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Discussions

Interactive comment on "Reconstruction of droughts in India using multiple land surface models (1951–2015)" *by* Vimal Mishra et al.

Anonymous Referee #1

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Review of the paper "Reconstruction of droughts in India using multiple land surface models (1951-2015)" by Mishra et al. (hess-2017-302) submitted to Hydrol. Earth Syst. Sci.

The authors manually calibrate three land surface models (CLM, Noah, VIC) by using observed monthly streamflow at streamflow gauges for one period (∼ 5 years) and validate for the independent other period (∼ 5 years). The three models were run from 1951 to 2015 to produce root zone (∼60 cm) soil moisture products. The authors use these soil moisture products to analyze Indian agricultural drought events including severity, frequency, and drought extent. The results found that there is larger uncertainty in crop growing season than the monsoon season. The large uncertainty is mainly due to the difference in model parameterizations – different soil moisture

persistence. The results suggest using multi-model ensemble for Indian drought monitoring. For model setup and calibration, model evaluation, and analysis of differences in model parameterizations, the paper shows some major deficiencies in its general appearance. Therefore, I recommend a major revision of the manuscript.

Major comments:

1. Model setup

(a) Spin-up period: Is a spin-up period run? If no, please explain reason. If so, how long is run for each model for this spin-up period? Was soil moisture equilibrium state including deep soil layer checked? (b) I do not think that you can use daily meteorological forcing data to run Noah and CLM? In general, hourly surface forcing data are used to drive such land surface models. How to divide daily meteorological forcing data into hourly time scale? (c) It is not clear how to calibrate Noah model. Why are depth of soil layers, Zilintikevich coefficient, surface runoff parameter and bare soil evaporation component selected? Is any sensitivity test performed or does the selection just depend on your own experience? Which are surface runoff parameter and soil evaporation component? What possible values do you use? How to manually tweak these values for each basin individually or together? I am puzzling how to calibrate soil layer depth. Based on my experience, the Noah four soil layers are 0-10 cm, 10-40cm, 40-100 cm, and 100-200 cm. The mid-layer is 5 cm, 25 cm, 70 cm, and 150 cm. If you calibrate soil depth, for each grid point at a given basin, you adjust these soil layer depths. If so, can you make a plot to compare these calibrated soil depths with default soil layer depths. (d) It is very confused how to calibrate CLM using soil depth layers. More explanations are needed. (e) What are soil parameters in Section 2.2.2 and 2.2.3? Are they soil textures (types)? Noah and CLM use the soil textures derived from FAO, and VIC uses soil texture derived Harmonized World Soil Moisture Datbase (HWSD). I am wondering how big differences exist between two datasets? It is very well known that different texture has different soil related parameters such as field capacity, wilting point, etc., which leads to different temporal variation. (f) Different vegetation type classification datasets are used for different models, which can result in additional uncertainty for soil moisture product as different vegetation type has different root zone (leads to different transpiration even though surface meteorological forcing is the same). (g) There is only one test in this study – calibrated run. I would like to see the control run/default run (the default parameters are used) and the comparison with the calibrated run. This will demonstrate what benefits you gain from the calibration process.

2. Model evaluation

(a) Calibrated model is only evaluated against observed streamflow. Unfortunately, I am very disappointed that the soil moisture used in this study is not evaluated against either in-situ observations or remotely sensed soil moisture. There are a few stations in India to measure soil moisture from different datasets such as Global Soil Moisture Data Bank (Robock et al. 2000), In-situ observations of soil moisture from India Meteorological Department (Unnikrishnan et al. 2016), and international soil moisture network (https://ismn.geo.tuwien.ac.at/). In addition, quite a few of remotely sensed soil moisture products such as SMAP, SMOS, SMOPS, ASCAT, AMSR2 and more are not used to evaluate LSMs soil moisture simulation products. However, the major variable used in this study is 60 cm soil moisture. Robock, A., et al., 2000: The Global Soil Moisture Data Bank, BAMS, 81, 1281-1299. Unnikrishnan, C. K., et al., 2016: Validation of two gridded soil moisture products over India with in-situ observations, J. Earth System Science, 125, 935-944. (b) The authors assumed 60 cm soil layer as root zone. However, for each individual model, it defines its root zone varying from vegetation type to vegetation type. For example in Noah, grass root zone is 1m and forest is 2m. I suggest the authors use 60 cm soil moisture in whole text to avoid confusing the readers.

3. Model result analysis

(a) The uncertainty analysis is very limited due to three models as the samples are

too few for a representative of model uncertainties. In general, the spread can roughly show an uncertainty range when three-model ensemble is used. The authors need to indicate this weakness in a discussion section. (b) The authors indicated that the uncertainty in soil moisture is mainly due to model parameterizations – resulting in different persistence of soil moisture. They assumed that there is a large field capacity for CLM but there is no further investigation. In practical, different soil texture datasets, different vegetation type classification datasets, different model structure (specific soil layer in CLM and Noah vs hydrological soil layer concept), and other ET parameterizations may affect this uncertainty together. I recommend make several sensitivity tests to clarify these issues. At least, plot field capacity, wilting point, soil type, vegetation type, root zone depth for all models and then compare their differences. (c) In Figure 3c, the seasonal cycles in Noah and VIC are comparable although the magnitude is quite different. However, that in CLM is completely different with Noah and VIC. This further suggests that soil moisture evaluation against in situ observations and remotely sensed product is needed to identify which is closer to the observations. (d) In line 33, page 7, the authors cited Wang et al. (2009) to explain higher persistence in soil moisture due to larger water holding capacity and thicker soil column. However, the authors used 60 cm soil layer for all models and also need plot water holding capacity for top 60 cm to verify this point. (e) The authors find an interesting point, that is, there are larger uncertainties in Rabi season than monsoon season. Unfortunately, the authors do not make further investigation to look for the reason. They use a general sentence "which can be associated with the role of air temperature and precipitation on soil moisture" to explain. When Figure 4 and Figure S3 are checked, during the monsoon season, three models have larger similarity than Rabi season mainly due to VIC model. A possible reason is that VIC water mode rather than energy mode is used in this study. During the monsoon season, water is unlimited and limited energy is used due to less net radiation (rainy and cloud sky). Energy and water-mode type model does not have big difference. However, during Rabi season, water is limited but energy may be unlimited, so that energy-type model (Noah, CLM) shows larger difference than water-mode type model (VIC). A quick check is to use VIC energy mode to re-run this test to compare with VIC water mode run.

Minor Comments:

1. Check Table S1: Surface downward shortwave and longwave radiation , for CLM v3.0, soil texture based on IGBP or FAO or vegetation type data based on IGBP. 2. Check Table S2: East coast, calibration and validation period is overlapped. 3. Check Table S2: Mahanadi, calibration and validation period is overlapped. 4. Check Table S2: Subarmarekha, calibration and validation period is overlapped.

I assumed that the authors used independent period to validate the calibrated models. If not, please explain the reason.

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