

Dear reviewers and editor,

We sincerely thank you for your careful evaluation of our manuscript and for the constructive comments and suggestions, which have been invaluable in improving the quality and clarity of our work.

Following these comments, we have carefully revised the manuscript and further strengthened the Discussion section in particular, with emphasis on clarifying the scale and interpretation of WUE, the assumptions and limitations of the attribution framework, the uncertainty associated with driver interactions, the applicability of the  $WY = P - ET$  assumption, and the interpretation of future  $[CO_2]$ -related projections. In addition, we have revised the manuscript to better articulate the methodological contribution of the improved CCW model and the new insights gained from introducing dynamic WUE into the attribution analysis. We have also addressed the minor comments by improving the presentation of figures, clarifying terminology and trend calculations, reducing redundant statements, and standardizing units throughout the manuscript.

We believe all of the concerns of referees have been properly addressed. Please see our point-to-point responses to the reviewers below.

Best,  
All authors

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# General Response to the Editor

Synthesis: This is the second-round revision of this manuscript, the reviewer who reviewed this manuscript last time is satisfied with the revised manuscript. The new reviewer suggested 1) clarify the scale and interpretation of WUE and provide stronger justification for its application to total ET estimation. 2) quantify the associated uncertainty and discuss potential interaction effects among drivers. 3) explicitly discuss the limitations of assumption of  $WY=P-ET$  with negligible storage change. 4) clarify the assumptions underlying these projections and provide a more balanced discussion of uncertainties. 5) better articulate how this methodological refinement leads to new insights; and several minor comments. I encourage the authors carefully address these comments in the Discussion section of the manuscript.

**Response:** We sincerely thank the Editor for the careful summary and guidance on this second-round revision. We are encouraged that the reviewer from the previous round is satisfied with the revised manuscript, and we greatly appreciate the Editor's clear identification of the key issues that still required further clarification and strengthening.

Following the Editor's recommendation, we have carefully revised the manuscript, with particular emphasis on the Discussion section, while also making corresponding revisions in the Methods, Abstract, Conclusions, and figure presentation. Specifically, we have:

(1) clarified the scale and interpretation of WUE, explicitly stating that the adopted formulation follows the ecosystem-scale definition, and further explained the scope and limitation of applying it to annual ET and WY attribution;

(2) revised the attribution framework to clarify that the scenario experiments represent the direct effects of each driver under fixed-background assumptions, rather than strictly separable contributions, and strengthened the discussion of interaction effects and the resulting limitation of the framework;

(3) explicitly discussed the validity and limitation of the  $WY = P - ET$  assumption, emphasizing that it is mainly appropriate at the annual scale and may be less reliable in regions affected by groundwater depletion or reservoir regulation;

(4) moderated the interpretation of the future [CO<sub>2</sub>]-related projection, clarifying that the SSP585-based estimate is a simple elasticity-based sensitivity illustration rather than a fully constrained deterministic projection, and added a more balanced discussion of related uncertainties;

(5) better articulated the methodological contribution and new insights of introducing dynamic WUE, emphasizing that its main value lies in improved process representation and attribution of CO<sub>2</sub> physiological effects on WY, rather than in large gains in fit statistics alone.

In addition, we have addressed the minor comments by improving figure readability and captions, clarifying terminology and trend definitions, reducing redundant text, and standardizing unit notation throughout the manuscript.

We believe that these revisions have substantially improved the clarity, rigor, and balance of the manuscript, and we are grateful for the Editor's guidance in helping us strengthen the paper at this stage.

# Response to Referee #1

**Synthesis:** This manuscript presents an improved coupled carbon–water model incorporating dynamic water-use efficiency to attribute water yield changes across China to climate, vegetation, and atmospheric CO<sub>2</sub>. The attempt to explicitly represent CO<sub>2</sub> physiological effects is valuable and addresses an important limitation in Budyko-type frameworks. However, several conceptual, methodological, and interpretational issues limit the robustness of the conclusions. Concerns remain regarding the attribution framework, model assumptions, and the interpretation of CO<sub>2</sub> effects.

**Response:** We sincerely appreciate the reviewer’s thorough and constructive evaluation of our manuscript. We are grateful that the reviewer recognized the value of explicitly incorporating CO<sub>2</sub> physiological effects into the improved coupled carbon–water model and acknowledged its relevance in addressing an important limitation of Budyko-type frameworks. We also thank the reviewer for clearly identifying the remaining conceptual, methodological, and interpretational concerns regarding the attribution framework, model assumptions, and the interpretation of CO<sub>2</sub> effects. We have carefully considered these comments and revised the manuscript accordingly, with particular emphasis on strengthening the Discussion section and clarifying the scope, assumptions, and uncertainties of our approach. The point-to-point responses to the comments and revision are detailed below. In the following, we have detailed how these comments (in black) are raised and our responses (in deep sky blue).

## **Major comments:**

**1:** The manuscript estimates WUE using the formulation of Cheng et al. (2017), which is derived from stomatal conductance theory and incorporates variables such as VPD, CO<sub>2</sub> concentration, and LAI. While this formulation captures key physiological controls, it is fundamentally rooted in leaf- to canopy-scale transpiration processes rather than directly representing ecosystem-scale evapotranspiration (ET). This raises a conceptual concern regarding its subsequent use in estimating total ET via  $ET = GPP/WUE$ . The formulation appears more consistent with transpiration-based WUE, whereas total ET also includes soil evaporation and interception loss. The authors should clarify the scale and interpretation of WUE (leaf, canopy, or ecosystem level) and provide stronger justification for its application to total ET estimation.

**Response:** Thanks for your constructive comment. We agree that the previous manuscript did not sufficiently clarify the scale and interpretation of the WUE formulation adopted from Cheng et al. (2017), which could lead to confusion between leaf-/canopy-scale transpiration-based WUE and ecosystem-scale WUE. We would like to clarify that the WUE formulation used in this study follows the ecosystem-scale definition in Cheng et al. (2017), where WUE is defined as the ratio of GPP to total ecosystem evapotranspiration (E) rather than as leaf-level intrinsic WUE. In the original WEC framework, total evapotranspiration includes transpiration, soil evaporation, and canopy interception, and the upscaling from leaf-scale stomatal theory to ecosystem-scale WUE is achieved analytically through the transpiration–soil evaporation partitioning term and the interception term. Therefore, the use of  $ET = GPP/WUE$  in our study is conceptually consistent with the original formulation.

At the same time, we agree that our implementation involves an important simplification that needs to be stated more explicitly. In the revised manuscript, we now clarify that the canopy interception term  $f_i$  was fixed at 0 in our application. This means that canopy interception was not explicitly represented, although soil evaporation is still implicitly considered through the LAI-based partitioning term. We acknowledge that this simplification may introduce bias in the absolute magnitude of ET, especially in humid or densely vegetated regions, and therefore the estimated ET should be interpreted as an effective annual ecosystem-scale ET suitable for long-term WY attribution.

To make this point unambiguous, the manuscript now gives a clearer description of the scale of WUE in the Methods section, explains the physical meanings of the  $[1-\exp(-k \times \text{LAI})]$  and  $(1-f_i)$  terms more explicitly. And the Discussion section also now states more clearly that this framework is more appropriate for annual-scale WY attribution than for detailed ET component partitioning.

**Relevant text reads (line 205-215):** In this study, we improve the CCW model by incorporating dynamic water use efficiency (WUE) instead of static UWUE. This enhancement addresses the limitations of the original model, particularly its inability to adapt to environmental changes such as varying  $[\text{CO}_2]$  and vapor pressure deficit (VPD). WUE's estimation method is estimated using the WEC (Water Efficiency and Carbon) equation proposed by Cheng et al. (2017), where WUE is defined at the ecosystem scale as the ratio of GPP to total ecosystem evapotranspiration rather than as a leaf-level intrinsic WUE. Under this framework, evapotranspiration includes transpiration, soil evaporation, and canopy interception, and the analytical upscaling from stomatal theory to ecosystem WUE is represented through the transpiration–soil evaporation partitioning term and the interception term. The final formula for calculating WUE is:

**line 224-227:** This equation provides a dynamic estimate of WUE, considering the effects of environmental factors like VPD,  $\text{CO}_2$  concentration, atmospheric pressure, and canopy structure (LAI). The factor  $1-\exp(-k \times \text{LAI})$  represents the partitioning between transpiration and soil evaporation associated with canopy cover at monthly-to-annual scales. In this study, the interception evaporation factor ( $f_i$ ) was set to zero to reduce data requirements and maintain consistency in long-term grid-scale attribution.

**line 240-242:** Under this framework, ET should be interpreted as an effective annual ecosystem-scale evapotranspiration suitable for long-term WY attribution, rather than a detailed partitioning of individual ET components.

**line 613-619:** Firstly, the adopted WUE formulation is defined at the ecosystem scale, but the present implementation simplifies the interception term by assuming  $f_i = 0$ . Soil evaporation is still implicitly represented through the LAI-based partitioning term, whereas canopy interception is not explicitly allowed to vary across space or time. This simplification may bias the absolute magnitude of ET and WY, particularly in humid and forested regions, although its influence on long-term WY trends is expected to be smaller than on short-term hydrological fluxes.

**2:** The scenario-based attribution approach assumes that climate, vegetation, and  $\text{CO}_2$  effects can be isolated by fixing other variables. However, these drivers are not independent, as vegetation dynamics are strongly influenced by climate and  $\text{CO}_2$  (as partially acknowledged in Lines 575–583). This introduces non-orthogonality in attribution, meaning the reported

contributions cannot be interpreted as strictly separable. The authors should quantify the associated uncertainty and discuss potential interaction effects among drivers.

**Response:** Thank you for this constructive comment. We agree that climate, vegetation, and [CO<sub>2</sub>] are not fully independent, and that the previous wording could overstate the separability of the scenario-based attribution results. We therefore revised the manuscript to clarify that the vegetation, climate, and [CO<sub>2</sub>] scenarios are intended to represent the direct effects of each driver on WY when the other drivers are held fixed, rather than strictly orthogonal or fully independent contributions to the total observed WY change.

We also acknowledge that the interaction-related uncertainty associated with this non-independence was not quantified in the present study. A rigorous quantification would require additional factorial scenario experiments or coupled vegetation–climate modeling that explicitly resolves interaction pathways, which is beyond the scope of the current framework. Instead, we strengthened the Discussion to more explicitly acknowledge this limitation and to clarify the potential interaction and feedback effects among climate, vegetation, and CO<sub>2</sub>.

Accordingly, the manuscript was revised in two places. In Section 2.3, we clarified the definition and interpretation of the scenario experiments and the meaning of the reported relative contributions. In Section 4.3, we expanded the discussion of interaction effects and explicitly stated that the attribution results should be interpreted as conditional direct effects under fixed-background assumptions. This interpretation is also consistent with previous studies emphasizing that water-yield attribution is complicated by coupled climate–vegetation interactions and incomplete separability among drivers.

**Relevant text reads (line 274-275):** The resulting WY series under each scenario represents the direct impact of the corresponding driver under the assumption that the other drivers are fixed.

**line 291-293:** the relative contributions ( $RC_{vegetation}$ ,  $RC_{climate}$ , and  $RC_{CO_2}$ ) are expressed as percentages, indicating the normalized magnitude of the scenario-derived direct effects.

**line 623-637:** Secondly, the complex interrelationships among climate, vegetation, and [CO<sub>2</sub>] cannot be fully disentangled. Vegetation exhibits tight biophysical interactions and feedback with climate, making it difficult to separate the impacts of climate change, vegetation dynamics, and [CO<sub>2</sub>] on hydrological responses **in a strictly independent manner**. Changes in vegetation, such as NDVI, reflect a combination of climate change, human activities (e.g., reforestation and irrigation), and natural vegetation growth. Additionally, vegetation greening in upwind regions can increase atmospheric moisture, potentially enhancing precipitation downwind (Zhang et al., 2021a), which may counteract some of the negative impacts of increased evapotranspiration on local WY. Although the climate data used in our model may implicitly capture some of these feedbacks, they cannot be explicitly separated in this analysis. Consequently, our results represent an attempt to estimate the direct first-order net impacts of climate, vegetation greening, and [CO<sub>2</sub>] increase on WY (Zhang et al., 2021b). Future research should adopt more comprehensive models that consider soil-vegetation-atmosphere interactions to better differentiate the contributions of each driving factor to WY.

**3:** The study assumes WY=P-ET with negligible storage change. This assumption may not hold in regions with significant groundwater depletion or reservoir regulation. The authors should explicitly discuss the limitations of this assumption and clarify the temporal scale at which it is considered valid.

**Response:** Thanks for your constructive comment. We agree that the assumption  $WY = P - ET$  with negligible storage change is an approximation that is mainly appropriate at the annual scale, and may not hold in regions affected by substantial groundwater depletion or strong reservoir regulation. We have revised the manuscript accordingly in two places. In Section 2.2, we clarified the temporal scale at which this assumption is considered valid. In Section 4.3, we expanded the discussion of its limitations by explicitly noting that groundwater pumping, irrigation, and reservoir operation can decouple modeled  $WY$  from observed runoff and therefore introduce uncertainty into the attribution results.

**Relevant text reads (line 254-256):** This approximation is most suitable for annual to multi-year analyses in regions without substantial long-term groundwater depletion or strong reservoir regulation.

**line 638-643:** Thirdly, the improved CCW model does not incorporate certain human activities, such as large-scale irrigation, groundwater pumping, and reservoir regulation, which also limits the assumption of  $WY = P - ET$  with negligible storage change. This assumption is mainly appropriate at the annual scale and becomes less reliable in regions where long-term groundwater depletion or reservoir operation substantially alters water storage and runoff routing.

**4:** The manuscript concludes that  $CO_2$  may drive a +1.29% annual increase in water yield under SSP585. This conclusion appears overstated, as it does not account for potential counteracting effects such as increasing VPD or saturation of WUE under elevated  $CO_2$  and drought stress. The authors should clarify the assumptions underlying these projections and provide a more balanced discussion of uncertainties.

**Response:** Thank you for your constructive comment. We agree that the original wording of the +1.29%  $WY$  increase under SSP585 was too strong. This result is a simple elasticity-based estimate intended to illustrate the potential importance of rising  $[CO_2]$  through its direct physiological effect on transpiration and  $WY$ , rather than a fully constrained projection. We have therefore revised the manuscript to present this result more cautiously. In the Abstract, we now use more tentative wording. The fuller discussion of uncertainties, including the possible roles of rising VPD, drought stress, and WUE saturation, is retained in Sections 4.2, 4.3, and the Conclusions.

**Relevant text reads (line 30-34):** Therefore, CMIP6 SSP585 projections indicate that accelerating  $[CO_2]$  rise will amplify its hydrological effect to a +1.29% annual  $WY$  increase by 2100, surpassing vegetation influences, implying a stronger hydrological role under future forcing. By explicitly representing dynamic WUE, this study improves process-based  $WY$  attribution and shows that  $[CO_2]$  physiological regulation can partly offset vegetation-induced  $WY$  reductions, thereby providing new insights for regional water resource management under climate change.

**line 587-598:** This historical constraint, however, belied  $CO_2$ 's transformative potential under intensified forcing scenarios. CMIP6 SSP585 projections indicate  $[CO_2]$  will rise at 2.34%/yr – nearly fivefold faster than historical rates (Cheng et al., 2022). Using the historical elasticity coefficient as a simple sensitivity-based approximation,  $CO_2$ 's elasticity would drive a +1.29% annual  $WY$  increase, eclipsing both vegetation greening effects and even surpassing precipitation-driven changes in some regions. However, this estimate assumes that the

historical [CO<sub>2</sub>] elasticity remains unchanged and does not account for possible counteracting effects such as rising VPD, drought stress, or saturation of WUE under elevated [CO<sub>2</sub>] (Adams et al., 2020; Li et al., 2023). Nevertheless, the result suggests that [CO<sub>2</sub>] may become a more important hydrological modifier under strong future forcing, particularly in the 400–1600 mm/yr precipitation zones.

**line 654-657:** In particular, the simple elasticity-based SSP585 estimate of the [CO<sub>2</sub>] effect does not account for concurrent changes in VPD or possible saturation of WUE under elevated [CO<sub>2</sub>] and drought stress, which adds uncertainty to the future WY response.

**line 682-683:** Thus, accelerating [CO<sub>2</sub>] rise (2.34% /yr under SSP585) will amplify its hydrological role, potentially elevating [CO<sub>2</sub>]-driven WY increases to +1.29% annually by 2100, surpassing climate and vegetation impacts, although this estimate does not account for concurrent VPD changes or possible WUE saturation.

**5:** The main methodological novelty appears to be the use of a dynamic WUE formulation to estimate ET, compared to previous approaches using static WUE. However, the improvement in model performance (e.g., R<sup>2</sup> increasing from 0.68 to 0.70 in Figure 4) is relatively modest. The authors should better articulate how this methodological refinement leads to new insights, particularly in terms of water yield dynamics, and clarify the fundamental differences between the two approaches beyond marginal statistical improvement.

**Response:** Thank you for your constructive comment. We agree that the improvement in model performance is modest and that the significance of the dynamic-WUE refinement should not be judged mainly by the small increase in R<sup>2</sup>. Its main value lies in improving the process representation of ET and WY, because it explicitly incorporates the physiological effect of rising [CO<sub>2</sub>] and thus enables the separation of [CO<sub>2</sub>]-driven stomatal effects from LAI (vegetation structural) effects. This leads to new insights into WY dynamics, especially by showing that [CO<sub>2</sub>] can partially offset vegetation-induced WY reductions and may play a stronger hydrological role under future CO<sub>2</sub> rise. We have revised the Abstract, Section 4.1, and Section 4.2 to emphasize this point more clearly.

**Relevant text reads (line 30-34):** Therefore, CMIP6 SSP585 projections indicate that accelerating [CO<sub>2</sub>] rise will amplify its hydrological effect to a +1.29% annual WY increase by 2100, surpassing vegetation influences, implying a stronger hydrological role under future forcing. By explicitly representing dynamic WUE, this study improves process-based WY attribution and shows that [CO<sub>2</sub>] physiological regulation can partly offset vegetation-induced WY reductions, thereby providing new insights for regional water resource management under climate change.

**line 522-529:** Compared with the original CCW model using static UWUE, the improved framework does not merely provide a modest statistical improvement. Its main advance is that it allows WUE to vary dynamically with [CO<sub>2</sub>], VPD, and canopy structure, thereby explicitly representing the physiological regulation of ET by rising [CO<sub>2</sub>]. This makes it possible to distinguish LAI (vegetation structural) water consumption from [CO<sub>2</sub>]-induced stomatal water-saving effects, which is essential for interpreting why WY responses differ across regions and why [CO<sub>2</sub>] can partly offset vegetation-related WY reductions.

**line 559-568:** Although the improvement in overall model performance is modest, the main value of introducing dynamic WUE is not limited to statistical enhancement. More importantly,

this refinement allows the improved CCW model to explicitly represent the physiological effect of rising [CO<sub>2</sub>] on evapotranspiration, which cannot be resolved by the original static-UWUE formulation. As a result, the model can distinguish vegetation-driven increases in water consumption from [CO<sub>2</sub>]-induced stomatal water-saving effects, thereby providing new insight into why WY responses differ across regions and why [CO<sub>2</sub>] can partially offset vegetation-related WY reductions, especially in transitional hydroclimatic zones.

### **Minor Comments**

**1:** Lines 44–61: The importance of computing water yield as a key metric is not clearly articulated. The authors should explicitly state why water yield is chosen and how it relates to the broader research motivation presented in this section.

**Response:** Thank you for your constructive comment. We agree that the importance of using water yield (WY) as the key metric was not sufficiently articulated in the original Introduction. The manuscript already noted in the second paragraph that, at the annual scale, WY can be approximately linked to runoff when long-term storage change is negligible. However, our motivation for using WY is broader than this equivalence alone. In this study, WY was chosen because it is directly defined at the grid scale as precipitation minus evapotranspiration, which makes it more suitable than basin-integrated runoff for spatially explicit attribution analysis across China. We have therefore revised the Introduction to clarify both points: (1) WY retains a clear hydrological connection to runoff at annual scales, and (2) WY is particularly appropriate for our grid-based framework aimed at resolving regional heterogeneity beyond basin-scale runoff analysis.

**Relevant text reads (line 52-54):** Among hydrological metrics, water yield (WY) is especially relevant because it directly represents the amount of water remaining after evapotranspiration and is therefore closely linked to runoff generation and regional water availability.

**line 59-62:** Understanding the intricate interplay among vegetation dynamics, climate change, and [CO<sub>2</sub>] within the water cycle, particularly concerning WY therefore it is not only of global relevance but also of profound importance for advancing sustainable water resource management and ecological restoration strategies in China under accelerating environmental change.

**2:** Line 271: The term “trend” is not clearly defined. Please clarify whether it refers to a linear slope or another metric, and provide details of the calculation method.

**Response:** Thank you for your constructive comment. We agree that the term “trend” was not defined explicitly enough in the original text, although the calculation method was briefly mentioned. We have therefore revised the Methods section to clarify that “trend” refers to the Theil–Sen slope of the annual WY series over 1982–2017 for each scenario, representing the long-term monotonic rate of change in WY. We also clarified that this slope is used directly in Eq. (6) to calculate the relative contributions of vegetation, climate, and [CO<sub>2</sub>]. The corresponding revision was made in Section 2.3.

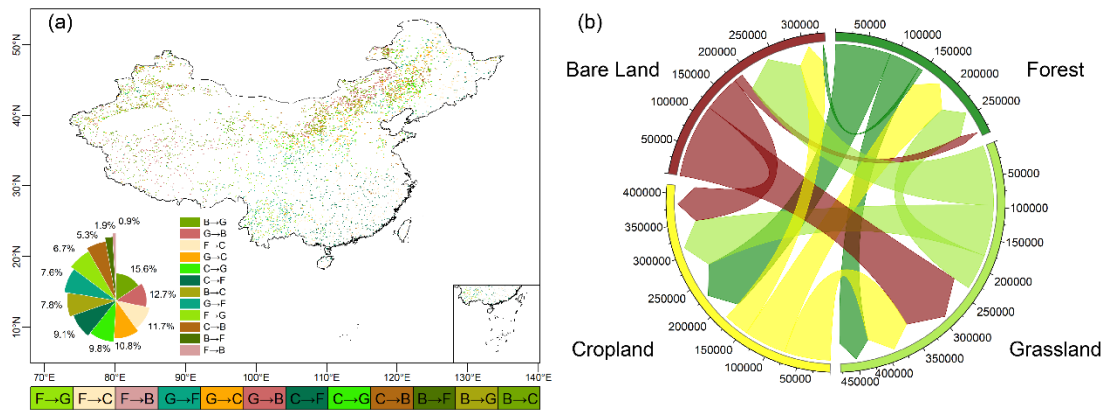
**Relevant text reads: (line 281-284):** For each scenario, the long-term trend in annual WY over 1982–2017 was quantified using the Theil–Sen estimator, yielding a robust slope. Here, “trend” refers to the Theil–Sen slope of the annual WY series, representing the long-term monotonic rate of change in WY over the study period.

**Line 288-293:** where  $trend_{vegetation}$ ,  $trend_{climate}$ , and  $trend_{CO_2}$  denote the Theil–Sen slopes of the annual WY series under vegetation, climate, and [CO<sub>2</sub>] scenarios, respectively. These slopes represent the long-term rates of WY change attributable to each driver within the scenario framework; the relative contributions ( $RC_{vegetation}$ ,  $RC_{climate}$ , and  $RC_{CO_2}$ ) are expressed as percentages, indicating the normalized magnitude of the scenario-derived direct effects.

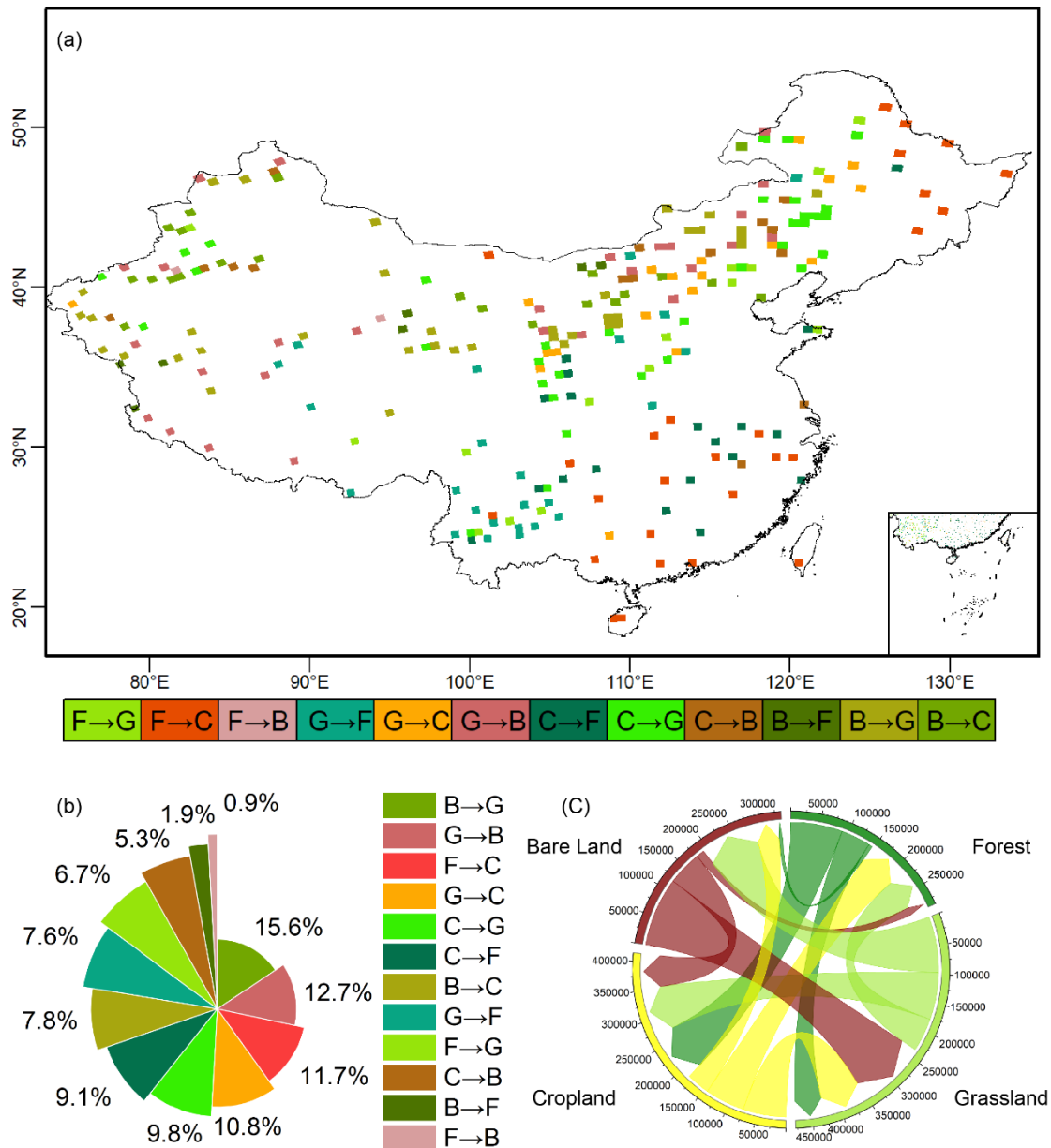
**3:** Figure 3b: The color legend is missing, and the unit of the values shown in panel (b) is unclear. Please add a complete legend and specify units.

**Response:** Thank you for your constructive comment. To address this comment together with the following one on Figure 3a, we revised Figure 3 as a whole. In the revised figure, the original Figure 3b has been reorganized as Figure 3c. We directly labelled the land-cover classes around the chord diagram, which makes it straightforward to identify the conversion from one land-cover type to another. We also clarified in the caption that the values around the circle indicate converted area (km<sup>2</sup>).

Initial picture:



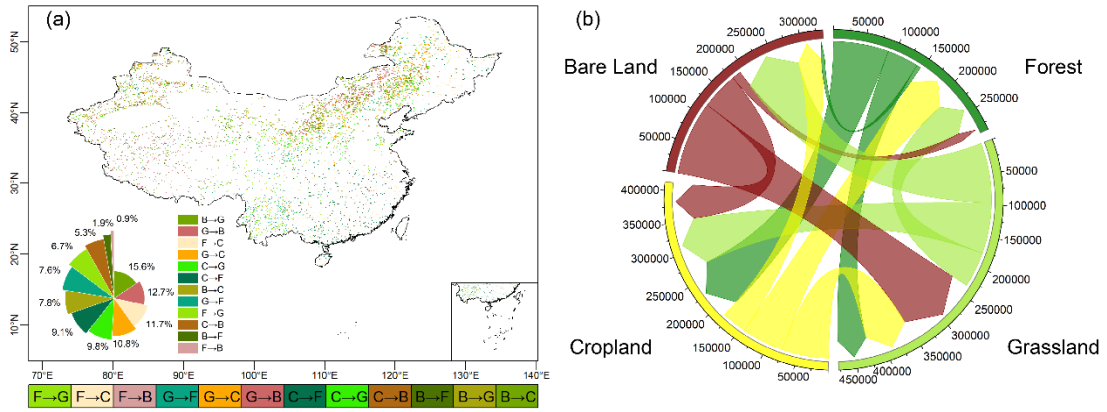
Revised picture:



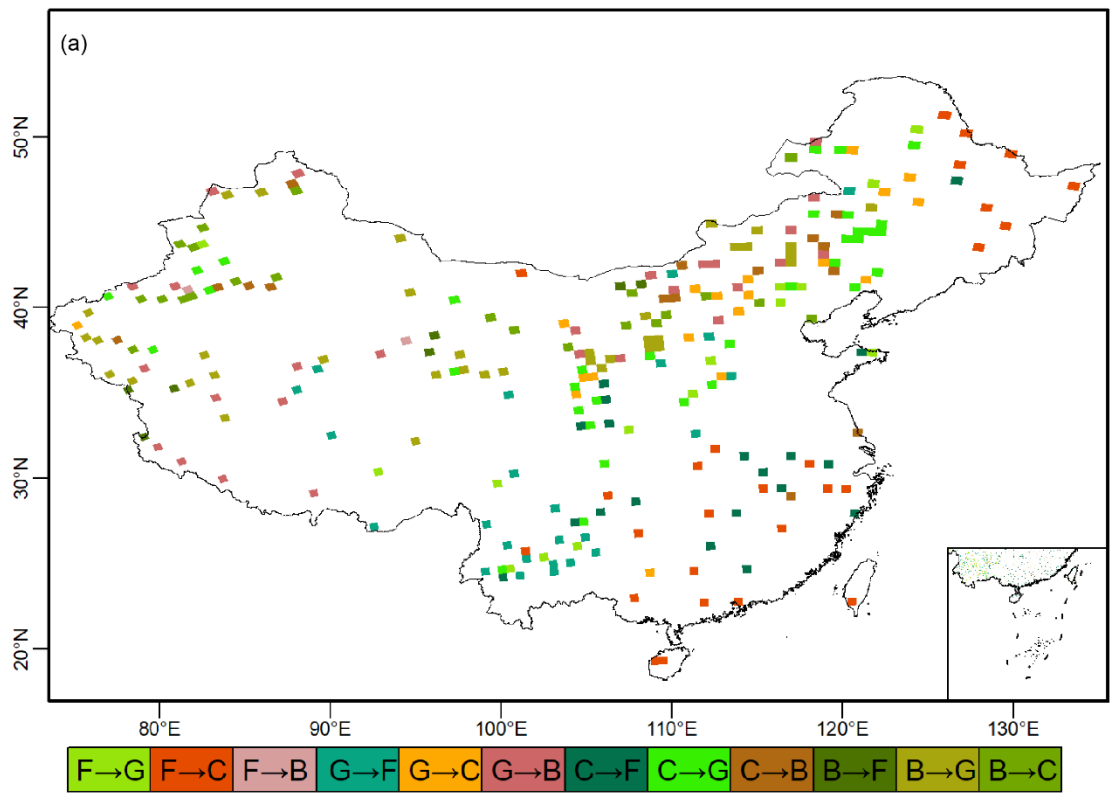
4: Figure 3a: The map is too small to interpret clearly, and the meaning of the legend (e.g., f-g) is not well explained. Please enlarge the figure and provide a more detailed caption.

**Response:** Thank you for your constructive comment. We agree that the original map panel was too compact and that the abbreviated conversion labels were not sufficiently explained. We therefore redesigned Figure 3 by separating the original inset content into independent panels and enlarging the spatial map substantially. We also revised the caption to explicitly define the abbreviations used in panel (a), where F = forest, G = grassland, C = cropland, and B = bare land, and clarified that the arrow indicates the direction of land-cover conversion (e.g., F→G means conversion from forest to grassland).

Initial picture:



Revised picture:



**5:** Lines 83–93: Multiple variables are represented using the same symbol “n,” which creates confusion. It is recommended to use distinct symbols or explicitly state variable names to improve clarity.

**Response:** Thank you for your constructive comment. After checking the manuscript carefully, we confirm that the symbol “n” was intended consistently to denote the Budyko catchment parameter, rather than multiple different variables. We agree, however, that this was not stated clearly enough in the original text and could cause confusion. We have therefore revised the manuscript to define it explicitly at first mention as the Budyko parameter (n) and to use this wording consistently in the relevant Introduction, Discussion, and Table 4 descriptions. These revisions improve clarity without changing the scientific meaning.

**Relevant text reads (line 81-95):** Among these conceptual models, the Budyko framework, widely used to separate climate change effects on runoff, quantifies water balance through the aridity index (PET/precipitation) and incorporates a catchment-specific Budyko parameter (n) representing integrated land surface characteristics (e.g., vegetation, soil, topography) (Zhang et al., 2022, 2016). However, most Budyko-based applications primarily emphasize climate-driven attribution; vegetation and [CO<sub>2</sub>] influences are typically introduced only indirectly—by assigning temporal changes in the Budyko parameter (n) to vegetation (Tan et al., 2024; Xue et al., 2022; Zhou et al., 2023) or correlating the Budyko parameter (n) with NDVI (Liu et al., 2024; Tan et al., 2023), and by embedding [CO<sub>2</sub>] effects through PET adjustments (Liu et al., 2024). These practices conflate vegetation with other controls captured by the Budyko parameter (n) (e.g., soil, topography) and mix [CO<sub>2</sub>]-physiological impacts with meteorological drivers in PET, making it difficult to isolate vegetation structural change from [CO<sub>2</sub>]-induced stomatal adjustments and to ascribe mechanisms robustly (Gan et al., 2021).

**line 476-479:** Traditional Budyko-based frameworks often attribute vegetation effects to temporal variations in the Budyko parameter (n) by either statistically regressing the Budyko parameter against vegetation proxies such as NDVI (Liu et al., 2024; Tan et al., 2023) or simplistically equating the Budyko parameter to vegetation effects (Li et al., 2020; Zhou et al., 2023).

**line 483-485:** For example, while rising [CO<sub>2</sub>] levels directly reduce stomatal conductance and transpiration, Budyko-based studies often misinterpret this effect as part of the the Budyko parameter’s variability, erroneously attributing it to vegetation changes (Zeng et al., 2020).

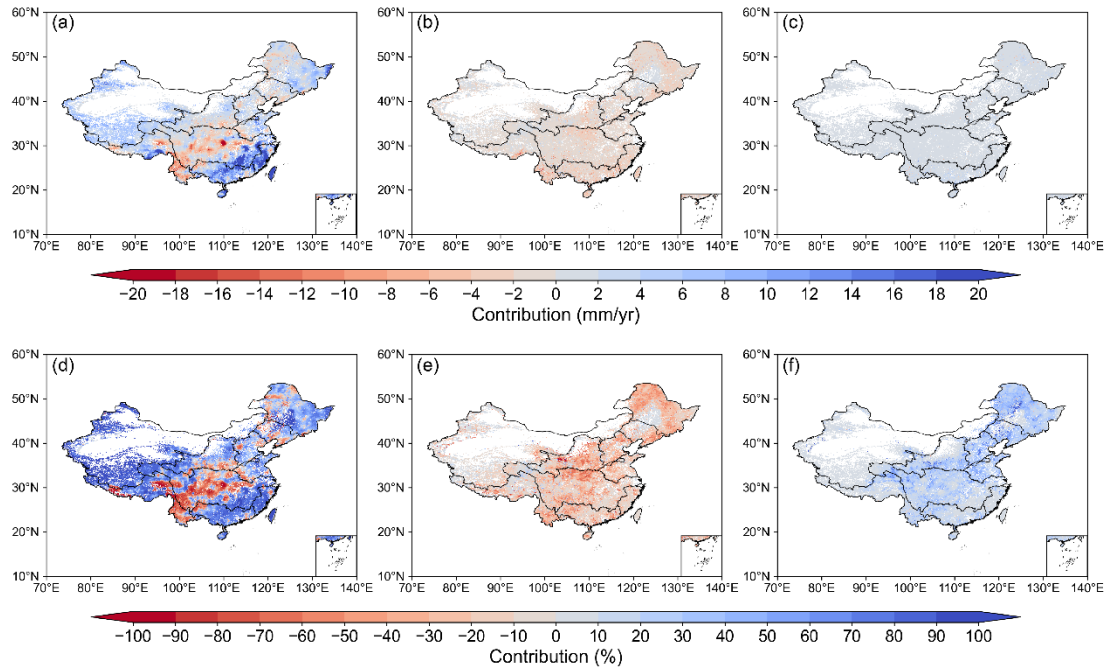
**6:** Figure 5: The spatial patterns in the panels are highly similar. Consider retaining only the relative change map and enlarging it for clarity, while moving the remaining panels to the supplementary material.

**Response:** Thank you for your constructive comment. We agree that the original layout of Figure 5 was relatively compact and could limit visual clarity. After careful consideration, however, we chose to retain both the absolute and relative contribution maps in the main text because they provide complementary information rather than redundant patterns: the absolute maps show the hydrological magnitude of each driver’s effect, whereas the relative maps indicate the normalized dominance of each driver. Presenting both is important for interpreting cases where a driver has a limited absolute effect but a non-negligible relative contribution.

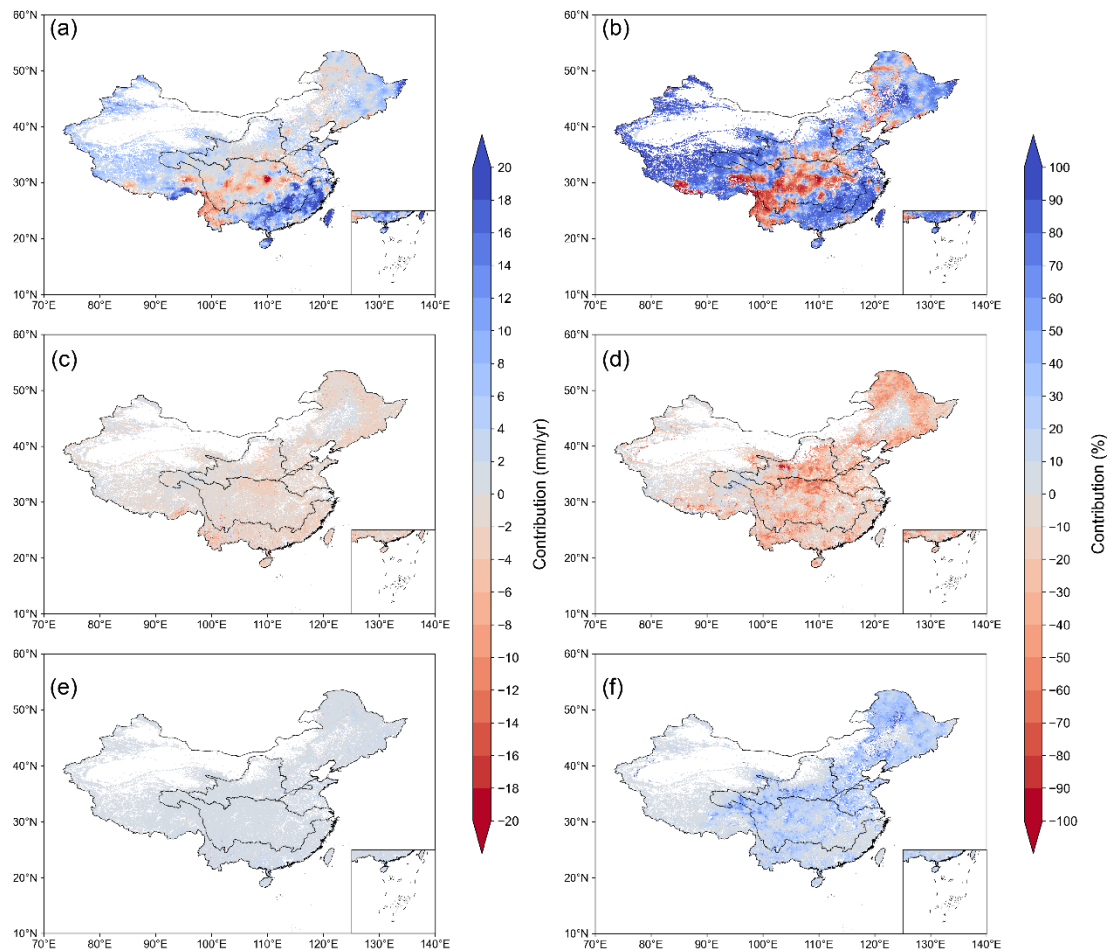
Instead of moving part of the figure to the supplementary material, we revised the layout of Figure 5 from a three-panel horizontal arrangement to a two-column vertical arrangement,

enlarging each panel by approximately 1.5 times. This substantially improves readability and makes the spatial differences among panels clearer, while preserving the direct comparison between absolute and relative contributions in the main text. We also revised the corresponding figure caption and all in-text references to match the the new panel order.

Initial picture:



Revised picture:



## 7: Discussion section

The discussion is primarily organized by river basins. It would be beneficial to also analyze results across different climate zones to provide more process-based insights.

**Response:** Thank you for your constructive comment. We agree that a climate-zone perspective can provide additional process-based insight beyond basin-scale comparison. The discussion in Section 4.2 was mainly organized by river basins to maintain consistency with previous basin-based studies, while the later analysis already considered hydroclimatic differences through precipitation-zone and elasticity-based interpretation. To make this perspective more explicit, we made a minor revision in Section 4.2.

**Relevant text reads (line 557-559):** This basin-scale contrast is also consistent with a broader hydroclimatic gradient, as vegetation and [CO<sub>2</sub>] effects become relatively more important in the intermediate 400–1600 mm precipitation zone.

**Line 574-579:** However, spatial analysis showed that vegetation and [CO<sub>2</sub>] collectively dominated WY changes in 400–1600 mm precipitation zones, despite their lower sensitivity rankings. The joint effect of elasticity and the magnitude of driver change determines each driver's net contribution. In the 400–1600 mm precipitation zones, NDVI displayed (Fig. 8) a larger relative temporal variation compared with precipitation, which fluctuated within a narrower range. Consequently, vegetation's stronger relative change amplified its hydrological influence, overriding its lower elasticity.

**8:** Lines 16–18: The sentence structure is unclear (e.g., “integrating dynamic WUE better capture...”). Please revise for grammatical correctness and clarity.

**Response:** Thank you for your constructive comment. We agree that the original sentence was grammatically unclear. We have revised the sentence in the Abstract for correctness and clarity.

**Relevant text reads (line 16-18):** Therefore, this study improved the coupled carbon and water (CCW) model by integrating dynamic water-use efficiency (WUE) to better capture [CO<sub>2</sub>] physiological feedbacks.

**9:** Lines 56–61; There is redundancy in describing the importance of the study. This section could be shortened to improve conciseness.

**Response:** Thank you for your constructive comment. We agree that the end of the first paragraph in the Introduction was somewhat repetitive in emphasizing the importance of the study. We have therefore shortened and streamlined this part to improve conciseness, while retaining the key points regarding the relevance of WY, the representativeness of China as a study region, and the broader significance for water resource management and ecological restoration.

**Relevant text reads (line 55-59):** China’s diverse climatic zones and pronounced greening make it an ideal natural laboratory for investigating these ecohydrological feedbacks, with implications for both China (Ogutu et al., 2021; Yang et al., 2019) and other semi-arid and monsoon-influenced regions such as the Sahel, South Asia, and the Mediterranean Basin (Nkiaka et al., 2025; Rahman et al., 2025; Serrano-Notivoli et al., 2022).

**10:** Throughout the manuscript, units are not consistently formatted (e.g., mm/yr vs mm/year). Please standardize unit notation throughout the manuscript.

**Response:** Thank you for your constructive comment. We carefully checked the unit notation throughout the manuscript and standardized it for consistency. Following the notation already used in most figures, trend and rate units were unified as mm/yr, °C/yr, Pa/yr, W/m<sup>2</sup>/yr, and %/yr, and inconsistent expressions such as mm/year and mm·yr<sup>-1</sup> were revised accordingly. We also standardized precipitation-zone notation. The corresponding revisions were made in the figure captions and the main text.

**Relevant text reads (line 378-384):** As shown in Fig. 4a and b, the observed annual water yield (WY) and the simulated annual WY by the improved CCW model showed strong linear correlations ( $R^2 = 0.7$ ), with the regression line slope being 1.45,  $R^2$  being 0.7, and RMSE being 9.54 mm/yr. By contrast, the initial model without WUE showed weaker skill (slope = 1.45,  $R^2 = 0.68$ , RMSE = 9.62 mm/yr), indicating that explicitly representing [CO<sub>2</sub>]-induced regulation of water-use efficiency measurably improves accuracy and reduces bias.

**line 574-576:** However, spatial analysis showed that vegetation and [CO<sub>2</sub>] collectively dominated WY changes in 400–1600 mm precipitation zones, despite their lower sensitivity rankings.

**line 577-579:** In the 400–1600 mm precipitation zones, NDVI displayed (Fig. 8) a larger relative temporal variation compared with precipitation, which fluctuated within a narrower range.

**line 597-598:** Nevertheless, the result suggests that [CO<sub>2</sub>] may become a more important hydrological modifier under strong future forcing, particularly in the 400–1600 mm precipitation zones.

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# Response to Referee #2

Synthesis: I would like to thank the authors for their careful and thorough responses. My previous concerns have been well addressed, and the clarifications provided have resolved my doubts. The manuscript has reached a quality that is suitable to proceed to the next stage of publication.

**Response:** We sincerely thank the reviewer for the positive assessment of our revised manuscript. We are grateful that the concerns raised during the previous review have been satisfactorily addressed and that the clarifications provided have resolved the remaining doubts. We appreciate the reviewer's valuable comments and support, which have helped improve the manuscript.