This is the response letter to Reviewers' comments on the manuscript **hess-2019-298 "Technical Note: Evaluation of the Skill in Monthly-to-Seasonal Soil Moisture Forecasting Based on SMAP Satellite Observations over the Southeast US"**. The reveiwer's concerns are shown in red text and the author's responses are presented in blue.

Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-298-RC1, 2019

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Technical Note: Evaluation of the Skill in Monthly-to-Seasonal Soil Moisture Forecasting Based on SMAP Satellite Observations over the Southeast US" *by* Amirhossein Mazrooei et al.

Anonymous Referee #1

Received and published: 8 September 2019

I finished my review for the paper titled "Evaluation of the skill in monthly-to-seasonal soil moisture forecasting based on SMAP satellite observations over the southeast US" by Mazrooei et al. This study evaluated the seasonal surface soil moisture forecasting skill based on LSM and dynamical climate forecasts. The SMAP satellite soil moisture observations were used to assess the forecasting skills. Overall, I see the topic of this study fits the HESS journal well and this paper is well-written. I only have one major comment about the drought case study.

I am puzzled why the authors included a drought study here because they focused on the surface soil moisture forecast. It is important to note that drought is a multi-faceted disaster and soil moisture can only partially characterize the agricul-tural drought (vegetation is another import factor). In particular, ROOT-ZONE SOIL MOISTURE should be used instead of SURFACE SOIL MOISTURE. Based on my limited literature review and publications reading, I did not see any important drought paper using the surface soil moisture to quantify drought. In Figure 5, I see a good consistency between the Noah 1-month forecasts and USDM products. However, I am not convinced by the drought severity classification. First, in my opinion, 20-year simulation is too short to estimate soil moisture percentile from a climatological point. Second, 0.5 percentile should be normal condition (i.e., think it in Z-statistics) instead of D0 drought

condition. Note that in USDM, the drought severity is classified as: D0 (abnormally dry, percentile ≤30%), D1 (moderate drought, percentile ≤20%), D2 (severe drought, percentile ≤10%), D3 (extreme drought, percentile ≤5%), and D4 (exceptional drought, percentile ≤2%). See: https://droughtmonitor.unl.edu/AboutUSDM/AbouttheData/DroughtClassification.aspx, and Table 1 in Svoboda et al. (2002). I recommend the authors only focused on surface soil moisture forecast and validation and remove the drought case study.

References: Svoboda, M., LeComte, D., Hayes, M., Heim, R., Gleason, K., Angel, J., ... & Miskus, D. (2002). The drought monitor. Bulletin of the American Meteorological Society, 83(8), 1181-1190.

Response: Thank you for your review and comments. We agree that analysing and forecasting drought conditions should not be performed solely by top-layer SM variables, but the main idea behind the presented case study was to exhibit the high compatibility/similarity between the forecasted percentiles and the monitored drought indexes during a historical severe drought in the region.

As it was suggested by you and the other reviewer, the material related to the case study assessment is now completely removed from the manuscript, and some minor edits are applied in order to maintain the flow of the text as it was.