

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 reviewer's comments

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 modelling framework to include the 1D flood modelling 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 sentences 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 237-241):

“The sea 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 244-246).

“Dam simulation was based on the optimisation scheme developed by Lauri et al. (2012), which aims to maximize 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.