

Cover Letter

Dear Dr Xing Yuan:

On behalf of all the contributing authors, I would like to express our sincere appreciation of your letter and reviewers' constructive comments concerning our article entitled "Enhancing Evapotranspiration Estimates Under Climate Change: The Role of CO₂ Physiological Feedback and CMIP6 Scenarios"(EGUSPHERE-2025-2560).

I have considered the comments very carefully and have revised the paper accordingly. These comments are all valuable and helpful for improving our article. We have extensively modified our manuscript according to the editor and reviewers' comments. We hope that the corrections are satisfactory. The reviewer comments are in italic font below, and specific concerns have been numbered. Our response is given in regular font, and changes/additions to the manuscript are shown in red text. Point-by-point answers to the kind editor and two kind reviewers are listed below this letter.

Thanks again and we are looking forward to hearing from you soon.

Best wishes,

Qianfeng Wang, on behalf of all co-authors

Email: wangqianfeng@fzu.edu.cn

Dear Editor and Reviewer 1,

Thank you very much for your valuable comments and constructive suggestions on our manuscript. These insights have significantly helped improve the quality and rigor of our work. We have carefully addressed each of your comments, and the key revisions are summarized below:

1. Abstract: Revised the phrase "reduces the deviation in projected ET trends by approximately 15–20%" to "reduces the deviation in projected ET trends by approximately 15–20% compared to CMIP5-based frameworks".
2. Keywords: Added "Penman-Monteith model".
3. Introduction: Supplemented a note on the key parameter of the "Yang et al. (2019) model" (original coefficient 2.4×10^{-4}).
4. Section 2.1: Specified the resampling method as "bilinear interpolation".
5. Section 2.2: Added a brief explanation of the rationale for choosing "ET/P ratio < 2.0" as the criterion for "non-water-limited regions" (based on previous studies).
6. Sections 2.3.2–2.3.4: Added Table 1 (Variable Declaration) to provide a unified list of symbols (e.g., s , γ , u) for clarity.
7. Figure 1 (Taylor diagrams): Added units (e.g., relative humidity in %) in the figure caption to enhance readability.
8. Section 3.2: Specifying "From 2015 to 2100" and revising to "average decadal growth rate" for clarity.
9. Figure 5 (time series decomposition): Labeled subplots (a)–(d) with corresponding SSP scenarios (e.g., (a) CMIP6-ssp126) and added frame lines to improve distinction.
10. Section 4.1: Retained the observation on dispersed distribution in low-emission scenarios as it is derived from the study's own figures, without additional citations.
11. Section 4.2 (Limitations): Added details on future work to relax the "fixed vegetation responses to CO₂" assumption, such as integrating species-specific physiological parameters (e.g., dynamic correlations between photosynthetic rate and CO₂ concentration).
12. Conclusion: Revised "improved consistency with CMIP6 data" to "improved consistency with CMIP6 simulations" to align with terminology used in the main text.

We hope that these revisions can fully address your concerns. If both you and the editor agree to these revisions, we will upload a complete, point by point revision document and the revised manuscript later.

With utmost sincerity,
Qianfeng Wang, on behalf of all co-authors
Email: wangqianfeng@fzu.edu.cn

Reviewer 1 Comments

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.

Response: Thank you for your professional comments, based on your suggestions, we have made the appropriate revisions

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.

Response: Thanks, we have amended the sentence to read as follows. "The inclusion of CO₂ physiological effects reduces the deviation in projected ET trends by approximately 15 – 20% compared to CMIP5-based frameworks, accounting for the increase in stomatal resistance driven by CO₂ concentrations rising from ~284 ppm to ~935 ppm." (Line: 37-40).

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

Response: Thank you for the suggestion, we have added in the Keywords.

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} .

Response: Thanks, we have amended the sentence to read as follows. "However, PM-RC-CO₂ model proposed by Yang et al. (2019) still relies on formulas derived from CMIP5-era models, that the original coefficient (2.4×10^{-4}) may introduce inconsistencies when applied to CMIP6 scenarios." (Line: 115-117)

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.

Response: Thank you for the suggestion, we have modified the method as below. "To ensure consistency across datasets, all outputs were resampled to a uniform spatial resolution of $0.25^\circ \times 0.25^\circ$ by bilinear interpolation." (Line: 147)

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.

Response: Thank you for your professional comments, based on your suggestions, we have made the appropriate revisions (Line: 163, 174).

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.

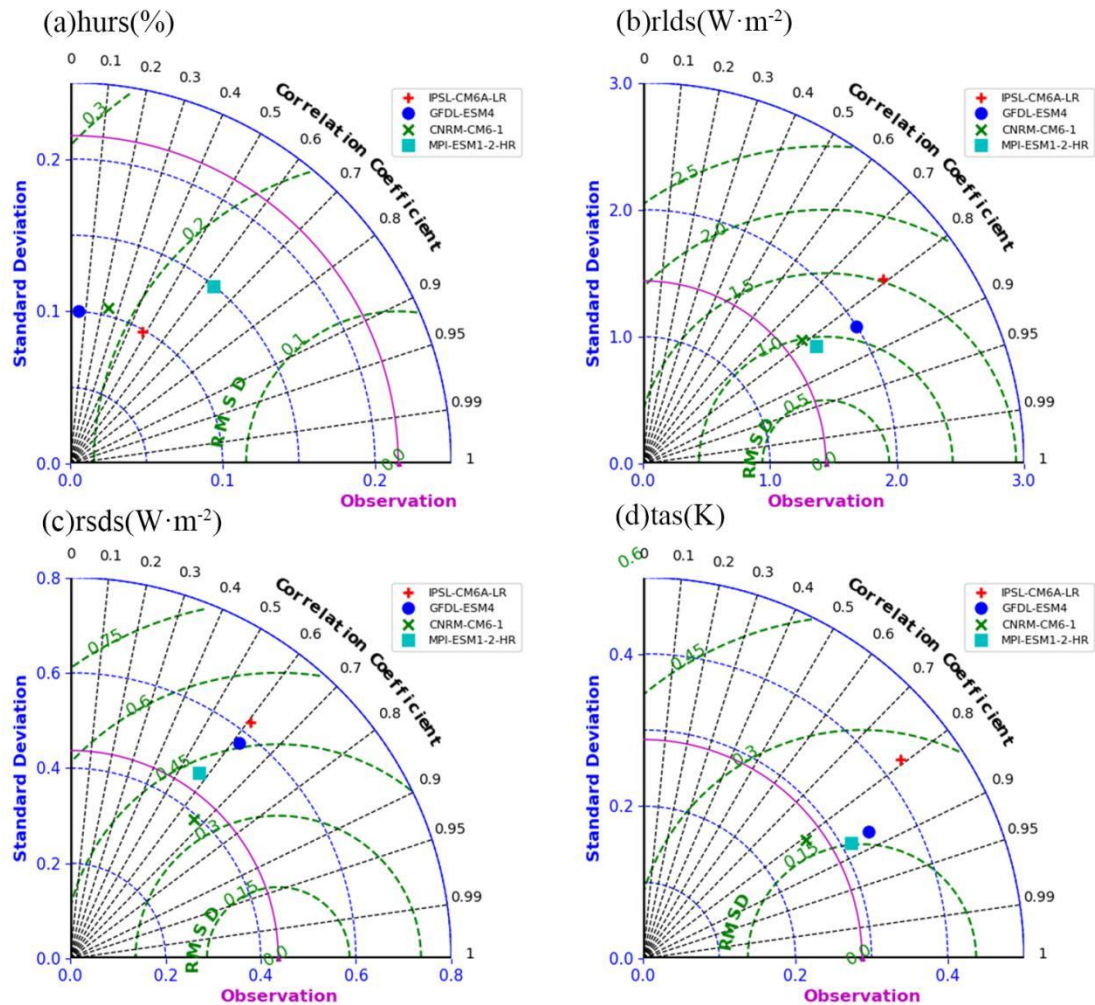
Response: Thanks for the suggestion, as below, we have added Table 1. (Line: 233).

Table 1. Variable declaration

λ	temperature-dependent latent heat of vaporization ($\text{J}\cdot\text{kg}^{-1}$)
s	gradient of the saturation vapour pressure with respect to temperature ($\text{Pa}\cdot\text{K}^{-1}$)
R_n^*	available energy ($\text{MJ}\cdot\text{m}^{-2}\cdot\text{day}^{-1}$)
ρ_a	air density ($\text{kg}\cdot\text{m}^{-3}$)
C_p	specific heat at constant pressure ($\text{J}\cdot\text{kg}^{-1}\cdot\text{K}^{-1}$)
r_s	surface stomatal resistance ($\text{s}\cdot\text{m}^{-1}$)
r_a	Aerodynamic resistance ($\text{s}\cdot\text{m}^{-1}$)
D	vapor pressure deficit (Pa)
γ	psychrometric constant ($\text{Pa}\cdot\text{K}^{-1}$)
u	wind speed ($\text{m}\cdot\text{s}^{-1}$)
T	air temperature ($^\circ\text{C}$)
α	shortwave albedo

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.

Response: Thanks for the suggestion, as below, we have modified Fig. 1 (Line: 270).



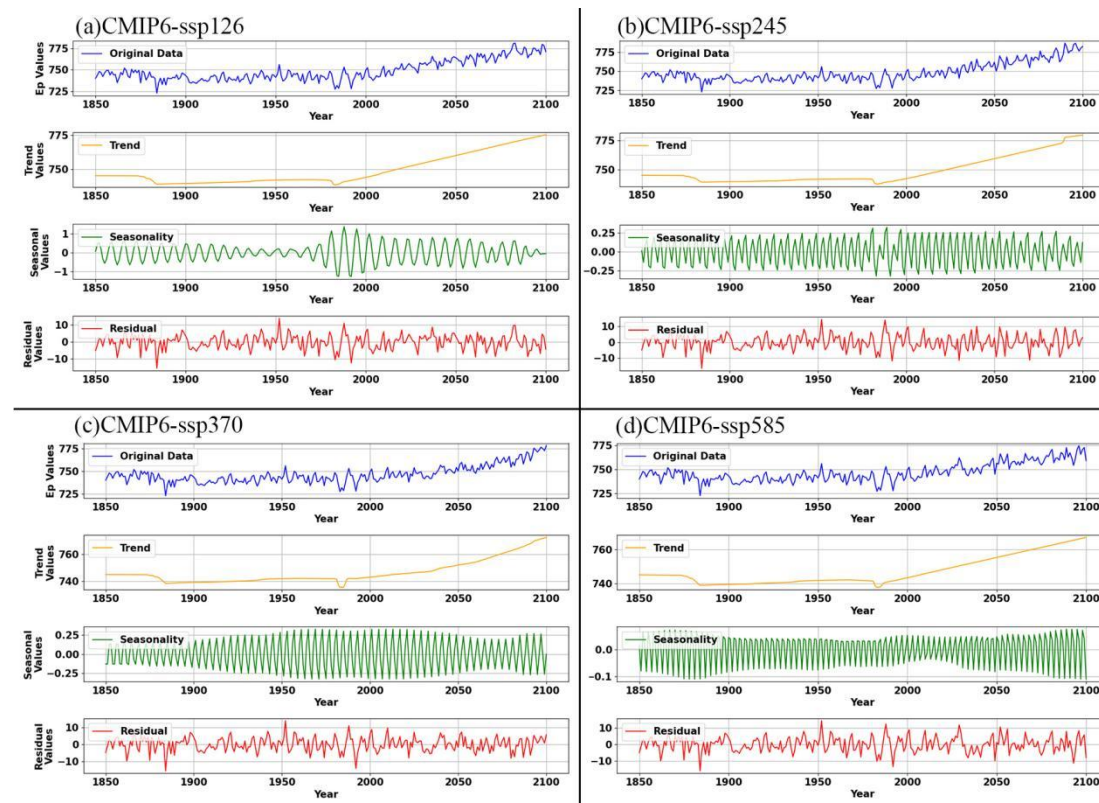
8. In Section 3.2, "Ep intensification rates vary by scenario: under SSP5-8.5, the decadal increase is approximately 2.1%" – it is suggested to specify the reference period (e.g., relative to 2015–2025) for this rate.

Response: Thanks. This should be to the average growth rate per decade, without a comparison period. To make it clearer, we have amended the sentence to read as follows. "From 2015 to 2100, the rates of Ep intensification differ across scenarios: under SSP5-8.5, the average decadal growth rate is approximately 2.1%, whereas under SSP1-2.6, it stands at 1.2%." (Line: 318-320)

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.

Response: Thank you for your suggestion. You may have some misunderstandings about it. In Figure 5, there are a total of four sub-figures, corresponding to the four scenario annotations (a)-(d). The relationship between each sub-figure and its

secondary sub-figures may not be clear enough, so we have added frame lines to facilitate distinction as below(Figure 5, Line: 324)



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.

Response: Thanks for the suggestion. The differences among simulations from various institutions objectively exist, but the observation that they are relatively scattered is a result derived from the figures in this paper. Since it is solely based on the content of this paper, further citations for corroboration are not necessary.

11. The limitations section (4.2) notes that the model "assumes fixed vegetation responses to CO_2 ". 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.

Response: Thanks, we have amended the sentence to read as follows. "Given the high specificity of vegetation types at regional scales, fixed-response models struggle to capture physiological variations among species. For finer-scale assessments, model accuracy can be enhanced by integrating detailed representations of vegetation physiological responses, such as dynamic correlation data between photosynthetic rate and CO_2 concentration (Luo et al., 2018a; Luo et al., 2018b)."(Line: 511-516).

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.

Response: Thanks for the suggestion, we've revised the sentence as below. "The updated PM-ET model reduces CO₂-induced evapotranspiration (ET) bias by 15-20% compared to earlier approaches, showing improved consistency with CMIP6 simulations." (Line: 554).

Dear Editor and Reviewer 2,

We would like to express our sincere gratitude to you for the professional evaluation and valuable suggestions on our manuscript (Manuscript ID: EGUSPHERE-2025-2560; Title: *Enhancing Evapotranspiration Estimates Under Climate Change: The Role of CO₂ Physiological Feedback and CMIP6 Scenarios*). You affirmed the value of our study and pointed out directions for improving the analysis and figures that help us a lot. In response to the 8 specific revision comments, we have made detailed revisions one by one. The core revision contents are as follows:

1. Enriched the content of the abstract graph by adding a technical flowchart and result diagrams. These additions clearly present the experimental ideas, background, and key results.
2. Standardized the background color of Figures 5–8 to white, eliminating visual distractions and ensuring figure consistency.
3. Supplemented the definition of the CMIP6 data time range in Section 2.1, specifying the historical period (1850–2014) and future period (2015–2100, including 4 emission scenarios) as well as the data source. This revision could resolve potential confusion among readers regarding the analysis period.
4. Unified the expression of the reference variable for evaporation in Section 2.3.2, adjusting the variables in both the description and formula to be consistent, thus ensuring accuracy.
5. Added a description of the calibration process for the PM-RC-CO₂ model coefficient in Section 2.3.4, clarifying that the coefficient is derived from the ensemble mean of multiple models to improve method reproducibility.
6. Expanded the result analysis in Section 3.3 by adding Figures 9–10 (graphs showing the proportion of evapotranspiration (Ep) trends across different land use types) and corresponding analyses. These additions intuitively reflect the characteristics of the underlying surface and provide more comprehensive support for Ep-related results.
7. Standardized the expression "EP" to "Ep" in the Discussion section to maintain consistency with the previous text and avoid misunderstandings.
8. Supplemented specific regional cases of regional uncertainty in the Conclusion section. Drawing on the new content added in Section 3.3, it is pointed out that the vegetation response differences between cropland and shrubland are significant, making the conclusion more specific.

All modifications have been accurately applied to the corresponding positions in the original manuscript, aiming to improve the quality and completeness of the manuscript. Thank you again for the reviewer's careful guidance. We hope that these revisions can fully address your concerns. If both you and the editor agree to these modifications, we will upload the revised manuscript later.

With utmost sincerity,
Qianfeng Wang, on behalf of all co-authors
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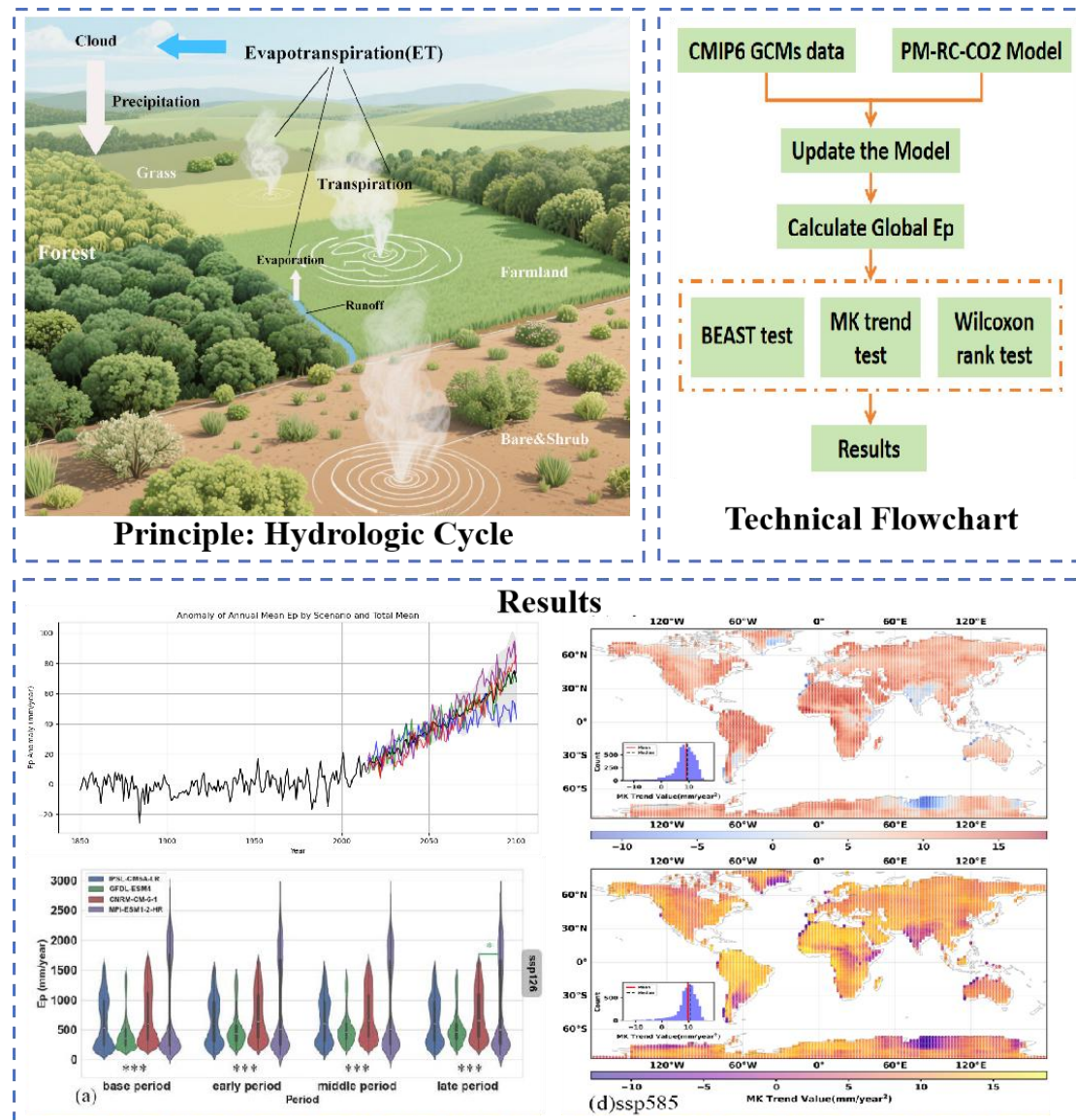
Reviewer 2 Comments

*This manuscript (EGUSPHERE-2025-2560, Enhancing Evapotranspiration Estimates Under Climate Change: The Role of CO₂ Physiological Feedback and CMIP6 Scenarios) addresses a critical gap in evapotranspiration (ET) estimation under climate change by integrating CO₂ physiological feedbacks with CMIP6 multi-scenario projections to update the Penman-Monteith (PM) model. The focus on nonlinear vegetation-atmosphere interactions also provides valuable insights for hydrological uncertainty analysis under global change. In general, this is a well-written manuscript, and it could be improved by polishing the analysis and figures. **I recommend the publication of this work once a series of revisions have been carried out.***

Response: Thank you for your professional comments, based on your suggestions, we have made the appropriate and detailed revisions.

1. Abstract Graph: The abstract diagram is too simple. Flow charts can be added to show the experimental ideas and writing background (such as how to derive new indicators, how to analyze, etc.) to enrich the content of abstract graph.

Response: Thank you for your suggestion. We have added technical process diagrams and result diagrams to the abstract graph to enrich its content as below:



2. Figure 5-8: There are different colors in the background. Unify their background colors (e.g., light gray/white) to avoid visual distraction and ensure coherence.

Response: Thank you for your advice. We have updated the relevant images and determined that the background color is consistent with white.

3. In Section 2.1, the manuscript does not clearly explain the specific details of the time range division for CMIP6 data, such as whether the connection logic between the "historical period" (1861-2100) and the "future period" (2015-2100) are uniformly applicable to all GCMs. It is recommended to supplement the clear definition of the data time range to avoid confusion among readers regarding the analysis period.

Response: Thanks. We have added the following contents to the corresponding position (Line:140-141) in Section 2.1: "These models were obtained from the CMIP6 data portal (<https://esgf-node.llnl.gov/search/cmip6/> , 1850-2014) and include

simulations for both the historical period and four future emission scenarios (2015-2100): SSP1-2.6, SSP2-4.5, SSP3-7.0, and SSP5-8.5.”

4. In Section 2.3.2 (Line 194), the description part uses ET_0 to refer to evaporation, but the corresponding formula does not have this variable, only E_p . It is suggested to unify the implied evaporation reference in this description and the formula.

Response: Thank you for your professional comments, based on your suggestions, we have made the appropriate revisions (Line: 202) to make the reference accurate and uniform.

5. In Section 2.3.4, the specific steps of how this the updated PM-RC-CO₂ model coefficient was derived from the four selected GCMs are not detailed. It is recommended to add a brief description of the calibration process, such as whether the coefficient is an ensemble mean of GCM-specific results or derived from a specific GCM to improve method reproducibility.

Response: Thank you for your constructive suggestions. This parameter is derived from the average of multiple model sets, and relevant description has been added in the corresponding position of section 2.3.4 (Line: 229-231).

6. The part 3.3 of the results is relatively single. It is recommended to enrich the analysis content and add some charts, such as the proportion of different land use types. This can more intuitively reflect the characteristics of underlying surface and provide more comprehensive background support for evapotranspiration (E_p) related results.

Response: Thanks for the suggestion, as below, we have added Figure 9-10 and its corresponding analysis in section 3.3:

“Regardless of the original or updated models, among the land use types such as Forest, Grassland and Tundra, the proportion of the area with MK trend greater than 0 ($Slope > 0$) is generally very high (more than 95%). These ecosystems are driven by climate change, and the E_p has a significant upward trend. The trend differentiation between Cropland and Shrubland is relatively obvious. For example, in the high emission scenario (SSP5-8.5), the proportion of farmland $slope < 0$ in the updated model (1.8%) is lower than that in the original model (5.3%), and the proportion of shrub $slope < 0$ in the updated model (3.9% -9.5%) is also different from that in the original model (8.3% -18.6%).

At the same time, updating the model effectively reduced the bias in trend estimation. The updated model also enhances the ability to capture CO₂ physiological feedbacks and scenario-specific responses, thereby reducing biases in trend estimation. Under different scenarios, the proportion of E_p rising trend of various land uses under high emission scenarios (such as SSP5-8.5) is more prominent than that under medium and low emission scenarios (such as SSP1-2.6). This indicates that emission intensity

exerts an amplifying effect on the differentiation of evapotranspiration trends across different land use types.

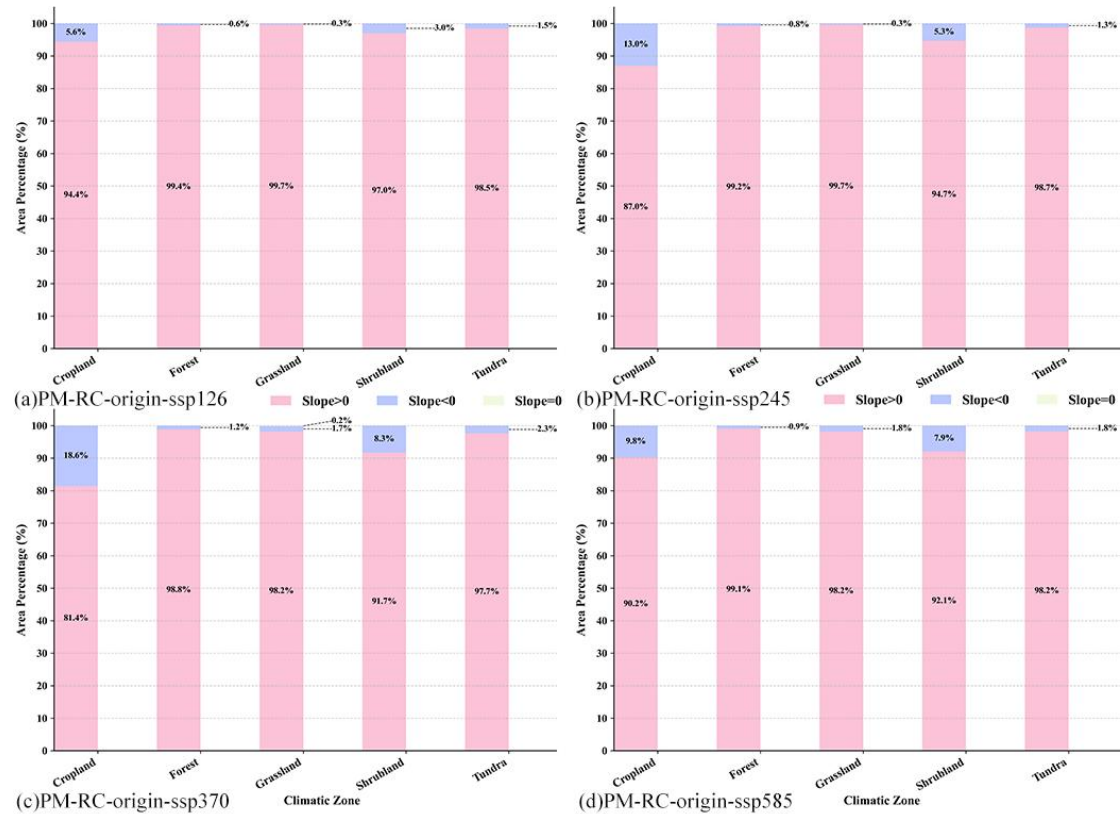


Figure 9 Ep trend percentage across different land uses derived from the Mann-Kendall (MK) method using the original ET-RC-CO₂ model under (a) SSP1-2.6, (b) SSP2-4.5, (c) SSP3-7.0, and (d) SSP5-8.5. The pink part represents MK trend greater than 0, the blue and purple part represents less than zero, and the light green part represents equal to zero.

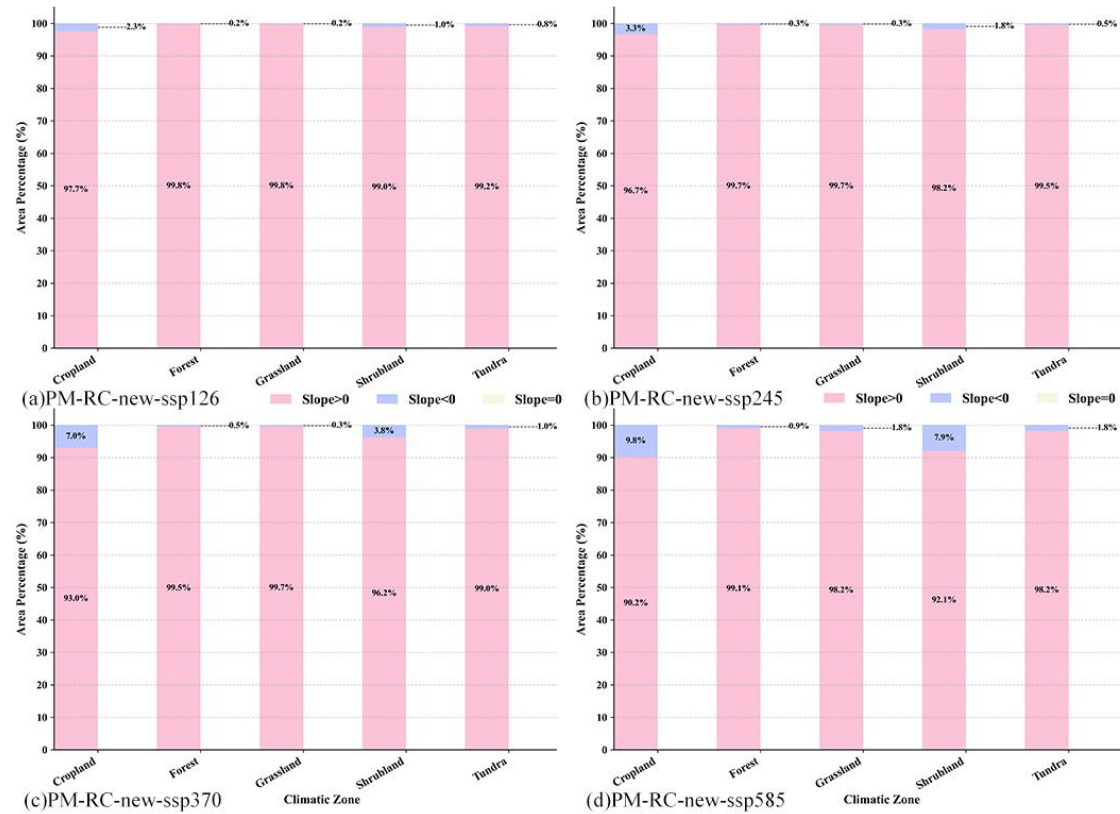


Figure 10 Same as Figure 9, but for the updated ET-RC-CO₂ model.”

7.Discussion(Line 427): Is the EP here the same as Ep in the previous paragraph? If so, it is recommended to change it to Ep to avoid misunderstanding.

Response: Thank you for your careful advice. Your understanding is correct. To avoid misunderstanding, we have unified relevant references as follow(Line: 463): “The observed increase in Ep severity is consistent across multiple analytical methods, supporting earlier findings by Mondal et al. (2024), Weij et al. (2024), and Yang et al. (2023).”

8. Section 5 (conclusion) mentions that "regional uncertainty stems from the heterogeneity of vegetation response", but does not specify which regions are more significant. It is suggested to add an explanatory sentence corresponding to recommendation and opinion 4 to make the conclusion more specific.

Response: Thank you for your professional advice. Referring to the new content added in section 3.3 of the main text (enriched based on your comment 4), we have added content as follow(Line: 558-562): “(3) Model parameterization plays a critical role in capturing CO₂-physiological feedbacks; however, regional uncertainties remain due to heterogeneity in vegetation responses and methodological sensitivities—for instance, the distinct differences in responses between croplands and shrublands. Additionally, regional uncertainties persist due to heterogeneous vegetation responses and methodological sensitivities.”