Reviewer 1

We appreciate the comments and insights provided by Reviewer#1, and below in **bold** include our response to the comments.

General Overview:

The study by Naha et al. aims to assess the role of future land use land cover (LULC) change in altering the nature of hydrological extremes in the Mahanadi river basin in India, under different future scenarios, described by a combination of socio-economic pathways (SSPs) and representative concentration pathways (RCPs). The manuscript deals with an important topic in a region sensitive to hydrological extremes. It is, in general, well-written and easy-to-read.

We would like to thank the reviewer for this positive comment.

However, several aspects of the study, including novelty, introduction, and methodology (specifically the sensitivity analysis and calibration) are either not well designed or incompletely described. I cannot recommend the publication of this study until these issues are satisfactorily addressed.

We have now completely redesigned this study based on the reviewer's suggestions and will be rewriting a significant portion of the manuscript, highlighting its novelty, and providing more clarity to the sections mentioned above.

Major Comments:

1. Introduction:

One of the main issues is the lack of novelty in the presented research, outside its contribution as a case study. The authors need to clearly bring out the novelty of their research compared to the existing research (cited in the study) which have already shown that model parameters have an impact on future land use change studies.

• Line 50 - 55: "However, the exact role of LULC changes in modifying river discharge is still elusive (Rogger et al., 2016) and therefore, remains a challenge to isolate the sole impacts of land use changes on hydrology of a river basin (Tsarouchi and Buytaert, 2018)". These claims are very vague. What are the specific challenges and how does the present study aim to overcome them?

• Line 105 - 110: What is missing in these studies which the authors have solved in the presented study? This is not very clear.

• Line 115 - 120: The research questions are very vague. None of the research questions pertain to the central question: impact of LULC change on hydrological extremes. Instead, it focuses on the modelling part.

We thank the reviewer for suggesting more clarity in highlighting the novelty. As we have redesigned the study, we will modify the Introduction section of our manuscript highlighting the novelty and reframing the research questions with lot more clarity.

We think the reviewer is trying to point out the citations, (Chaney et al., 2015; Singh et al., 2014; Zhang et al., 2019) by saying 'existing research (cited in the study)'. These are some studies that highlighted the need of considering model parameter uncertainties while modelling the changes in hydrology (not particularly in context of land cover change studies). Our intention of adding these citations was to highlight that although much attention has been paid to the model predictive uncertainties, less for uncertainties in the context of impact assessment of land use change. For instance, several studies (Bennett et al., 2018; Feng and Beighley, 2020; Her et al., 2019) exist that takes into account model parameter uncertainties while dealing with climate change, however rarely, it has been considered while assessing land cover changes.

We agree that the way in which the citations are used in these sentences in the original manuscript are unclear. We will reframe the sentences along with some added citations to prove our statement as we mentioned in the paragraph above.

2. **Design of Study and Methodology:**

I have serious issues with the design of the study. As it stands, it reads like two different studies which are unrelated to each other: 1) quantify impact of parameter uncertainty and 2) quantify impact of lulc cover change. However, there is no attempt to connect these two objectives. It is well known that parameter uncertainty will have an impact on the output of hydrologic models. However, how does this specifically relate to LULC change studies? This is not clear.

We thank the reviewer for pointing this out. As we explained above, we will re-frame the objectives of the paper and will add necessary explanations and figures (if required) to better associate the land cover change impacts with the model parameter uncertainties.

• It is not clear as to why only soil parameters are selected for sensitivity analysis. I would imagine that the selection of the hydrologic model should be based on how sensitive the model parameters are to vegetation parameters which LULC. In fact, this should be the first step to understand the behaviour of the model to changes in LULC. This information is missing.

We thank the reviewer for pointing this out. This was an important feedback provided by the reviewer. We realize this was the shortcoming in our study and now we have taken the vegetation parameters into account. We are re-running the Sensitivity Analysis (SA) and model calibration experiments. We have now conducted some global sensitivity analysis experiments including the vegetation parameters and a few more soil-related parameters (Figure 1). Our preliminary results suggests that canopy height is not sensitive, and as roughness length and displacement height are both computed from canopy height directly (following standard meteorological approaches), we have decided not to include these parameters in our final experiments. We derived daily Leaf Area Index (LAI) product directly from MODIS AQUA/TERRA (2000-2015), hence not included as a parameter. We also redesigned the root zone allocation in our study. In our new approach shown in Figure 2, the root zone fractions for each root zone depths and fractions varies more realistically with respect to the soil depths while calibrating. In total, 16 parameters including soil, vegetation and routing parameters are subjected to sensitivity analysis in the revised version and our

preliminary results suggest that re-designing our SA and calibration experiments have improved our model performance.

• Model calibration and Pareto ranking are poorly described. What are the specific steps involved in assigning the ranks?

We are now re-running the calibration experiments and the Pareto Ranking approach is no longer applied.

• I am not sure if I understood this correctly but only a range of parameter values are provided for each LULC type. It would be better to provide the parameter values that are used for each LULC type to see if they are physically consistent.

We thank reviewer for this suggestion, and we will incorporate this in our revised manuscript. We will add a table in the supplementary section showing the vegetation parameter values or ranges (if subjected to sensitivity analysis) along with their sources, used in this study for each dominant LULC types in the basin.

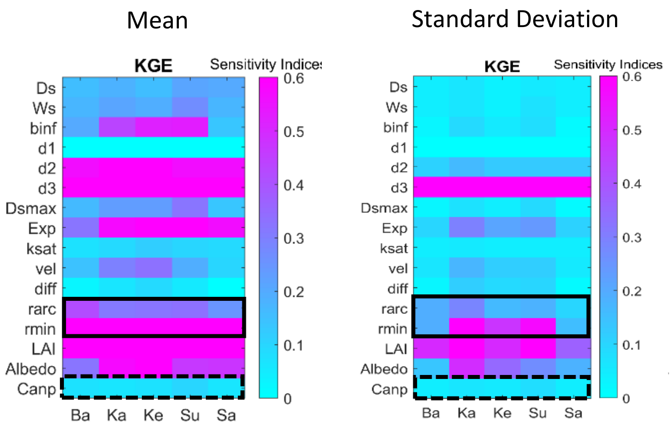

Figure 1: Sensitivity indices of EET method for VIC model parameters for all the sub-catchments. Colour bar on the right side indicates sensitivity of the model parameters to the streamflow. '0' indicates least sensitive and '0.6' indicates most sensitive. Mean indicates direct effect of these parameters on model output. Standard Deviation indicates parameters interaction effects on model output. Vegetation parameters marked in 'solid black' rectangle considered for SA and marked in 'dash black' are not considered.

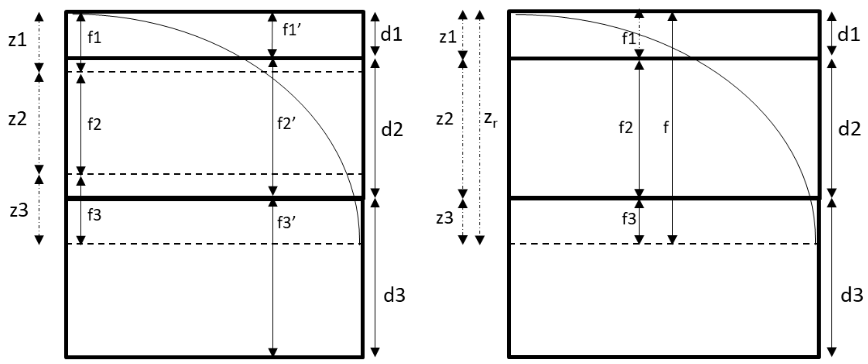

Figure 2: (left) Representation of rooting distributions (typically kept fixed) in VIC-3L model. z1, z2 and z3 are the user-defined depths of three root zones, respectively. d1, d2, d3 are the depths of three soil layers. f1, f2 and f3 are user-defined fractions of root in each zone, respectively. f1', f2' and f3' are fractions of root in each soil layer computed by VIC. We used this approach in the original manuscript. (right) Our new approach of representation of rooting distributions in VIC-3L model proposed in the revised version. z_r is the total root depth which is fixed. However, z1, z2, z3 varies with d1, d2 and d3 respectively. f1, f2, f3 are the root fractions in respective root zones computed using (Zeng, 2002)**.**

3. Results, Discussion and Conclusions:

• Figure 6b: The calibrated parameter ranges are too wide (equifinality). I am not sure why calibration was not able to narrow the range of parameters here. The authors need to discuss why the ranges are not more constrained even after calibration. The discussion in lines 615 - 635.

• Figure 9: The issue of using only related parameters for calibration is reflected in the water balance components. Evaporation, which I assume is dependent on vegetation parameters, shows very little change (less than 8 %), while soil moisture changes exhibit large variations. This makes it very difficult to discern the impact of LULC changes alone. I recommend that the authors also present results with only the best parameter. Of course, I assume here that actual evaporation is explicitly modelled in VIC and is not an input.

We have made substantial changes in the model sensitivity and calibration methods which we are reanalysing now, and our preliminary results indicate improvement in the model performance (not shown). Hence, we believe these changes will have impact on the issues raised by the reviewer in these particular figures. We will add necessary explanation and discussion in this regard in the revised manuscript.

• Line 655 - 670 - Many of the points under major findings are a repetition of the results and do not represent substantial or novel conclusions.

We thank the reviewer for pointing that out and will take that into account in the revised version of the manuscript.