Response to Reviewer #3 comments

This manuscript demonstrate the method to reconstruct meteorological and soil moisture droughts in India by three LSMs, i.e. VIC, Noah, and CLM. The overall scientific idea is clearly expressed in detail. The manuscript could be considered to be published after the following minor concerns are addressed. And the language should be carefully polished and make the whole manuscript concise and precise.

We thank the reviewer for his/her insightful comments. We have addressed the reviewer's comments and we will further check the manuscript for language and conciseness.

Specific comments:
1. Page 1 Line 8. In Abstract, "As a large population of India is dependent on agriculture, soil moisture droughts adversely affect agriculture and groundwater resources" This sentence is illogical and should be rephrased.

Thank you. We will reformulate this sentence in the revised manuscript.

2. Page 5 Line 5. The definition and the formula of SPI and SSI should be clearly expressed in the manuscript instead of just giving the cited literature and leaving the readers to the literature. In another word, the manuscript should be self-contained.

We will explicitly mention the definition (and formula) for SPI and SSI in the revised manuscript – as an Appendix or in Supplement.

3. Page 5 Line 18. It would be better if the Indo-Gangetic Plain Region can be shown in supplemental figure.

Thank you. We will show the Indo-Gangetic Plain region in the supplements.

4. Page 6 Line 20. How do you compare the TWS (which is the total terrestrial water storage including both surface water, soil moisture and groundwater) and the total column soil moisture (which is just the soil moisture stored in aquifers)?

We used the anomaly of the entire soil-water column for every model and compared it against the GRACE derived TWS anomaly. In doing so, we agree with the reviewer that we might have missed the groundwater component; and this might be relevant for a certain part of India. On the other hand, considering that the temporal dynamics of monthly groundwater is rather very slow, the most of the temporal variability in the TWS anomaly can be expressed by the soil-water part. Therefore, comparing the anomaly of the modeled soil water column with GRACE derived TWS anomaly for assessing the skill in terms of capturing the temporal variability, as considered in this study, would be reasonable. Here, our aim is not to predict TWS using soil moisture, instead we

just considered TWS as surrogate of observations to evaluate the models' skill for soil moisture simulation.

5. Page 7 Line 5. Why the ranks of 2014 and 2015 droughts identified in this manuscript are different from the cited literature Mishra et al., 2016b. Which study is confirmed by the in-situ records and which is biased identification? This affects the quality of the drought reconstruction in this manuscript and should be addressed carefully.

Here we think there is some misunderstanding. In the cited paper Mishra et al, 2016b; we looked for the muti-year drought ranking, rather than a single year drought (as done in this study). Moreover, here the ranking is based on soil moisture drought rather than meteorological drought as in Mishra et al. (2016). Soil moisture droughts are affected by the soil moisture persistence, therefore, ranking (from soil moisture and precipitation) may be different.

6. Page 8 Line 2-5. What do you mean by "soil depths were calibrated in all the three LSMs"? Are the soil depths in LSMs the same or not? If they are the same, "The 1-month lag between peak precipitation and peak root-zone soil moisture from the CLM can be due to a relatively deeper soil column." Should be deleted. If not, "since: : :: : :" should be deleted. Above all, you should clearly express the reason and not confuse with each other: soil depths, the number of soil layers or processes related to soil hydrology.

We followed the (calibration) approach of the VIC model in which the soil depths are estimated via a calibration procedure such that the modeled stream-flow matches the observed values. We followed this approach given the wide success of the VIC model application in a wide variety of river basins across different climatic conditions. Moreover using a similar (calibration) approach, we aim to harmonize the different model applications over India. So, the soil depths in LSMs are treated as calibration parameters, which vary across the models and the river basins. We will make this clear in the revised manuscript.

Technical corrections:

1. Page 1 Line 25. The citation Mishra et al., 2016b comes first before 2016a. The literature should be cited in order of their appearance in manuscript.

Thank you. We will edit this during the revision.

2. Page 3 Line 4. Digital elevation map (DEM) should be rewritten as digital elevation model (DEM).

Thank you. We will edit this during the revision.