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Revision notes: We sincerely thank the Editor for recognizing the potential value of our manuscript and for the time and effort devoted to handling the review process, including coordinating the assessment and inviting expert reviewers. We also sincerely thank both reviewers for their careful reading of our manuscript and for providing constructive and insightful comments. These comments have helped us identify several aspects that require further clarification and improvement. In the following response, the reviewers' comments are shown in black, and our point-by-point responses and planned revisions are shown in blue. At this final-response stage, we provide a detailed explanation of how we will revise the manuscript according to the Editor's and reviewers' suggestions. We will carefully improve the Methods, Results, Discussion, figures, captions, and uncertainty analysis to make the manuscript more rigorous, transparent, and internally consistent. We respectfully hope to be given the opportunity to revise the manuscript and incorporate the suggested modifications.

Responses to the reviewer's comments

Response to Reviewer #2

We sincerely thank the reviewer for the careful evaluation of our manuscript and for providing detailed and constructive comments. We fully recognize that the current version still requires substantial clarification in several key aspects, including the distinction between established knowledge and our quantitative contribution, the assumptions and spatial scale of the moving-window approach, the consistency between temporal trends and sensitivity estimates, the readability of Figures 1 and 2, the reporting of nonlinear VPD thresholds, and the interpretation of the SEM pathways. In the responses below, we provide a point-by-point plan for how we will revise the manuscript in accordance with the reviewer's suggestions. We will carefully check the data processing, clarify the temporal and spatial definitions of the analyses, correct inconsistent threshold values, strengthen the uncertainty discussion, and revise the figures, captions, Methods, Results, and Discussion accordingly. We sincerely appreciate the reviewer's comments, which have helped us identify several aspects that require further clarification and improvement, and we respectfully hope to be given the opportunity to revise the manuscript and incorporate these suggested modifications in a more rigorous and transparent way.

Comment 1: The abstract should better distinguish established knowledge from the novel contribution of this study. Statements that VPD affects evapotranspiration, that soil moisture limitation constrains E in arid regions, and that transpiration is an important pathway are broadly consistent with existing understanding. The main potential contribution of this work is the global quantification of VPD–E sensitivity, regional heterogeneity, and apparent VPD thresholds. The abstract should emphasize this quantitative contribution rather than presenting well-known mechanisms as new findings.

Response: Thank you for this constructive comment. We agree that the abstract should more clearly distinguish established knowledge from the new contribution of this study. The effects of VPD on evapotranspiration, the role of soil moisture limitation in dry regions, and the importance of transpiration are already well recognized in previous studies. In the revision, we will avoid presenting these mechanisms as novel findings. Instead, we will use them only as background to introduce the scientific context.

We will revise the abstract to emphasize the quantitative contribution of this study. Specifically, we will highlight that our work provides a global estimate of VPD–E sensitivity, identifies the spatial heterogeneity of this sensitivity across climate and land-cover conditions, and quantifies apparent VPD thresholds in different aridity zones. We will also report the main numerical results more directly, including the global mean sensitivity, the proportion of land showing positive or negative VPD–E sensitivity, and the aridity-zone-dependent threshold values. These changes will make the abstract more focused on what this study adds beyond existing knowledge, rather than restating mechanisms that are already broadly understood.

Comment 2: The space-for-time moving-window approach is not sufficiently explained. This method can be useful for estimating the local apparent sensitivity of E to VPD, but it relies on strong assumptions: neighbouring pixels must share similar climate, soil, vegetation, topography, and management conditions, and spatial VPD gradients must be a reasonable proxy for temporal VPD changes. The manuscript should clarify how "similar background climate" was defined and whether differences in soil moisture, radiation, elevation, vegetation structure, and land management were controlled.

Response: We agree that the space-for-time moving-window approach needs a clearer explanation. This method is useful for estimating the local apparent sensitivity of E to VPD, but it does rely on important assumptions. In the revision, we will explicitly define this estimate as an apparent local sensitivity, rather than a strict causal response or a direct substitute for temporal VPD change. We will also clarify that "similar background climate" is mainly constrained by the local moving-window design. All calculations will be conducted on a common 0.1° grid, and each target pixel will be compared only with candidate pixels within a 5 × 5 grid-cell neighborhood, corresponding to a 0.5° × 0.5° local window. This local window is intended to reduce large-scale climatic differences among compared pixels.

We will further clarify how surface heterogeneity is controlled within this window. Candidate pixels will be retained only when they share the same dominant MODIS land-cover type as the target pixel. The difference in the fraction of the dominant land-cover type will be required to be less than 10%. Pixels with an elevation difference greater than 100 m from the target pixel will also be excluded. These screening rules are designed to reduce differences in vegetation type, land-cover composition, and topographic conditions. We will describe these criteria more explicitly in the Methods section, so that readers can understand how the moving-window samples are selected.

At the same time, we will state the limitations of this approach more clearly. Soil moisture, radiation, soil properties, vegetation structure, and land management cannot be fully controlled by the current moving-window screening. They may still differ among neighboring pixels, especially in irrigated croplands, heterogeneous landscapes, mountainous areas, and coastal regions. We will therefore avoid claiming that the method fully isolates the independent effect of VPD. Instead, we will explain that the method reduces some major sources of spatial heterogeneity but cannot remove all confounding factors. We will add this limitation to the Methods and Uncertainty sections, and we will interpret dE/dVPD as a spatially derived apparent sensitivity that should be considered together with the partial correlation, SEM, and independent dataset checks.

Comment 3: The spatial scale of the moving window needs to be explained. The authors mention using a 5 × 5 km window, but this seems quite small relative to the spatial resolution of the input datasets. The manuscript should clarify the final resolution of the analysis and the resampling procedure for each dataset. Sensitivity tests with different window sizes are recommended to demonstrate the robustness of the results.

Response: Thank you for this helpful comment. We agree that the original description of the moving-window scale was confusing. The analysis is not conducted using a 5 × 5 km window. In the revision, we will correct this wording and clearly state that all analyses are conducted on a common 0.1° grid. This grid is used for the ERA5-Land VPD data and the GLEAM E data in the main analysis. The moving window will therefore be defined as a 5 × 5 grid-cell neighborhood, corresponding to a 0.5° × 0.5° window centered on each target pixel, rather than a 5 × 5 km window.

We will also add a clearer description of the resampling and harmonization procedure. Continuous variables will be harmonized to the common 0.1° grid before the moving-window calculation. For the MODIS MCD12C1 land-cover product, we will use the dominant land-cover type and the fractional cover of that dominant type within each 0.1° grid cell. Candidate pixels will be retained only when they share the same dominant land-cover type as the target pixel, and when the difference in dominant land-cover fraction is less than 10%. Elevation will also be harmonized to the same grid, and candidate pixels with an elevation difference greater than 100 m from the target pixel will be excluded. We will describe these rules explicitly in Section 2.2.2 so that the spatial scale, input resolution, and sample selection procedure are clear.

Following the reviewer's recommendation, we will add a window-size sensitivity test to evaluate the robustness of the moving-window result. In addition to the main 5 × 5 grid-cell window, we will test alternative window sizes, such as 3 × 3 and 7 × 7 grid-cell neighborhoods, while applying the same land-cover and elevation screening criteria. We will compare the resulting dE/dVPD patterns with the main result in terms of sign consistency and broad spatial distribution. These results will be reported in the Supplementary Information. This additional test will help show whether the main spatial pattern of VPD sensitivity depends strongly on the selected window size. We will also soften the interpretation of dE/dVPD and describe it as a local apparent sensitivity, rather than a fully isolated causal effect of VPD.

Comment 4: The abstract states that E increases with rising VPD over 60.7% of the land surface, whereas Section 3.1 states that E decreases in areas where VPD increases, suggesting that atmospheric drying has an inhibitory effect. Section 3.2 then reports that 60.71% of land areas show positive sensitivity. This description is confusing and should be clarified.

Response: We agree that the original wording may cause confusion because it mixed two different quantities: the temporal trend of E and the sensitivity of E to VPD. The statement in Section 3.1 refers to the spatial co-occurrence of long-term E trends and VPD trends, whereas the result in Section 3.2 refers to the estimated local apparent sensitivity, dE/dVPD. These two results are related but not identical. Therefore, the manuscript should not describe them using the same wording.

In the revision, we will clarify this distinction throughout the Abstract, Section 3.1, and Section 3.2. In the Abstract, we will avoid the ambiguous phrase "E increased with rising VPD." Instead, we will state that 60.7% of the global land surface shows positive apparent sensitivity of E to VPD, while negative or weak sensitivity is mainly found in water-limited regions. In Section 3.1, we will describe only the long-term temporal changes of VPD and E. In Section 3.2, we will separately present the spatial dE/dVPD sensitivity results. This revision will make clear that the 60.7% value refers to sensitivity, not to the fraction of land where E and VPD both increased over time. We will also adjust the related wording to avoid implying that rising VPD universally enhances E, because the response depends strongly on soil moisture and aridity conditions.

Comment 5: Figure 1 is difficult to interpret. The colorbars in panels (a) and (b) should clearly indicate the variable, unit, and sign convention. The text states that significant increases in VPD cover 76.22% of the land surface, but the figure does not clearly distinguish significant from non-significant trends. Hatching, stippling, or a separate significance panel should be added. I also suggest revising the colorbars so that decreases and increases are clearly represented by blue and red, respectively, with zero set as the midpoint.

Response: The colorbars and significance information should allow readers to understand the variable, unit, sign convention, and statistical significance directly from the figure. In the revision, we will redraw Figure 1 to improve its readability. For the VPD trend map, the colorbar will be labelled as Sen's slope of VPD, with the unit hPa yr^{-1}. For the E trend map, the colorbar will be labelled as Sen's slope of E, with the unit mm yr^{-1}. We will also explicitly define the sign convention: positive values indicate increasing trends, while negative values indicate decreasing trends.

Following the reviewer's suggestion, we will use a diverging color scale for the trend maps, with zero set as the midpoint. Decreasing trends will be shown in blue and increasing trends in red. This will make the spatial pattern of increases and decreases easier to interpret. We will also add stippling to indicate pixels with statistically significant trends at $P < 0.05$. This change will make it clear which regions contribute to the reported significant increase in VPD over 76.22% of the land surface, and which regions show non-significant trends.

We will further revise the figure caption to describe these changes explicitly. The revised caption will state that panel a shows Sen's slope of VPD, with stippling indicating significant trends at $P < 0.05$, and that panel d shows Sen's slope of terrestrial E with the same significance convention. We will also clarify that the insets show the percentage distribution of pixels among slope classes. For the time-series panels, we will clearly identify the monthly global mean, annual mean, 5-year moving average, anomalies relative to 1981–2020, and Theil-Sen or piecewise trends where applicable. These revisions will make Figure 1 more self-contained and will better connect the visual evidence with the quantitative statements in the text.

Comment 6: Figure 2b is particularly confusing. A large fraction of samples appears to fall between approximately -7 and -28 °C. While such temperatures may occur in high-latitude or high-elevation regions, their dominance raises concerns that cold, ice-covered, tundra, or very low-E regions may be overrepresented. The precipitation range also appears too low if it represents annual precipitation; humid and tropical regions commonly exceed 300 mm yr^{-1}. The authors should clarify whether the axes show annual means or monthly values, whether each point represents a grid cell or a grid cell–month sample, whether area weighting was applied, and whether permanent ice/snow, barren, or very low-LAI regions were excluded.

Response: We agree that Figure 2b is difficult to interpret in its current form and may raise concerns about sample representation and data consistency. In the revision, we will recheck the input data used for this panel and redraw the figure after confirming the temporal scale, spatial sampling unit, and masking procedure. In particular, we will verify whether the temperature and precipitation values represent annual means, monthly means, or grid cell–month samples. We will also check the precipitation calculation to ensure that the unit and aggregation period are consistent with the interpretation in the figure and text. We will revise the figure caption and Methods to state explicitly what each point represents. We will clarify whether the points are individual grid cells or grid cell–month samples, and whether area weighting is applied when summarizing the distribution. If the figure is based on monthly samples, we will label the axes accordingly and avoid interpreting the values as annual climatic conditions. If annual means are used, we will ensure that precipitation is aggregated as annual total precipitation rather than monthly precipitation, and we will revise the axis unit accordingly.

We also agree that cold, ice-covered, tundra, barren, or very low-vegetation regions may be overrepresented and may distort the relationship shown in Figure 2b. In the revision, we will apply additional screening before remaking this figure. Permanent snow and ice areas will be excluded. We will also examine whether barren land, tundra, and very low-LAI pixels should be removed or shown separately, because these regions often have very low E and may not represent the vegetation-mediated VPD-E response that is central to this study. We will clearly report the final masking criteria in the Methods and figure caption.

After these checks, we will remake Figure 2b and revise the related text accordingly. If the corrected figure still shows a large influence of cold or low-E regions, we will interpret this pattern explicitly rather than treating it as a general global response. If the original pattern is found to result from inconsistent temporal aggregation or insufficient masking, we will replace it with the corrected result. These revisions will make the figure more transparent and will ensure that the conclusions drawn from Figure 2b are supported by reliable and consistently processed data.

Comment 7: Some explanations in the Results section, particularly lines 284–287, are not fully supported. In addition, the threshold results are unclear. Line 333 mentions that the arid-zone threshold is about 1.31 kPa, lines 337–338 refer to "temperate zones" with values of 1.67–1.68 kPa, and lines 406–409 again mention "arid regions" with values of 1.67–1.68 kPa. These inconsistencies should be corrected.

Response: In particular, the text around Lines 284-287 mixed direct result description with mechanistic interpretation. In the revision, we will rewrite this part to ensure that the Results section only reports patterns that are directly supported by the data. Interpretations related to physiological regulation, soil moisture limitation, and land–atmosphere feedbacks will be moved to the Discussion, where they can be linked more explicitly to the partial correlation analysis, SEM path coefficients, and previous literature. We will also avoid causal wording unless it is directly supported by the corresponding analysis.

We also agree that the threshold values were inconsistent in the previous version. The manuscript originally mixed different classification schemes and therefore reported incompatible values, including an arid-zone threshold of about 1.31 kPa, "temperate-zone" thresholds of 1.67-1.68 kPa, and later references to arid regions using the same 1.67-1.68 kPa range. This inconsistency will be corrected throughout the manuscript. In the revision, we will use one consistent classification framework based on aridity zones. The threshold results will be reported as: arid zone, semi-arid zone, semi-humid zone and humid zone. The earlier inconsistent values will be removed from the Results, Discussion, Abstract, and Conclusion.

We will further clarify how these thresholds are derived and interpreted. We will state that the thresholds are estimated from the comparison of one-breakpoint piecewise regression, two-breakpoint piecewise regression, and GAM, rather than being assumed a priori. The reported values will be described as dominant transition points in the observed nonlinear VPD-E relationship, not as exact or universal ecological limits. We will also revise the wording in Section 4.1 and the Discussion to avoid switching between "climate zones," "temperate zones," and "arid regions" without a consistent definition. These changes will make the threshold analysis internally consistent and will prevent overinterpretation of the nonlinear response results.

Comment 8: Piecewise regression and GAM are major components of the study, yet their results are mainly presented and interpreted in the Discussion. The Results section should include a clear subsection on nonlinear VPD thresholds, including model fits, threshold estimates, uncertainty ranges, and differences between methods. The Discussion should then focus on the ecohydrological interpretation of these thresholds.

Response: We agree that the nonlinear threshold analysis is an important part of the study and should be presented more clearly in the Results section. In the previous version, the piecewise regression and GAM results were described mainly in the Discussion, which made it difficult for readers to distinguish the statistical results from their ecohydrological interpretation. In the revision, we will restructure this part of the manuscript.

Specifically, we will add a new subsection in the Results section focusing on nonlinear VPD thresholds. This subsection will report the fitted VPD–E relationships for different aridity zones, the threshold estimates, and the comparison among one-breakpoint piecewise regression, two-breakpoint piecewise regression, and GAM. We will also include model performance information, such as R^2, RMSE, AICc or ΔAICc where applicable, to show why the selected threshold model is used as the main result. The revised Results section will clearly report the threshold estimates for each aridity zone, including the arid, semi-arid, semi-humid, and humid zones. We will also add uncertainty information for the threshold estimates, such as confidence intervals or ranges derived from model fitting and sensitivity tests, depending on the final model output. We will then revise the Discussion so that it no longer serves as the main place for presenting the threshold results. Instead, the Discussion will focus on the ecohydrological meaning of these thresholds. For example, we will discuss why higher thresholds occur in arid and semi-arid zones, how water limitation may weaken the positive response of E to VPD under high atmospheric demand, and why humid regions show lower transition values. We will also emphasize that these thresholds should be interpreted as dominant transition points in the observed nonlinear VPD-E relationship, rather than exact or universal ecological limits. This revision will make the manuscript structure clearer and will separate statistical reporting from process-based interpretation.

Comment 9: The SEM analysis needs clearer reasoning and explanation. As I understand it, the model suggests that changes in VPD can explain 66% of the variation in LAI. I am skeptical that this reflects a direct effect of VPD on LAI; it may instead reflect spatial climate gradients or seasonal changes. High VPD does not necessarily promote leaf area development; it usually imposes atmospheric water stress. This pathway may simply indicate that warmer, more productive regions have both higher VPD and higher LAI. The authors should test the SEM using deseasonalized anomalies, climate-zone-specific models, and additional control variables such as radiation, soil moisture, vegetation type, and growing-season length. Also, what do the red lines in Figure 5 represent?

Response: We agree that a strong VPD-LAI pathway should not be interpreted simply as evidence that high VPD directly promotes leaf area development. High VPD usually represents atmospheric water stress and can suppress stomatal conductance and vegetation activity, especially under limited soil water availability. Therefore, the positive VPD-LAI pathway in the current SEM may partly reflect shared spatial climate gradients, seasonal covariation, or the fact that warmer and more productive regions tend to have both higher VPD and higher LAI. In the revision, we will avoid interpreting this pathway as a direct physiological effect of VPD on LAI.

To address this concern, we will revise both the SEM analysis and its explanation. First, we will test the SEM using deseasonalized anomalies to reduce the influence of seasonal cycles. Second, we will conduct climate-zone-specific SEM analyses to examine whether the VPD-LAI relationship is consistent across arid, semi-arid, semi-humid, and humid regions, rather than being driven by broad spatial climate gradients. Third, we will include additional control variables where data availability allows, including radiation, soil moisture, vegetation type, and growing-season length. These tests will help determine whether the VPD-LAI pathway remains robust after accounting for climatic seasonality, vegetation background, and water-energy constraints.

We will also strengthen the mechanistic discussion by reviewing relevant studies on the effects of VPD on vegetation growth, canopy development, stomatal regulation, and plant water stress. If the revised SEM shows that the VPD-LAI pathway weakens substantially after deseasonalization or within climate zones, we will explicitly state that the original pathway was likely influenced by covariation rather than a direct VPD effect. If the pathway remains significant in some regions, we will still interpret it cautiously as a statistical association under specific environmental conditions, not as general evidence that high VPD promotes vegetation growth. In addition, we will revise Figure 5 and its caption to make the path notation clear. We will explicitly explain what the red lines represent, including whether they indicate positive standardized path coefficients, negative path coefficients, or statistically significant pathways, depending on the final graphical convention. We will also add the standardized coefficients, significance levels, and model-fit statistics more clearly in the figure or caption. These revisions will make the SEM more transparent and will prevent readers from overinterpreting the VPD-LAI pathway as a direct causal mechanism.

Additional planned revisions

In addition to the revisions made in response to the two reviewers' comments, we also plan to make several additional changes to improve the rigor and clarity of the manuscript. First, we will remove the analysis of canopy interception from Section 3.3. The reason is that the mechanisms linking VPD to transpiration and soil evaporation are relatively well established, whereas the influence of VPD on canopy interception is less direct and more difficult to interpret mechanistically. Canopy interception is mainly controlled by canopy structure, commonly represented by LAI, and by precipitation amount and intensity. Therefore, a statistical relationship between VPD and interception may not necessarily indicate a causal mechanism. To avoid overinterpretation, we will focus Section 3.3 on transpiration and soil evaporation, for which the physical and ecohydrological interpretations are more robust. Second, we will detrend the data used in Section 3.3 before conducting the correlation analysis, so that the results better reflect interannual covariation rather than shared long-term trends. We will also add significance markers in Figure 3 to indicate correlations significant at $P < 0.05$. Third, we will add the official citation for ERA5-Land to improve the completeness of the data-source description. Finally, we will add supplementary maps showing the spatial distributions of land-cover types and climate zones, which provide necessary background information for interpreting the spatial heterogeneity of the VPD–E relationship.