

Review of the paper *A Study of the Dependence between Soil Moisture and Precipitation in different Ecoregions of the Northern Hemisphere*

Please note that I wrote my review before reading the comments from the first stage of the review process. Nevertheless, I share many of the same concerns previously raised, most importantly, the novelty of the paper is not clearly stated. While the research topic is certainly interesting and the relationships among the analyzed variables remain an open area of investigation, the manuscript fails to clearly explain how it contributes new insights or identifies novel relationships.

Specific Comments:

- **Line 75:** *“Most current analyses of the relationship between soil moisture and precipitation assume a linear relationship.”*
This is an important statement. Could you please provide supporting references to substantiate this claim?
- **Data Sources:**
You use soil moisture data from ERA5, which also provides precipitation and near-surface air temperature data. Why did you not use ERA5 for all variables, where available, to ensure consistency?
- **Line 207:**
“In this study, the ecoregion boundaries rather than Köppen climate zones were used to investigate the spatial patterns of precipitation–soil moisture feedbacks.”
If you are not using Köppen climate zones, why are you mentioning them here?
Please clarify the purpose of this comparison.
- **Line 331:**
“The percentages of grid cells exhibiting negative dependence at these depths were 19.2%, 0.7%, and 2.3%, respectively.”
Is 0.7% a meaningful value in this context? Also, the decrease in negative dependence with increasing depth is expected, but why does the middle layer exhibit *less* negative dependence than the deepest layer? This result seems counterintuitive. Can you provide a physical interpretation?
- **Lines 341–349:**
The variables λ_U and λ_L are formatted inconsistently. Please correct the notation. Additionally, you describe their behavior without providing any interpretation of what these patterns mean physically or statistically.
- **Monthly vs. Yearly Maps:**
How do you interpret the differences between the maps based on monthly data and those based on yearly data? This comparison is presented, but not adequately discussed.
- **Seasonality:**
Time series of climatological data are typically affected by seasonal cycles. However, you do not seem to have removed seasonality in your analysis. This is a standard procedure when applying copula models and tail dependence metrics. See, for instance:
 - Neumeyer, N., Omelka, M., & Hudecová, Š. (2019). *A copula approach for dependence modeling in multivariate nonparametric time series*. Journal of Multivariate Analysis, 171, 139–162.
 - Durante, F., Fuchs, S., & Pappadà, R. (2025). *Clustering of compound events based on multivariate comonotonicity*. Spatial Statistics, 66, 100881.

You acknowledge the presence of seasonality, so why not attempt to de-seasonalize the data and compare the results? This would add robustness to your analysis.

- **Line 379:**

“The results from ridge regression revealed more distinct patterns at the seasonal scale compared to the monthly scale (Fig. 5).”

This observation further highlights the influence of seasonality. It reinforces the concern that the analysis may benefit significantly from seasonal adjustment across the board.

- **Figure 7 and Clustering Approach:**

How are you performing clustering? In Figure 7, clusters appear to overlap. This suggests the use of fuzzy clustering, yet you previously stated (line 319) that K-means was used, which does not allow for overlapping clusters. Please clarify and correct any inconsistencies in your methodology description.

- **Section 4.3 – Data Reliability:**

This section feels largely redundant with Section 2.1. Consider consolidating or cross-referencing to avoid repetition.

- **Figure Quality:**

Some figures still appear to be of low resolution or poor quality. Please ensure all figures are adequately rendered and suitable for publication.