

General assessment:

This manuscript addresses an interesting and timely question: whether the dominant drivers of vegetation stability shift across gradients of hydroclimatic extremity. The global scope, the stratification by land-cover type and SPEI category, and the attempt to integrate climatic, biodiversity-related, edaphic, topographic, and land-use variables are all strengths. The paper is also clearly motivated in the Introduction, especially in distinguishing event-based resistance/recovery from stochastic-resilience approaches.

However, I have substantial concerns about the current implementation and interpretation. **In particular, the definitions of resistance and resilience, the treatment and interpretation of “biodiversity” predictors, and the inference drawn from LightGBM feature importance need much stronger justification.** I am open to the authors using different thresholds or formulas, but these choices need to be justified, ideally with support from relevant literature. It would be even better if the authors could include specific reasoning or comparative results in the Methods or supplementary materials. At present, some conclusions appear stronger than what the data and methods can support. I therefore recommend major revision.

Major comments

1. The definitions of resistance and resilience need stronger conceptual and mathematical justification

The formulas used here to quantify resistance and resilience are unusual and, to my knowledge, not among the commonly used formulations in this literature, where ratio-based (Isbell *et al.* 2015), bounded (Orwin & Wardle 2004; Liu *et al.* 2022), and log-response (Xu *et al.* 2022; Yan *et al.* 2025) metrics are more typical. Since the entire analysis depends on these definitions, the authors should provide a clear justification for adopting these equations instead of more standard alternatives. Without such justification, it is difficult to assess the ecological meaning and comparability of the reported results.

2. NDVI as a choice for indicating ecosystem function

The manuscript would benefit from a clearer justification for using NDVI as the vegetation activity metric. In many large-scale vegetation studies, EVI is often

preferred because it can be less sensitive to saturation in high-biomass systems and less affected by soil and atmospheric background noise. The authors should therefore explain why NDVI was chosen here and discuss whether the main results would likely hold if EVI were used instead. A sensitivity check with EVI would greatly strengthen confidence in the conclusions.

3. The interpretation of biodiversity-related predictors is currently overstated

The paper repeatedly refers to ASI, ASL, and ASR as biodiversity controls and interprets their emergence as top drivers under severe drought as evidence that “biodiversity” becomes more important than climate. But these are not direct observations of local biodiversity; they are modeled anthropogenic species increase/loss/richness products derived from broad-scale data layers. The paper does acknowledge uncertainty in these products, but the discussion still treats them as if they were direct ecological mechanisms.

This needs to be toned down and clarified. Specifically:

Avoid statements implying causal effects of species gain/loss unless supported by independent evidence.

Discuss the possibility that these variables are serving as latent correlates of human modification, land-use intensity, or biome context.

Show whether these proxies remain important after accounting for climate and land-use variables in a way that reduces confounding.

As written, the conclusion that driver dominance shifts “from climatic to biodiversity” is too strong.

4. The thresholding of “interpretable models” is arbitrary and may bias conclusions

The manuscript emphasizes results only when both $R^2 > 0.5$ and variation in resistance/resilience > 0.01 , with bolding in Table 2 restricted accordingly. This filtering may be reasonable as a heuristic, but it needs justification. Otherwise, it risks creating a narrative in which some systems appear to lack clear controls simply because of an arbitrary interpretive rule.

Please explain:

why 0.5 and 0.01 were chosen,

whether the conclusions change under slightly different thresholds,

whether the same systems emerge as “responsive” under a continuous interpretation rather than thresholding.

5. The paper needs clearer separation between pattern, prediction, and mechanism

The manuscript is strongest when it describes broad patterns: under normal conditions, temperature-related variables are frequently most important, whereas under severe drought some sparsely vegetated systems show stronger contributions from non-climatic predictors. That is a useful empirical pattern. But the discussion often goes further, implying mechanistic explanations that are not directly tested. For example, statements suggesting that species loss contributes to reduced drought resistance or that introducing drought-tolerant crop varieties shapes stability are plausible, but not demonstrated by the data used here.

Please revise the discussion and conclusion so that:

statistical association is clearly distinguished from mechanism,

speculative mechanistic interpretations are labeled as hypotheses,

the strongest claims are reserved for patterns actually tested.

6. Abbreviations and variable symbols should be revised to better match common disciplinary conventions. For example, Table 1 uses “Ca” for soil organic carbon, although “Ca” is conventionally reserved for calcium, which may confuse readers. Similar notation choices should be checked throughout for clarity and consistency.

Minor comments:

- 1) There is an apparent inconsistency in spatial resolution wording. Section 2.1 states the datasets were “harmonized to a $5^\circ \times 5^\circ$ spatial grid,” but later states all datasets were harmonized to a 5-min resolution. This needs correction because 5° and 5 min are very different.
- 2) The notation in the normalization section is confusing. Equation (4) labels X_{mean} as “mean” but the equation seems to use a min–max form, and the variable

definitions below the equations appear mismatched. Please check and rewrite these equations carefully.

- 3) Please define more clearly what is meant by “physiologically plausible range of 0.8–1.2” for boxplot visualization. Why were these bounds chosen, and could clipping obscure true extremes?
- 4) The manuscript should report uncertainty more systematically in the results, not just medians and selected R^2 examples.
- 5) The text uses “resilience” in the Introduction but “resilience” and “recovery” somewhat interchangeably in places. Please standardize terminology.
- 6) Some wording needs polishing. For example, “may implicitly shape observe responses” should be “observed responses.”
- 7) Table 1 contains several wording issues, including “Explanation of valuables in Light GBM,” which should be “variables,” and “short radiation,” which likely should be “shortwave radiation.”
- 8) Figures are informative, but the manuscript would benefit from one synthesis figure showing how driver classes shift with event severity across land-cover types.

Literatures:

1.

Isbell, F., Craven, D., Connolly, J., Loreau, M., Schmid, B., Beierkuhnlein, C. *et al.* (2015). Biodiversity increases the resistance of ecosystem productivity to climate extremes. *Nature*, 526, 574-577.

2.

Liu, S., García-Palacios, P., Tedersoo, L., Guirado, E., van der Heijden, M.G.A., Wagg, C. *et al.* (2022). Phylotype diversity within soil fungal functional groups drives ecosystem stability. *Nature Ecology & Evolution*, 6, 900-909.

3.

Orwin, K.H. & Wardle, D.A. (2004). New indices for quantifying the resistance and resilience of soil biota to exogenous disturbances. *Soil Biology and Biochemistry*, 36, 1907-1912.

4.

Xu, Q., Yang, X., Song, J., Ru, J., Xia, J., Wang, S. *et al.* (2022). Nitrogen enrichment alters multiple dimensions of grassland functional stability via changing compositional stability. *Ecology Letters*, 25, 414-424.

5.

Yan, P., He, N., Fernández-Martínez, M., Yang, X., Zuo, Y., Zhang, H. *et al.* (2025). Plant Acquisitive Strategies Promote Resistance and Temporal Stability of Semiarid Grasslands. *Ecology Letters*, 28, e70110.