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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 #1

We sincerely thank the reviewer for carefully reading our manuscript and for providing constructive and detailed comments. We fully recognize that several aspects of the manuscript require further clarification, particularly the consistency of the nonlinear threshold values, the methodological assumptions of the moving-window approach, the unit and interpretation of dE/dVPD, the description of interannual VPD variability, the scale and interpretation of the SEM analysis, and the uncertainty associated with soil moisture and evapotranspiration products. In the responses below, we provide a point-by-point plan for how we will revise the manuscript according to the reviewer's suggestions. We will correct the inconsistent terminology and units, clarify the spatial scale and assumptions of the analysis, revise unsupported or over-causal interpretations, and strengthen the uncertainty discussion. We sincerely appreciate the reviewer's comments and respectfully hope to be given the opportunity to revise the manuscript and incorporate these suggested modifications.

Comment 1: Lines 27–29: The climatic-gradient thresholds are reported inconsistently. In the Abstract, the arid-zone threshold is given as 1.31 kPa, whereas in Section 4.1 and the Conclusion (Line 468) it is given as 1.67–1.68 kPa. The same discrepancy applies to the temperate zone (0.68 kPa in the Abstract; 1.67–1.68 kPa referred to in Section 4.3, Line 407). The authors must reconcile these values, clarify whether the Abstract reports the GAM-derived or piecewise-derived threshold, and ensure consistency throughout.

Response: Thank you for pointing out this inconsistency. We agree that the threshold values were not reported in a clear and consistent way in the original manuscript. In the revision, we will use one clear aridity-based framework throughout the manuscript. We will divide global land areas into four aridity zones: arid, semi-arid, semi-humid, and humid regions. We will no longer mix different climate grouping methods when reporting threshold values. This change will make the threshold comparison clearer. We will also clarify how the threshold values are obtained. We will not assume in advance that the one-breakpoint piecewise model is the only suitable model. Instead, we will compare three models: the one-breakpoint piecewise model, the two-breakpoint piecewise model, and the GAM. We will use model performance metrics, including model fit and error, to judge which model better describes the VPD–E relationship in each aridity zone. The reported thresholds will then be based on the model that gives the most suitable description of the nonlinear response. Under this revised framework, we will report one consistent set of threshold values throughout the Abstract, Results, Discussion, and Conclusion. The threshold values will be 1.90 kPa for the arid zone, 1.46 kPa for the semi-arid zone, 0.49 kPa for the semi-humid zone, and 0.47 kPa for the humid zone. We will remove the earlier inconsistent values, including 1.31 kPa, 0.68 kPa, and 1.67–1.68 kPa, from the relevant parts of the manuscript. We will also revise the wording around these values. We will describe them as estimates of the main transition in the observed VPD–E relationship. We will avoid presenting them as exact ecological limits. This wording is more appropriate because the threshold estimates depend on the model structure and the input data.

These changes will make the threshold values and their methodological basis consistent across the Abstract, Methods, Results, Discussion, and Conclusion.

Comment 2: Section 2.2.2: The "space-for-time" moving-window approach to compute dE/dVPD is borrowed from land-use-change literature, where the logic is well-established for discrete cover transitions. Applying it here to derive VPD sensitivity assumes that spatial VPD gradients at 5 × 5 km are driven solely by biophysical feedbacks, rather than by mesoscale atmospheric dynamics. Please provide justification for this assumption for the global domain, especially in complex terrain or coastline-adjacent pixels. Furthermore, the authors should discuss the robustness of this assumption and, if possible, offer a cross-validation against pixel-level temporal regression (dE/dVPD from time series at each pixel).

Response: We agree that the space-for-time moving-window approach needs a clearer methodological explanation. We also agree that this method should not be interpreted as direct causal evidence.

In the revision, we will first clarify the spatial scale of this method. The analysis will be conducted on a common 0.1° grid. The moving window will be defined as a 5 × 5 grid-cell neighborhood around each target pixel, rather than a 5 × 5 km window. This description will avoid confusion about the spatial resolution of the input datasets.

We will also add local screening rules within the moving 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%. Candidate pixels with an elevation difference greater than 100 m from the target pixel will also be excluded. These rules will reduce the influence of strong land-cover differences and topographic gradients.

We agree that the moving-window approach cannot fully remove the influence of mesoscale atmospheric processes. Local VPD gradients may still be affected by topography, air movement, and land-sea contrast. This issue may be more important in mountainous and coastal regions. We will add this limitation to the manuscript and will state that the estimated dE/dVPD should be interpreted as an apparent local sensitivity, not as direct causal evidence.

Given the limitations of the current final-response stage, we will not overstate the validation here. In the revision, we will either provide a supplementary temporal-regression comparison where feasible, or explicitly describe the absence of such a comparison as a limitation and a priority for future testing. In addition, we will support the reliability of the input data by comparing the main ERA5-Land VPD and GLEAM E datasets with independent VPD and E products. This dataset-level comparison will help assess whether the main results depend strongly on a single data source.

These changes will make the method clearer and will make the interpretation more cautious.

Comment 3: Line 164: The moving-window size is stated as "5 × 5 km based on previous studies," but no citation is provided, and the dataset native spatial resolutions (ERA5 ~31 km, GLEAM ~25 km, MODIS MCD12C1 ~5.5 km) are substantially coarser than 5 km. This creates a fundamental mismatch: in practice, a 5 × 5 km window over ERA5 or GLEAM data contains a single pixel. The authors need to explicitly state the actual grid resolution at which the analysis was conducted and reconcile it with the stated window size.

Response: Thank you for pointing out this problem. We agree that the phrase "5 × 5 km" was misleading. This wording does not match the spatial resolution of the datasets used in this study.

In the revision, we will correct this description. We will state that the analysis is conducted on a common 0.1° grid. This grid is used for both the ERA5-Land VPD data and the GLEAM E data in our analysis. Therefore, the moving window is not a 5 × 5 km window. It is a 5 × 5 grid-cell window. This means that each target pixel is compared with candidate pixels within a 0.5° × 0.5° neighborhood. We will describe this clearly in Section 2.2.2. This correction will remove the mismatch between the stated window size and the actual data resolution.

We will also explain how candidate pixels are selected within this window. To reduce the effect of surface heterogeneity, we will keep only neighboring pixels with the same dominant MODIS land-cover type as the target pixel. We will require the difference in dominant land-cover fraction to be less than 10%. We will also remove pixels with an elevation difference greater than 100 m from the target pixel.

These changes will make the spatial resolution, moving-window size, and sample selection rules clearer. They will also make the method consistent with the actual resolution of the input datasets.

Comment 4: Line 270: The global mean sensitivity of 293.27 ± 62.28 mm·hPa^{-1}·yr^{-1} is a central quantitative result, yet the units are unconventional. Sensitivity (dE/dVPD) is dimensionally [mm / hPa], not [mm·hPa^{-1}·yr^{-1}]. The inclusion of yr^{-1} suggests this may be derived from annual trend magnitudes rather than a pure partial derivative. Unless I have misunderstood the definition of what quantity this represents, e.g., is it a sensitivity coefficient, a trend-normalised ratio, or a regression slope?

Response: Thank you for this careful comment. We agree that the original unit of dE/dVPD was incorrect. In the revision, we will clarify that dE/dVPD is a sensitivity coefficient. It describes the change in E per unit change in VPD. Its correct unit is mm hPa^{-1}. It is not an annual trend rate, and it is not a trend-normalized ratio. We will therefore remove yr^{-1} from this unit throughout the manuscript. We will also clarify how this sensitivity is estimated. In this study, dE/dVPD is calculated as the slope between E differences and VPD differences within the moving-window framework. The Theil–Sen slope estimator is used to reduce the influence of outliers. We will report the global mean sensitivity as 293.27 ± 62.28 mm hPa^{-1}, rather than 293.27 ± 62.28 mm hPa^{-1} yr^{-1}. We will also revise the related text in the Results, figure captions, and Conclusion to avoid confusion between sensitivity and temporal trend.

Comment 5: Lines 296-303: The statement that "VPD rose abruptly in 1998 (0.034 hPa yr^{-1})" reports a trend rate, not an abrupt change, and the phrasing conflates trend magnitude with event-driven anomaly. Additionally, Figure 3b shows VPD peaking at 7.839 hPa in 2010, the connection between the 1998 El Niño and the 2010 peak is not straightforward and warrants more rigorous treatment, potentially with ENSO-index partial correlation.

Response: Thank you for this careful and constructive comment. We agree with the reviewer that our original wording was not rigorous enough. The phrase "VPD rose abruptly in 1998 (0.034 hPa yr^{-1})" was inappropriate, because 0.034 hPa yr^{-1} is a trend rate rather than the magnitude of an abrupt event. This wording may cause confusion between a long-term trend and a short-term climate anomaly. We also agree that the VPD peak in 2010 should not be directly linked to the 1998 El Niño without a dedicated attribution analysis. Such a link would require additional tests, such as an ENSO-index partial correlation or other attribution methods. These analyses are beyond the current scope of this manuscript. Therefore, we will remove the statement about the abrupt increase in 1998 and will avoid linking the 2010 VPD peak to the 1998 El Niño.

In the revision, we will rewrite the relevant part of Section 3.3 in a more cautious way. We will only describe the observed long-term trend and interannual variability of VPD and E during 1981-2020. We will not make an event-based explanation unless it is directly supported by the analysis. We will also adjust the focus of this section. Instead of discussing ENSO-related interpretation, this section will focus on the relationships between VPD and the two E components, namely E_t and E_b. We will use partial correlation analysis to examine the relationships between VPD and E_t/E_b after controlling for precipitation and soil moisture. This change will make the interpretation more consistent with the evidence shown in Fig. 3. It will also reduce the risk of overinterpreting the time-series patterns. We sincerely appreciate this comment, because it helps us separate trend description from event attribution and makes the discussion more reliable.

Comment 6: Lines 362-364: The SEM model is stated to explain 77% of the variance in E, and this is presented as a single global metric. However, SEM was presumably fitted globally on spatially aggregated, or grid-cell mean data. The authors should clarify the unit of analysis (pixel-level, regional mean, or annual global mean) and include model fit statistics (Fisher's C, AIC) in the main text rather than only referencing Figure 5.

Response: We agree that the original description of the SEM analysis was not clear enough. The statement that the SEM explained 77% of the variance in E may be misunderstood as a pixel-level or spatially explicit global result. This is not what we intended. In the revision, we will clearly state that the SEM is fitted using annual global-mean variables. The unit of analysis is therefore the annual global mean, not individual pixels or regional averages. We will also revise the wording around R^2 = 0.77. We will explain that this value refers only to the SEM fitted at the annual global-mean scale.

We will also add the main model fit statistics directly to the main text. We agree that these statistics should not only appear in Figure 5. In Section 4.2, we will report Fisher's C, P , and AIC. We will also explain that these values are used to evaluate the overall fit of the SEM. This change will make the SEM description more transparent. It will also help readers understand the scale, meaning, and limits of the reported 77% explained variance. We sincerely appreciate this comment, because it helps us avoid overstating the SEM result and makes the interpretation more precise.

Comment 7: Lines 375-378: The finding that LAI increases markedly under high VPD (standardised coefficient = 0.90) but has only a weak direct effect on Et (standardised coefficient = 0.07) is counterintuitive and deserves deeper mechanistic discussion. A strong VPD–LAI relationship with near-zero LAI–Et effect suggests that either collinearity in the SEM is absorbing the LAI pathway into the VPD direct effect, or that the LAI variable used (GIMMS V1.2 monthly) does not resolve the sub-seasonal dynamics at which VPD–stomatal coupling operates. Please, make sure to address this explicitly.

Response: The combination of a strong VPD–LAI path and a weak direct LAI–E path may look counterintuitive. We will revise Section 4.2 to explain this point more carefully. We will make clear that the weak direct LAI effect does not mean that vegetation is unimportant for E. Instead, it may show that the vegetation signal is partly shared with VPD and SM in the SEM. In this case, part of the LAI-related effect may be absorbed by the direct VPD pathway or by the SM-related pathway. We will therefore describe this result as a statistical pathway under multi-factor coupling, not as direct evidence that high VPD promotes vegetation growth.

We will also add a clear limitation about the LAI data and the temporal scale of the analysis. The SEM uses annual global-mean variables, while the VPD–stomatal response can occur at much shorter time scales. The GIMMS LAI data may not capture these short-term vegetation and stomatal changes. This mismatch may weaken the direct LAI–E pathway in the SEM. In the revised text, we will state that VPD regulates E mainly through atmospheric moisture demand and soil moisture limitation at the analysis scale used here. We will also state that the LAI pathway should be interpreted with caution because of possible collinearity and scale mismatch. We sincerely appreciate this comment, because it helps us avoid an over-strong mechanistic interpretation of the SEM result.

Comment 8: Lines 406-410: In the discussion of the SM–atmospheric water vapor feedback under high VPD in arid regions, the mechanism is described qualitatively without connecting it to the quantitative results established earlier (e.g., the SEM path coefficients or the dE/dVPD spatial distribution). The discussion would be very interesting if the authors could map the regions where VPD already exceeds the 1.67-1.68 kPa threshold (as identified in Section 4.1) onto the negative-sensitivity regions shown in Figure 2a to demonstrate that the feedback mechanism is already active.

Response: We agree that the previous discussion was too qualitative. The link between the proposed soil moisture–atmospheric water vapor feedback and our quantitative results was not clear enough. In the revision, we will rewrite this part of

Section 4.3. We will connect the discussion more directly to the dE/dVPD spatial pattern, the aridity-zone thresholds, and the SEM path coefficients. In particular, we will discuss the strong negative VPD–SM pathway in the SEM and the negative dE/dVPD regions shown in Fig. 2a. This change will help show how high atmospheric dryness may weaken the positive response of E to VPD when water supply becomes limiting. We will also adjust the threshold discussion to match the revised threshold framework. The manuscript will use aridity-index-based zones rather than the earlier threshold values mentioned in the original text. We will therefore discuss the arid and semi-arid thresholds as 1.90 kPa and 1.46 kPa, respectively. If the revised analysis confirms a clear spatial overlap, we will add an overlay analysis between areas exceeding the relevant VPD threshold and areas with negative dE/dVPD. This analysis will be used to test whether high-threshold regions are already linked with suppressed E under rising VPD. If the overlap is not strong or not spatially robust, we will not overstate this mechanism. Instead, we will describe it as a plausible feedback pathway supported by the SEM and spatial sensitivity results. This revision will make the discussion more quantitative and more cautious.

Comment 9: Lines 431-436: The acknowledgement that ERA5 overestimates SM in arid regions (Kokkalis et al., 2024) is important, but its consequence is underplayed. For example, given that the SEM assigns the VPD–SM path a standardized coefficient of -0.84, the strongest single path in the entire model, a systematic positive bias in SM would directly attenuate this coefficient, inflating the apparent direct effect of VPD on E. This has a directionally predictable effect on the ms's central conclusions and should be discussed with explicit reference to the SEM coefficients.

Response: We agree that the implication of ERA5-Land soil moisture uncertainty was underdeveloped in the original manuscript. This issue is especially relevant to our SEM interpretation, because the VPD–SM pathway has the strongest standardized coefficient in the model (−0.84). In the revision, we will explicitly discuss this point rather than only mentioning ERA5-Land SM bias in general terms.

We will clarify that possible overestimation of SM in arid regions may affect the estimated balance between the direct and indirect effects of VPD on E. If the SM product overestimates moisture availability, or if it weakens the representation of dry anomalies and soil moisture variability, the negative VPD–SM coupling may be underestimated. In that case, part of the SM-mediated indirect effect of VPD on E could be shifted into the apparent direct VPD–E pathway. This would make the direct effect of VPD appear stronger than it would be if soil moisture stress were fully captured. We will therefore state that the SEM result should be interpreted as a statistical pathway based on the available datasets, and that the relative magnitudes of the direct VPD effect and the SM-mediated indirect effect remain sensitive to SM uncertainty.

We will also strengthen the uncertainty discussion for E and its components. Current global ET products differ in long-term trends and in the partitioning of E into transpiration, soil evaporation, and interception evaporation. These differences may influence both the spatial dE/dVPD pattern and the SEM path coefficients. We will make clear that these uncertainties do not remove the evidence that VPD is closely linked to global E variability, but they do affect how confidently we can separate the direct atmospheric-demand pathway from the indirect soil-moisture pathway. We sincerely appreciate this comment, because it helps us present the SEM results with more appropriate caution and avoids overstating the central mechanism.

Comment 10: Lines 444-449: The authors acknowledge that piecewise regression assumes a single breakpoint. Yet in the Conclusion (Lines 466-469), a full table of single-threshold values per climate zone is presented as a primary finding. The authors should either present evidence that the VPD–E response in each zone is indeed well described by a single breakpoint (e.g., by showing that a two-breakpoint model does not significantly improve fit) or explicitly qualify the reported thresholds as approximations of the dominant transition, avoiding overstatement of their precision.

Response: We agree that the threshold values should not be presented as overly precise ecological limits. The original wording may have given the impression that the VPD–E response was assumed to have only one breakpoint in each zone. This was not our intention. In the revision, we will clarify that the thresholds are identified after comparing one-breakpoint piecewise regression, two-breakpoint piecewise regression, and GAM. The purpose of this comparison is to evaluate whether a dominant transition point can adequately describe the observed nonlinear VPD–E response, rather than to impose a single-breakpoint structure a priori.

We will revise Section 4.1 and the Conclusion to make this point explicit. We will state that the reported thresholds represent the dominant transition points supported by the model comparison, not exact or unique ecological boundaries. We will also explain that the two-breakpoint model does not provide a clear improvement over the one-breakpoint model based on the fit comparison, while the GAM results show broadly consistent transition behavior. The results from the GAM and two-breakpoint models will be presented as supplementary evidence to support the robustness of the main threshold interpretation. At the same time, we will soften the wording in the Conclusion and avoid overstating the precision of the threshold values. We sincerely appreciate this comment, because it helps us present the nonlinear threshold results in a more transparent and cautious way.

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.