst. Sci. 16, 4603-4619. 10.5194/hess-16-4603-2012.
Piani, C., Weedon, G.P., Best, M., Gomes, S.M., Viterbo, P., Hagemann, S., Haerter, J.O., 2010. Statistical bias correction of global simulated daily precipitation and temperature for the application of hydrological models. J. Hydrol. 395, 199–215.
Portmann, F. T., Siebert, S., & Döll, P., 2010. MIRCA2000-Global monthly irrigated and rainfed crop areas around the year 2000: A new high-resolution data set for agricultural and hydrological modeling. Global Biogeochemical Cycles, 24(1). https://doi.org/10.1029/2008GB003435
Räsänen, T. A., Someth, P., Lauri, H., Koponen, J., Sarkkula, J., & Kummu, M. (2017). Observed river discharge changes due to hydropower operations in the Upper Mekong Basin. Journal of Hydrology, 545, 28–41. https://doi.org/10.1016/j.jhydrol.2016.12.023
Sillmann, J., Kharin, V.V., Zhang, X., Zwiers, F.W., Bronaugh, D., 2013. Climate extremes indices in the CMIP5 multimodel ensemble: part 1. Model evaluation in the present climate. J. Geophys. Res. Atmos. 118, 1716–1733.
Triet, N. V. K., Dung, N. V., Fujii, H., Kummu, M., Merz, B., & Apel, H., 2017. Has dyke development in the Vietnamese Mekong Delta shifted flood hazard downstream? Hydrology and Earth System Sciences, 21(8), 3991–4010. https://doi.org/10.5194/hess-21-3991-2017
Yan, D., Werners, S.E., Ludwig, F. and Huang, H.Q., 2015. Hydrological response to climate change: The Pearl River, China under diYan, D., Werners, S.E., Ludwig, F. and Huang, H.Q., 2015. Hydrological response to climate change: The Pearl River, China under different RCP scenarios. Journal of Hydrology: Regional Studies, 4, pp.228-245.