

## Reviewer 1

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

### **General Overview:**

This is my second review of the study by Naha et al in which the authors attempt to quantify the changes in the hydrological cycle due to changes in the land cover under future climate change scenarios. I am pleased to see that the authors have taken onboard all my suggestions/criticisms of the previous version. Overall, the manuscript has significantly improved, and the results and analysis are appropriate for a regional study of land cover impacts on hydrology. I especially appreciate the inclusion of a comprehensive sensitivity analysis of the model parameters. I only have a few minor comments which are easily addressable.

**We would like to thank the reviewer for this positive feedback. We are happy to know that our proposed changes are being well received by the reviewer. We thank the reviewer for their comments/suggestions on the previous version of our manuscript.**

### **Specific comments:**

1. It is interesting to see that soil related parameters are the most important parameters for runoff. A naive question: Are there any links between soil parameters and land cover change? I am not very well-versed in VIC but my question is whether changing landcover also impacts these soil-related parameters?

**In VIC, changing landcover would not impact the soil parameters directly, as all the soil related parameter values are assigned solely based on soil textures.**

2. In Line 110, the authors claim that daily time steps are used in the study. How reliable are daily values derived from climate change scenarios? It would be great if the authors can elaborate on the robustness and relevance of the daily simulations, especially in a climate change study.

**We thank the reviewer for raising this point which we believe may have been caused by lack of clarity in the manuscript. In our study, we only test the impact of land-cover/land-use changes using LUH2 scenarios. In order to isolate these scenarios, we fixed the meteorological forcing using 'current' climate from 1990-2010 available daily from the Indian Meteorological Department. Notice that VIC is commonly employed at daily scales especially when running using the water balance only mode (Gou et al., 2020; Hengade et al., 2018; Hurkmans et al., 2009).**

3. Line 50: Here the authors suggest that the croplands have increased by 82%. I think it would be helpful to have a baseline (82% increase compared to which year?)

**Thanks! We will rephrase this sentence accordingly in our revised manuscript and provide the supporting reference. The proposed revised text is shown below**

**“A recent analysis on global land cover changes using recent satellite remote sensing data (2000–2017) reveals 86% changes in land cover pattern in India, out of which 82% is detected as croplands and a minor 4% as forests (Chen et al., 2019)”**

4. Line 225-230: The authors find that LAI values are in good agreement with a nearby Indian basin. It would be useful to mention the name of the basin here.

**Thanks! We will add the basin name. “Ganga river basin” (Patidar and Behera, 2019)**

5. Line 455: “...the model is able to estimate all the water budget components and maintain proper closure...”. This statement is very misleading. Unless, I have missed the validation of the other water balance components, the authors do not yet have evidence to support this statement. Of course, in this study, this is not very important as relative changes are more important, but several other studies have shown how calibrating with only streamflow adversely affects the accuracy of other water balance components. Moreover, hydrological and land surface models close the water and energy balance by construct, so the claim of proper closure is untrue.

**We agree with the reviewer and will remove this statement from our revised manuscript.**

6. There are several grammatical and language-related idiosyncrasies which need to be corrected. I request the authors to rectify them (I only give a few examples here). Evapotranspiration need not be capitalized similar to the other water balance components. Also, Potential Evapotranspiration can be just potential evapotranspiration.

Line 110-115: “...impact studies is limited just with ...” should be “...impact studies are limited just to...”

Line 220: “...are not accounted for SA” should be ...are not accounted for in SA”

Line 480: “...enhances the accuracy for predicting hydrological responses...” could be “...enhances the accuracy of hydrological predictions...”

Line 485: “...are sensitive to this basin...” is not very accurate. Do you mean basin’s runoff?

**Thanks, we will correct all grammatical and language-related idiosyncrasies in our revised manuscript.**