

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 reviewer's concerns are shown in red text and the author's responses are presented in blue.

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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.

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Review of Technical Note titled "Evaluation of the Skill in Monthly-to-Seasonal Soil Moisture Forecasting Based on SMAP Satellite Observations over the Southeast US"

Recommendation: Minor revision. The manuscript presents a look into generating soil moisture forecasts on a monthly-to-seasonal basis, validated with SMAP data. The topic is interesting and suitable for HESS. The manuscript is well written for the most part. I have a few concerns that I lay out below:

1. P1, L15: Should be “terrestrial”, and “primarily”.

Response: The typos are now corrected.

2. P2, L1-2: Efforts to develop SM forecasts through LSMs have been compared with simulated SM from LSMs? Not sure what the authors intend to state here. Right now, it reads as if the simulated SM is compared with itself.

Response: This is true, it is compared with the products of the same model. Most studies have evaluated the actual forecasts of terrestrial variables (e.g. SM, Streamflow) based on the model products/simulations, rather than actual observations. The difference is then the type of utilized

model forcings (either climate forecasts, or meteorological observations) to conduct these two experiments. This sentence is now rephrased in the manuscript to better convey the message.

Most efforts in developing SM forecasts through land-surface models (LSMs) have actually been compared to the model's SM products under a simulation scheme -using observation-based atmospheric forcings to execute the model- as opposed to actual SM observations (Mo et al., 2012; Mo and Lettenmaier, 2014). Nevertheless, systematic evaluation of our ability to

3. P2, L3-4: Are there not many published studies comparing simulated SM with remotely sensed data? Would they not count as “systematic evaluations”?

Response: There are studies that compare model SM simulations and SM observations, which estimates the errors associated with the “model structure” itself (Narasimhan et al. 2005; Hain et al. 2011). On the other hand, some studies have used model simulations as the benchmark to evaluate the SM forecasts (Mo et al. 2012, Mo and Lettenmaier 2014). However, the focus of this section of our introduction is to highlight the need for evaluating “actual forecasts” based on actual observations over a large domain such as remotely sensed data from SMAP satellite, to be accounted as the “true value”.

- Narasimhan, B., et al. "Estimation of long-term soil moisture using a distributed parameter hydrologic model and verification using remotely sensed data." *Transactions of the ASAE* 48.3 (2005): 1101-1113.
- Hain, Christopher R., et al. "An intercomparison of available soil moisture estimates from thermal infrared and passive microwave remote sensing and land surface modeling." *Journal of Geophysical Research: Atmospheres* 116.D15 (2011).
- Mo, Kingtse C., et al. "Do Climate Forecast System (CFSv2) forecasts improve seasonal soil moisture prediction?." *Geophysical Research Letters* 39.23 (2012).
- Mo, Kingtse C., and Dennis P. Lettenmaier. "Hydrologic prediction over the conterminous United States using the national multi-model ensemble." *Journal of Hydrometeorology* 15.4 (2014): 1457-1472.

4. P2, L31: NLDAS-2 is a comprehensive dataset of what properties?

Response: NLDAS-2 contains 11 fields of meteorological forcings in fine spatial resolution of 1/8° covering U.S., favorable to conduct large-scale hydrological modelling. The content of NLDAS-2 dataset is now explained in the manuscript:

Phase 2 of the North American Land Data Assimilation System (NLDAS-2) is a comprehensive dataset of meteorological forcings available at relatively fine spatio-temporal resolution (hourly temporal scale and 1/8° spatial resolution) from 1979 to present (Mitchell et al., 2004). Hence, it provides a valuable basis to compute hourly climatological forcings for hydrologic

5. P3, L10: ECHAM forecasts are more “skillful”? I am not sure how forecasts can be skillful. They may be more accurate. The ECHAM algorithm may be more skillful at generating more accurate forecasts.

Response: We mean that ECHAM4.5 has skillful predictions of climate variables as opposed to the computed climatology based on long-term data. The term “skillful forecast” is a commonly used term in our publications (Mazrooei et al. 2015; Mazrooei and Sankar 2017;2019), which

infers that a selected methodology/algorithm outperforms another one in developing accurate forecasts.

- Mazrooei, Amirhossein, et al. "Decomposition of sources of errors in seasonal streamflow forecasting over the US Sunbelt." *Journal of Geophysical Research: Atmospheres* 120.23 (2015): 11-809.
- Mazrooei, Amirhossein, and A. Sankarasubramanian. "Utilizing probabilistic downscaling methods to develop streamflow forecasts from climate forecasts." *Journal of Hydrometeorology* 18.11 (2017): 2959-2972.
- Mazrooei, Amirhossein, and A. Sankarasubramanian. "Improving monthly streamflow forecasts through assimilation of observed streamflow for rainfall-dominated basins across the CONUS." *Journal of Hydrology* 575 (2019): 704-715.

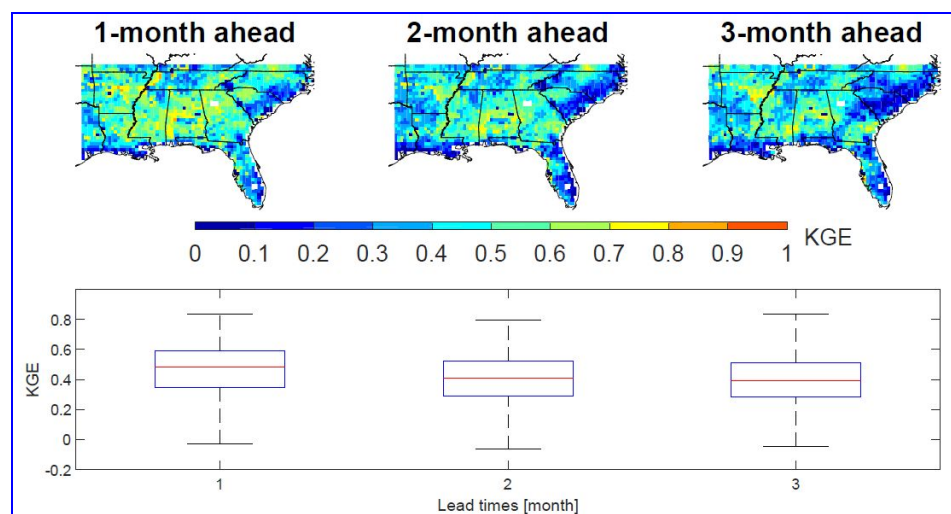
6. P4, L3: SST forecasts “constructed” from what?

Response: The GCM climate model is forced with the updated SST forecasts -developed using the constructed analog SST(CA-SST, Van Del Dool 1994); rather than using observed SST- in order to force GCMs and develop climate forecasts.

Van den Dool, H. M. "Searching for analogues, how long must we wait?." *Tellus A* 46.3 (1994): 314-324.

7. P6, L1-6: This is surprising. It has been shown many times in the past that the spatial variability of SM reduces significantly at the wet end of the curve. Hence, I would expect the performance to be better over the wetlands. Any thoughts as to why this phenomenon is observed in your study?

Response: Since RMSE is quantified using squared differences, the forecasting skill tends to be overestimated over low SM quantities and underestimated over high SM quantities. Another appropriate approach to quantify and visualize the forecasting skill is to use Kling-Gupta Efficiency (KGE) score (Gupta et al., 2009), which simultaneously accounts for correlation coefficient, mean bias, and relative variability in the predictions and observations. This additional analysis is now presented in this discussion letter for a better comparison between wet/dry regions of our study.



Gupta, Hoshin V., et al. "Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling." Journal of hydrology 377.1-2 (2009): 80-91.

8. I would prefer it if the Results and Discussion sections were merged, with the discussion happening immediately after each result is presented. The current split format makes me skip ahead to the Discussion after each result is presented, just to see what the authors make of it.

Response: Given that we have explained the methodology first, and then arranged the figures as a set of results, we would like to present them separately in the result section, followed by the discussion. We believe that the current flow of the manuscript is reasonable.

9. I am not really convinced that the drought section fits into this study. **a.** The rest of the study uses SMAP as the benchmark, this portion uses USDM data, which are probably not at the same resolution as the simulations. **b.** Further, no numbers are presented for this comparison. I am not sure the spatial patterns really match that well. For example, the Florida panhandle is shown to be extremely dry in the Noah plot, but USDM says different. At the very least, another map showing the difference between the Noah and USDM values would've given a better picture. **c.** Thirdly, using surface SM to forecast drought is making things too simplistic. There are various other factors that need to be taken into account. These thoughts lead me to recommend that the authors remove the drought study from this note.

Response: 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. We agree that analysing and forecasting drought conditions should not be performed solely by top-layer SM variables. 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.

Thanks for the detailed review and comments.