This study applies multiple methods to identify critical soil moisture (CSM) that separates water- and energy-limited regimes using several satellite-based data and in-situ observations with a specific spatial scope. Then, it explores the factors that dominate the variations using a feature regularization technique. I support this study as I think their analyses on CSM and the determinant factor advance the science on the water and energy cycle over land. However, I think the readability of the paper and the description of the analyses should be further improve. Moreover, I have concerns about the confidence in the CSM estimates. I suggest a major revision. Please see my comments below:

I do not wish to remain anonymous – Hsin Hsu

Major Comments

1. The article is very hard to read because the amount of abbreviation is overwhelming. I had to look up what an abbreviation stands for multiple times in just one sentence as they are from different categories (variables, locations, names of in-situ sites, land cover types, algorithms, statistical parameters, data products, etc.). I suggest retaining the full names for land cover types and some algorithms in the writing, as many do not occur frequently in the paper. The authors could also consider separating the abbreviations by different systems. For example, use Greek alphabets for statistical parameters and italics for variables.

The main methodologies used in this study may need abbreviations due to their lengthy names (e.g., Corr(ET,VPD) - Corr(ET,SM)). Sometimes they occur many times in one paragraph (or even one sentence), but the key information separating different correlation-difference methods is the second variable used in the first correlation calculation. The authors could consider modifying the notation of Denissen et al. 2020 to define:
$\Delta Corr_{Var}$ = Corr(ET,Var) - Corr(ET,SM).

2. Most of the cited work on critical soil moisture and regime examination is published before 2022. There are many new aspects of regimes and CSM since 2023. Not required to reference but the authors could consider integrating these recent studies:

i. New method for calculating CSM based on satellite data:
Fu et al. (2024). Global critical soil moisture thresholds of plant water stress. https://doi.org/10.1038/s41467-024-49244-7
ii. Global estimation of CSM based on soil moisture dry-down framework and an index to quantify vulnerability:
Dong et al. (2023). Land Surfaces at the Tipping-Point for Water and Energy Balance Coupling. https://doi.org/10.1029/2022WR032472
iii. In the abstract (line 11), author mentions that regimes can shift under climate change. This is not discussed in the introduction:
Hsu, H., Dirmeyer, P.A. (2023). Soil moisture-evaporation coupling shifts into new gears under increasing CO2. https://doi.org/10.1038/s41467-023-36794-5
Hsu, H., Dirmeyer, P.A. (2023). Uncertainty in Projected Critical Soil Moisture Values in CMIP6 Affects the Interpretation of a More Moisture-Limited World. https://doi.org/10.1029/2023EF003511
Duan et al. (2023). Coherent Mechanistic Patterns of Tropical Land Hydroclimate Changes. https://doi.org/10.1029/2022GL102285.

3. The robustness of each technique for estimating CSM is not described:

i. From Figure 4, it seems that CSM can be identified in almost all grid cells by the covariance method. Is this also true for all other methods? If not, what is the rate of agreement on the existence of CSM among all the methods?

ii. Do all methods for estimating CSM have significant tests? For example, when using the SM-EF method, it is common to use three different linear models (flat line, a positive slope line, and a linear-plus-plateau) and select the best one based on the Bayesian information criterion (BIC). If flat-line regression or positive-slope regression outperforms linear-plus-plateau regression, CSM should be considered as not identified. This procedure is also used for detecting CSM by the soil moisture-drydown framework.

iii. When using the correlation-difference method, if there is more than one SM value where the correlation-difference is zero, which SM value is identified as CSM? In Figure 3b, the red line locates at the wetter SM value when the correlation-difference is zero, but blue-green line locates at the drier SM value when the correlation-difference is zero. This should be clarified. Additionally, I assume that if either correlation is not statistically significant when calculating the correlation before taking the difference, CSM should also be treated as not identified. Is this the case in this study?

4. When examining the alignment of CSM between different methods, statistical significance is needed. I recommend a Chi-square test as it can address scenarios involving categorical data: comparing rates or proportions between two groups when the outcome is a binary variable, such as negative and positive outcomes. In this specific case, categorical data represent soil moisture (SM) values tagged as "drier-than-CSM" and "wetter-than-CSM". So, a Chi-square test can be used to compare the proportions of SM values below and above CSM between two sets of variables or groups (obtained by different methods). If there are significant differences, it means the CSM is different.

5. Figure 5 seems problematic. The CSM is extremely well aligned among different ET products. However, I assume the spread in temporal variation among ET products over many locations based on Figure 2a, where the correlation of each product's ET to in-situ data can be very different. Does that not lead to a different CSM estimate? The authors could provide some supporting information to justify the consensus, which looks too good. The tick labels on the y-axis in each bar chart are incorrect. The bars are spatial means, so error bars should also be provided.

6. In Figure 6, the CSM among different SM layers is also extremely well aligned (if I interpret it correctly). This makes me doubt the reliability of SM in deeper layers. I assume SM-EF could be decoupled in deeper soils if roots do not reach that deep in

some places, so there should be some inconsistency in CSM values. In either case, I suggest the authors provide additional analysis to examine soil moisture at deeper layers (maybe taking some grid cells for example and put as supporting information) and discuss the uncertainty of using these data products in the discussion. For example, some ET products are estimated; are the sources of input to derive ET independent of each other for all of them? Does method to derive SM at different layer inherently lead to consistency in CSM?

7. How does the author determine the set of input features for the ridge regression and why air temperature and VPD are not considered in the analysis?

Minor Comments

1. Line 213: should be "ea" not "ea."
2. Line 122: The term REddyProc is not explained nor mentioned elsewhere.
3. I suggest putting the unit of variables in every chart.
4. Is ΔCorr calculated monthly between June and September and then an annual mean is obtained?
5. The word “Slope” in figures 7 and 8 is confusing. Is this a temporal trend? What is the statistical significance?
6. Does the author perform cross-validation or bootstrapping for ridge regression?