Reviewer 2

We appreciate the comments and insights provided by Reviewer#2, and below in **bold** include our response to the comments.

General Overview:

The study investigates how the hydrological regime of the Mahanadi river basin would respond to the current and future land cover scenarios under a large-scale hydrological modelling framework. The recently released dataset LUH2, which has not yet extensively used in basin-scale hydrology, is firstly used in this study to provide current and future projected land cover scenarios. Although many studies have addressed the effects of future land cover changes on hydrological processes, I believe the novelty of the study lies with the consideration of parameter uncertainties of the physically based model VIC, which are widely used by the hydrology community when evaluating LULC and climate changes. Valuable insights are provided into the sensitivity of the model parameters to the model outputs and the interactions among the model parameters in producing changes of the hydrological regime within different LULC scenarios.

We would like to thank the reviewer for this positive comment. We are happy to see the comment about the LUH2 dataset not yet being extensively used, and we hope to provide new insights on the hydrological processes of the basin using this dataset.

To some point, I agree with RC1 that the parameter sensitivity tests are not properly designed and there lacks through analysis to link the sensitivity results with the LULC impacts. However, the authors agreed to re-conduct the sensitivity analysis and the calibration experiments with the involvement of more vegetation and soil-related parameters. I am looking forward to the new outcomes and modifications.

Thanks for pointing this out. We realize this was the shortcoming in our study as highlighted by Reviewer 1. We are already making substantial changes in the model sensitivity, calibration methods, and the overall structure of the paper (as discussed in our reply to Reviewer 1) which will be presented in the revised version of the manuscript. Therefore, in this short response, it is important for the reviewer to recognize that we are focusing on the key comments raised by reviewer which are not directly related to the formatting or presentation of the paper (i.e., typos and unclear sentences) as the revised version will likely look significantly different.

Specific comments:

1. Figure 2, please indicate in the figure title the time period of the LULC map and LAI data, and show the land use types in the legends rather than using abbreviations.

We thank the reviewer for pointing this out. We will add the time period for both LULC map and LAI data, and will consider revising the legends providing we reach a good balance between required text and information from the figure. Notice that the land cover types shown in the legend are standard and widely used across the scientific community following IGBP convention.

2. Line 198, explain what the exact meaning and the geographic scope of the “3 root zones”.

As part of your initial comment (and also from Reviewer 1), we are now redesigning the root zone allocation in our revised methodology (see our Response to Reviewer1 'Reply on RC1') and will add necessary explanations about this in the revised manuscript.

3. Line 251, should "Klein-Gupta Efficiency" be "Kling-Gupta Efficiency (KGE)"? It is better if the equations of NSE, lnNSE and KGE could be given.

Thanks for the correction/suggestion. As part of your initial comment (and also from Reviewer 1), we are now restructuring the manuscript, including its analysis. In order to simplify our analysis and to help with our discussion, we are now only using the Kling-Gupta Efficiency (KGE) metric. Note the KGE metric balances the contribution to the error coming from all three main components, namely correlation (e.g., timing/dynamics), variability (e.g., seasonality), and systematic bias, and is now a widely used metric in hydrometeorological studies. Some of the shortcomings of using NSE and benefits of using KGE are described in the Gupta et al. (2009). The revised version of the manuscript will include the KGE equation.

4. Line 330-331, "In all the six cases of model run, meteorological forcing is held constant:: :", are the forcing data from the baseline scenario (2005) are used? Please give detail explanations.

We realized this sentence can be confusing and affect the interpretation of our results. The meteorological data used is for the period 1990-2010. This period is used at all experiments presented in the paper. While the meteorological data used remain unchanged, we do however modify the land cover data to identify the hydrological impacts under LULC changes. These are represented by four distinct LULC maps of year 2005, 2015, 2050 and 2100 and two hypothetical scenarios with full land cover for Cropland and Forest, respectively. We name those LULC scenarios as baseline (2005), Present (2015), Near Future (2050), Far Future (2100) and the two hypothetical scenarios for cropland and forest. We will make sure this information is clearly presented in the revised version of the manuscript.

5. Line 421, the time period 1990-2010 is used for analysing the effects of different LULC scenarios on the streamflow in the study. However, this covers the calibration and validation periods used for generating the VIC model parameters. In normal case, data used for calibrating models should not be used again for further analysis.

We thank the reviewer for pointing this out. The separation between calibration and validation period we took in the initial part of the paper was to identify any possible significant deviations from the calibrated model in simulating key hydrological processes outside the calibration period. Our results indicated the expected reduction of performance but nothing to cause alarm and invalidate the experiment. For the revised version of the manuscript, the LULC scenarios will be analysed using only the common validation period (2000-2010) as pointed out by the reviewer.

6. Line 503-520: this paragraph describes how the parameters influences the change of the streamflow as well as other water balance components. However, these descriptions seem to be deductive from the physical definitions of the parameters, rather than being concluded

from Fig 8. The results showed in Fig 8 are not well analysed and the sensitivity of parameters should be given in a more quantitative way.

We thank the reviewer for raising this issue. We agree with the reviewer and as we are restructuring the revised version of the paper, we will take this comment into account to improve the discussion of the results.