The manuscript develops a physically intuitive framework linking absorbed solar radiation, evaporative limitation, lower-atmospheric heating, diurnal temperature range (DTR), and daytime vapor pressure deficit (VPD). In particular, the relationship $\Delta VPD \approx s \times DTR$ provides a useful bridge between surface-energy partitioning and atmospheric dryness. The analytical framework is elegant, the global results are encouraging, and the thermodynamic interpretation of the hump-shaped evaporation–VPD relationship is interesting. However, several central claims appear to extend beyond what is directly demonstrated by the current analyses, as detailed below. I believe that addressing the points below would improve the clarity and strength of the manuscript.

Main suggestions

1. The high agreement between estimated and observed daily maximum VPD is promising. However, the degree of independence between the estimate and the observation-based reference requires closer examination.

Observed daily maximum VPD is calculated from air temperature and dew-point temperature, whereas the estimated value is constructed as:

$$VPD_{max, estimated} = VPD_{min, observed} + \Delta VPD_{estimated},$$

where VPD_{min} is itself derived from observed minimum temperature and dew-point data. Moreover, the slope of the saturation vapor pressure curve, s, is highly temperature dependent. Consequently, part of the agreement between estimated and observed VPD_{max} may arise from the observed VPD_{min} term and other shared temperature information, rather than from the analytical DTR-based estimate alone.

To clarify the origin of the reported agreement, I recommend a component-wise evaluation of the framework. For example, the authors could directly compare estimated ΔVPD with observed $\Delta VPD = VPD_{max} - VPD_{min}$, and then compare the performance of three progressively more complete estimates: (i) VPD_{min} alone, (ii) VPD_{min} plus the radiation-driven ΔVPD component, and (iii) the full estimate including the water-stress contribution. This would help quantify the incremental explanatory skill contributed by each component. At minimum, I suggest clarifying that the framework currently functions primarily as a diagnostic decomposition unless its ability to estimate VPD from fully independent inputs is demonstrated.

2. Several simplifying assumptions are central to the proposed framework and would benefit from quantitative evaluation.

First, the manuscript approximates VPD as

$$\mathrm{VPD} \approx s \times (\mathrm{Tair} - \mathrm{Tdew}),$$

but it is unclear at which temperature the slope s = desat/dT is evaluated. Because the saturation vapor pressure curve is nonlinear, this approximation may introduce appreciable errors when Tmax and Tdew differ substantially, particularly in hot and dry environments. I suggest comparing the linearized estimate with the exact expression esat(Tmax) − esat(Tdew), especially across aridity classes and for the upper tail of the VPD distribution. It would also be useful to assess whether this approximation materially affects the estimated spatial patterns or component contributions.

Second, the analytical formulation assumes a globally constant daytime boundary-layer height of 1000 m. Because this parameter determines the effective heat capacity of the lower atmosphere, it may strongly influence the estimated DTR and VPD. I recommend a sensitivity analysis using several plausible boundary-layer heights. An additional evaluation using reanalysis-based or observationally constrained boundary-layer-height estimates would be valuable. At minimum, the manuscript could discuss how uncertainty in boundary-layer height and other major inputs affects the estimated DTR and VPD.

3. The water-availability factor, fw, is derived from actual evaporation, which is then used to estimate DTR and VPD. The same evaporation data are subsequently plotted against estimated VPD to interpret the hump-shaped evaporation–VPD relationship. This creates structural non-independence because evaporation enters both the construction of the predictor and the subsequent evaluation. Consequently, the relationship involving estimated VPD may partly arise from the model formulation itself.

I recommend testing this interpretation using an independently derived water-stress indicator or, alternatively, using separate datasets to estimate fw and evaluate evaporation. The current results support the conclusion that lower-atmospheric heating can contribute to the hump-shaped evaporation–VPD relationship, but they do not yet demonstrate that the observed relationship can be explained solely by this mechanism.

4. In Figure 4, the radiation contribution is calculated by setting fw = 1, while the water-stress contribution is defined as the difference between the actual and unstressed cases. This diagnostic decomposition is useful, but the estimated contributions depend on the chosen reference state and may include interactions between radiation and water stress.

I suggest clarifying this limitation and testing whether the relative contributions remain robust under alternative plausible baselines. If the terms are intended to represent uniquely attributable contributions, an interaction term or a symmetric attribution method could also be considered.

5. The manuscript would benefit from more clearly positioning its contribution relative

to recent work on sub-diurnal temperature variability, DTR, VPD, and ecosystem productivity. In our recent study, Zhong et al. (2025b) demonstrated asymmetric effects of daily maximum and minimum temperatures on saturation vapor pressure, actual vapor pressure, and long-term VPD changes. Zhong et al. (2025a) further linked recent DTR changes to contrasting vegetation-productivity responses in humid and arid zones, partly through the DTR–VPD pathway.

These studies overlap directly with the manuscript's discussion of Tmax, Tmin, DTR, VPD, soil-water limitation, and ecosystem responses. I therefore suggest distinguishing more clearly between previously documented empirical relationships and the specific advance provided by the present framework. In my view, the principal novelty of the manuscript lies in analytically deriving DTR from surface-energy balance and lower-atmospheric heat storage, linking radiation and evaporative limitation to daytime VPD changes, decomposing VPD into radiative, water-stress, and background-moisture components, and proposing a thermodynamic interpretation of the hump-shaped evaporation–VPD relationship. Clarifying these distinctions would strengthen the manuscript's novelty and contextualization.

References

Zhong, Z., Chen, H. W., He, B., and Su, B. (2025a). Contrasting vegetation productivity responses in arid and humid zones to recent changes in diurnal temperature range. *The Innovation Geoscience*, 3, 100163. https://doi.org/10.59717/j.xinn-geo.2025.100163

Zhong, Z., Chen, H. W., Dai, A., Zhou, T., He, B., and Su, B. (2025b). Sub-diurnal asymmetric warming has amplified atmospheric dryness since the 1980s. *Nature Communications*, 16, 8247. https://doi.org/10.1038/s41467-025-63672-z

Minor suggestions

Lines 35–40: "antecedent land-surface and, radiative conditions" should be "antecedent land-surface and radiative conditions."

Lines 75–80: "The DTR we then relate to solar radiation…" would read more clearly as "We then relate DTR to solar radiation…"

Line 113: The stated unit of specific heat capacity should be J kg−1 K−1 rather than J kg−1 m−3.

Lines 120–125: DTR would be more accurately described as being calculated from Tmax and Tmin rather than as a third temperature variable obtained directly from the CPC dataset.

Line 140: The text refers to the DTR expression as Eq. (8), whereas the main DTR expression appears to be Eq. (7).

Line 150: The statement that the RMSE "for each grid point remains less than 0.5 kPa across all data points" is difficult to interpret. Please clarify whether this refers to spatial errors, temporal errors, or the distribution of grid-cell RMSE values.

Line 155: In the Figure 2 caption, "observations" should be changed to "observed."

Lines 155–160: The references to "Figure 1D, E" and "Figure 1F" appear to refer to Figure 2D, E and Figure 2F, respectively.

Lines 165–170: "in shaping both, the DTR as well as the VPD" would read more clearly as "in shaping both DTR and VPD."

Lines 260–265: The Figure 5 caption does not appear to match panels A–C correctly and should be revised.

Lines 275–280: "recent work that questioned … and emphasizing…" should be revised to "recent work that questioned … and emphasized…"

Lines 293–296: "We used an analytical expression … and demonstrate…" should be revised for tense consistency, for example, "We used … and demonstrated…"

It would also be useful to present model performance separately for major climate zones or aridity classification, for example humid, semi-arid, and arid zones.

The reference list contains several punctuation, journal-title, and formatting inconsistencies that should be corrected. For example, Reference 17 includes an unmatched closing parenthesis in "Nat Commun 13, 989)." This should be removed, and the journal title, volume, and article-number format should be standardized. In Reference 26, the journal title "Hydrology and Earth System Science" should be corrected to the official title, "Hydrology and Earth System Sciences." I suggest carefully checking the entire reference list for similar inconsistencies.