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Interactive comment on "Controls on surface soil drying rates observed by SMAP and simulated by the Noah land surface model" *by* Peter J. Shellito and Eric E. Small

Anonymous Referee #1

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The authors estimate soil drying rates by identifying soil moisture "drydowns" in time series of SMAP observations and Noah land surface model outputs. They compare the estimated drydown rates to covariates such as NDVI, potential evaporation and soil texture, and discuss differences between SMAP and Noah. The study is fairly well written, and the analyses are generally thorough. For example, the authors thoroughly consider important factors such as differences between soil moisture depths of SMAP and Noah in their analyses.

The paper's biggest weakness, in my view, is in its motivation and framing. The story should be tighter. For example, the first line of the abstract says "Drydown periods that

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follow precipitation events provide an opportunity to assess the mechanisms by which soil moisture dissipates from the land surface." Various mechanisms contribute to soil moisture dissipation – bare soil evaporation, transpiration, percolation, runoff – but only one of these mechanisms is directly estimated from drydowns in the manuscript (bare soil evaporation). It is not clear to me how one would assess the other mechanisms by examining drydowns, and the reader is not provided with any further guidance or results in the manuscript. Perhaps the story could be reframed around estimating bare soil evaporation.

As far as I can tell, this is essentially a model validation study, except it excludes much of the available data from the analysis (i.e., only includes drydowns). What is gained from doing this that could not be gained from a standard validation study that uses the full time series of data? The authors partially and indirectly answer this question, but the paper would be more compelling if they spent more time directly addressing the paper's motivation. What exactly do we learn from examining soil moisture drydowns in isolation? What specific additional information do we obtain from this analysis compared to a standard validation study that uses all the data, not just drydowns?

The lack of a clear story also leads to some strange decisions in the data analysis. In Figure 5, for example, soil moisture drydowns are averaged across space and time, but with only a partial normalization in time, and no normalization in space. Averaging nonlinear (for example, exponential) drydowns with varying additive and multiplicative biases would be expected to significantly dampen the nonlinearity and cloud interpretation. Removing the mean of each drydown and scaling VSM to a saturation ratio before averaging would better preserve the expected exponential form of the average drydown. The authors' data analysis choices are not necessarily wrong, but they need to be justified in the context of a larger story that is currently not well-articulated.

I am also concerned about the robustness of the approach used by the authors to estimate drydowns, which relatively frequently produces unphysical, negative evaporative efficiencies. I offer some specific suggestions below.

Specific comments The introduction could be tighter. A lot of space is devoted to recapping fundamental physics of vadose zone hydrology. Much of this could be edited down, moved to later parts of the manuscript, or cut completely. Focus on motivation for the study here.

p. 3, line 29: “Soil moisture supply, PE rate, and vegetation cover are observed...” PE rate is not observed. It is estimated from a reanalysis which is significantly model-based.

Fig. 1: The SMAP orbit is clearly visible in Figs 1a and 1b. This should be noted as an unphysical artifact of the observations.

p. 7, line 3: how sensitive are the results to choices made in the automated selection process? Comment on this in the manuscript.

p. 8, line 9: “This is an artifact of the SMAP algorithm and does not reflect the drying process.” How do the authors know this? Please cite a reference or personal communication, if necessary.

p. 8, line 21: “Noah drying rates have a nearly 1:1 relationship with evaporation rates.” The relationship is substantially noisy with R = 0.47, and clearly not “1:1”. Perhaps rephrase to something like “the slope of the regression line is nearly 1”.

p. 14, line 12: “SMAP exhibits a considerable number of evaporative efficiency values below zero.” Indeed, there is a substantial fraction below zero, and this makes me question the accuracy of the estimates above zero, too. The authors attribute this to noise in the SMAP observations; if it is due to noise, then the authors need to redesign their analyses to be more robust to it. The authors should revisit their drydown algorithm given on p. 7 to ensure this fraction is lower. For instance, they could return to fitting an exponential model, rather than directly estimating soil drying rates from finite differences. They should also alter the criterion “the dry period must be at least 3 days long” (p. 7) to require the dry period to be longer. Three days is quite short given the SMAP revisit time is ∼3 days and makes the algorithm highly susceptible to noise in the observations.

p. 15, line 8: “vegetation amount (as indicated by NDVI)” NDVI is a measure of greenness, not vegetation amount. This is only loosely correlated with vegetation amount. Please add nuance to this statement. Also, since NDVI is an optical index and is therefore not observed during cloudy conditions, how do the authors expect this to impact their results? For example, cloudy conditions will lower PE, all else being equal; therefore these conditions are being systematically excluded from the comparison. Comment on this in the manuscript.

p. 19, line 6: “. . .are completely independent from the Noah model results.” NDVI and FG are hardly independent of one another: FG is estimated directly from NDVI!

p. 21, line 17: “Therefore, results confirm that these fundamental relationships exist at the continental scale.” To me, this is the most interesting story in the manuscript: translating known results from the point scale, to continental scales.

p. 23, line 1: “NDVI, and therefore vegetation amount,. . .” NDVI is a measure of greenness, not vegetation amount.

p. 23, line 18: “The results in Figure 10 show that the sensitivity to soil texture is too high in Noah.” Possibly, although an alternative explanation could be that the soil texture maps used are themselves error prone and insufficient, particularly at the large spatial scales relevant to SMAP and Noah.

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