Comment on esd-2021-60
Anonymous Referee #2

In this analysis, the authors attempted to provide a new alternative to track the spatial-temporal variations in water availability in Rio Santa basin (RSB). They acknowledged the limitation of in situ and remote sensing-based rainfall datasets for complex terrain, i.e., the problematic quality and temporal consistence. While it is surprising that the authors found that satellite-derived vegetation greenness (also phenology) was coupled well with recent changes in rainfall (CHIRPS, a combination of satellite and rain gauge data). The authors proposed a “bucket” model to better fit vegetation phenology derived from MODIS NDVI, the concerns also raised in the criteria in extracting SOS/EOS. Some additional serious issues related to the methods and result interpretation, the organization of discussion would weaken the reliability and implication of this study. A major revision is therefore recommended.

First, I have noticed that the authors claimed that both in situ and remote sensing precipitation datasets were questionable in signifying the changes in the timing and intensity of the wet season (e.g., lines: 4-7). However, it is quite strange that the authors used such dataset to demonstrate that the pattern of precipitation occurrence and the seasonality of vegetation indices are tightly coupled (e.g., lines: 10-11). Moreover, the authors used gridded precipitation data as a proxy of water availability (e.g., lines: 179-189). I would like the authors to rephrase these sentences and make the abstract more logical.

Second, some key messages are missing in the method section. i) How did the authors reconcile the NDVI, precipitation and soil moisture data with different spatial resolution? It should be noted that the topographical issues in the studied mountain areas should not be ignored; ii) The authors should provide more details related to the lagged correlation, e.g., the mathematic implementation of a cross correlation function; iii) It is interesting that why evapotranspiration data in the “bucket” model (e.g., Eq. 1) is set as constant over the study period? In other words, the seasonal variations in soil moisture are solely determined by precipitation? Then, where is the advantage of “bucket” model against the seasonal rainfall data (e.g., Liebmann and Marengo, 2001, also Figure A1)? A better criterion to extract SOS/EOS? iv) Similarly, the authors should state the rationale of applying two specific thresholds to define the simulated SOS/EOS (e.g., 0.2 and 0.35 m³/m³, lines: 169-172). Are these thresholds specifically optimized for the NDVI-based
SOS/EOS?

Third, the authors realized the NDVI signals were lagged behind the precipitation (e.g., Figure 2.a). i) Why not presented the variations of precipitation and NDVI after few months (instead of Figure 3). In theory, it could be able to support the coherence of SOS/EOS inferred from vegetation index and precipitation data. Unfortunately, it looks like the NDVI is always greening even take the lagged months into consideration. ii) As is shown in Figure 6, the lags between SOS derived from MODIS NDVI and CHIRPS rainfall data and that for EOS were the same?