

RC1: 'Comment on egusphere-2025-2560', Anonymous Referee #1, 06 Aug 2025

This is a valuable paper that enhances evapotranspiration estimates by incorporating CO₂ physiological feedback and CMIP6 scenarios, which is of great significance for understanding hydrological processes under climate change. The study contributes to improving the accuracy of future ET projections and provides insights for related research.

The data and methodology are generally sound, and the manuscript is well-structured and comprehensive. This study extends previous research by updating the Penman-Monteith model based on CMIP6 data, and it fits within the scope of relevant journals, which will be of interest to readers.

I recommend this work for publication after some minor revisions.

Reply: Thank you for your professional comments. Based on your suggestions, we will make the appropriate revisions in the new version of the manuscript.

Comments

1. In the Abstract, the phrase "reduces the deviation in projected ET trends by approximately 15–20%" should be revised to "reduces the deviation in projected ET trends by approximately 15–20% compared to CMIP5-based frameworks" for improved clarity.

Reply: Thanks. We will amend the sentence to the required wording in the new version.

2. The keywords are relevant, but it is suggested to add "Penman-Monteith model" to better reflect the methodological core.

Reply: Thank you for the suggestion. We will add "Penman-Monteith model" to the Keywords in the new version.

3. In the Introduction, when describing "Yang et al. (2019) model", it is recommended to supplement a brief note on its key parameter (e.g., original coefficient 2.4×10^{-4}) to lay a clearer foundation for the subsequent update to 1.9×10^{-4} .

Reply: Thanks. We will supplement a note on the key parameter of the "Yang et al. (2019) model" (original coefficient 2.4×10^{-4}) in the new version.

4. In Section 2.1, when mentioning "resampled to a uniform spatial resolution of $0.25^\circ \times 0.25^\circ$ ", it is suggested to specify the resampling method (e.g., bilinear interpolation) for reproducibility.

Reply: Thank you for the suggestion. We will specify the resampling method in the new version.

5. In Section 2.2, the criteria for "non-water-limited regions" (e.g., "ET/P ratio < 2.0") are clear, but it is recommended to add a brief explanation of why this threshold is chosen (e.g., based on previous studies) to improve rigor.

Reply: Thank you for your professional comments. Based on your suggestions, we will make the appropriate revisions in the new version.

6. The formulas in Sections 2.3.2–2.3.4 use symbols like "s", " γ ", and "u". It is suggested to add a unified list of symbols at the end of the paper or in an appendix to avoid confusion for readers.

Reply: Thanks for the suggestion. We will add Table 1 (a unified list of symbols) in the new version; please refer to the revised draft for details.

7. Figure 1 (Taylor diagrams) lacks explicit units for variables (e.g., relative humidity in %). It is recommended to add units in the figure caption to enhance clarity.

Reply: Thanks for the suggestion. We will modify Figure 1 by adding units (e.g., relative humidity in %) in the figure caption to enhance readability in the new version.

8. In Section 3.2, "Ep intensification rates vary by scenario: under SSP5-8.5, the decadal increase is approximately 2.1%" – it is suggested to specify the reference period (e.g., relative to 2015–2025) for this rate.

Reply: Thanks. This refers to the average growth rate per decade, with no comparison period. To make this clearer, we will amend the sentence accordingly in the new version.

9. Figure 5 (time series decomposition) has subplots labeled (a)–(d) corresponding to SSP scenarios, but the caption does not explicitly link each subplot to the scenario (e.g., "(a) SSP1-2.6, (b) SSP2-4.5..."). It is recommended to add this correspondence to help readers.

Reply: Thank you for your suggestion. There may be some misunderstandings regarding this point. In Figure 5, there are four subplots corresponding to the four scenarios labeled (a)–(d). Since the relationship between each main subplot and its secondary subplots may not be clear enough, we will add frame lines to facilitate distinction in the new version.

10. In Section 4.1, "low-emission scenarios exhibit more dispersed distribution across models", but it is suggested to briefly cite 1–2 relevant studies (e.g., Pan et al., 2020) to support this observation and strengthen the discussion.

Reply: Thanks for the suggestion. While differences among simulations from various institutions objectively exist, the observation that "low-emission scenarios exhibit more dispersed distribution across models" is derived from the figures in this paper.

As this conclusion is solely based on the content of this study, we believe further citations for corroboration are not necessary, so no additional citations will be added in the new version.

11. The limitations section (4.2) notes that the model "assumes fixed vegetation responses to CO₂". It is suggested to add a short sentence on how future work could relax this assumption (e.g., integrating species-specific parameters) to make the prospects more concrete.

Reply: Thanks. We will amend the sentence in accordance with your comment in the new version to make the prospects more concrete.

12. In the Conclusion, "improved consistency with CMIP6 data" is suggested to be rephrased as "improved consistency with CMIP6 simulations" to align with terminology used in the main text.

Reply: Thanks for the suggestion. We will revise the sentence to "improved consistency with CMIP6 simulations" in the new version to align with the terminology used in the main text.