

Interactive comment on “Leaf Area Index identified as a major source of variability in modelled CO₂ fertilization” by Qianyu Li et al.

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Dear editor,

We appreciate the insightful comments on our manuscript by all reviewers, and will make substantial revisions to improve this manuscript. We hope that our responses below address the concerns raised by reviewers. The major proposed changes are: We will include results from carbon-nitrogen (C-N) coupled and carbon-nitrogen-phosphorus (C-N-P) coupled simulations of CABLE to study how the CO₂ fertilization effects (β factor) at different levels will change with nutrient limitations for different plant functional types. Our results from C-N and C-N-P coupled simulations support our previous conclusion with C only simulations. We will clarify the motivation and contribution

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of our study as the reviewers suggested in our revised manuscript. We will expand the scope of our work to attract more readers and carefully correct language in our revised manuscript.

Reviewer 2: Synopsis: In this paper, the authors run CABLE for seven C3 vegetation types, without nutrient cycling, and calculate CO₂ fertilization for the RCP 8.5 scenario. CCSM simulations from 1901 to (the paper says 1910; I assume they mean 2010) holding carbon-climate feedbacks constant (driving the model with the averaged meteorology-I'm guessing average annual cycle, although the authors do not say) and feeding CABLE increasing CO₂ concentration from the CCSM RCP 8.5 results.

They find that CO₂ fertilization differs between PFTs, and decreases with time during the period 2011-2100. Fertilization is relatively constant both between PFTs and when the calculation is made on a per-unit leaf level, and shows much larger diversity both across PFTs and when the CO₂ fertilization is calculated on a unit-leaf vs. integrated canopy basis. The authors close with the claim that simulated LAI is critical to the calculation of CO₂ fertilization in climate simulations.

Response: We thank the reviewer for the time she or he spent on reviewing our manuscript. The above paragraphs are a good summary of what we did for this study. While most of the summary is accurate, we would like to clarify here that CABLE model has been run from 1901 to 2100. Before that, CABLE was spun up by using meteorological forcing from 1901 to 1910 repetitively until a steady state was achieved. And we indeed used the average annual cycle of meteorological forcing data to fix carbon-climate feedbacks. We will clarify these points in our revised manuscript.

Reviewer 2: Review: I have 2 major problems with this paper. Either one by itself, I believe, is fatal, but taken together I cannot make any recommendation for this paper other than rejection.

Response: We are sorry that this reviewer did not, unfortunately, find our study scientifically meritorious, largely due to the fact that our research objective was not well

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understood by the reviewer. We hope our responses to her or his comments could help the reviewer to re-evaluate our manuscript.

Reviewer 2: Problem #1: There is a rich body of literature from the FACE experiments that claims, pretty much unequivocally, that nutrient cycling and/or limitation becomes more and more important to CO₂ fertilization as CO₂ concentrations rise. Yet, in this experiment CABLE is run with nutrient cycling turned off!

Response: We feel sorry that our research objective was not clearly conveyed to the reviewer. Our study was to examine how variability, as measured by coefficient of variation (CV), in the CO₂ fertilization effect (i.e., CV of β factor) changes from leaf to canopy GPP, ecosystem NPP and total carbon storage levels. Our study was not to quantify the CO₂ fertilization effect itself.

We agree with the reviewer that nutrient limitations are universally observed in experiments. Nutrient cycling influences the CO₂ fertilization effect. But running CABLE with coupled carbon, nitrogen and phosphorus cycles does not change the conclusion about CV of the CO₂ fertilization effects, which was previously reached by running carbon-only version of CABLE. Thus, we hope this reviewer will re-evaluate our manuscript, particularly with new simulations results from running the coupled CABLE-CN and CABLE-CNP models as presented below.

Reviewer 2: Coskun et al. (2016) and references therein has a nice summary of both Free-Air CO₂ Enrichment (FACE) as well as Open-Top Chamber (OTC) experiments. Smith et al. (2015) discusses the divergence between multiple models and a satellite-derived product that underscores the importance of the interaction between nutrient cycling and CO₂ fertilization. Many of these studies focus on N limitation, although some research has indicated that P limitation is a factor as well (e.g. Hasegawa et al., 2016). These, and other studies, all conclude that understanding of CO₂ fertilization requires taking nutrients into account.

Response: We thank the reviewer for showing us these important references. The

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reviewer cited a paper of Smith et al. (2015) and believed the overestimation of the CO₂ fertilization in those CMIP5 models is mainly caused by lack of nutrient limitations. But a related comment De Kauwe et al. (2016) suggests that it is still too premature to reach this conclusion because the nitrogen-incorporated model CESM1-BGC did not work well in simulating CO₂ uptake and response of NPP to elevated CO₂ (eCO₂) against FACE experimental results.

Again, our study was not to quantify the CO₂ fertilization effect itself but to understand what caused changes in CV of β factor. Running CABLE without or with nutrient limitation reached a similar conclusion as shown below.

Reviewer 2: I have to confess that I was very surprised when I read that the authors ran the version of CABLE without nutrient cycling included. I am not a FACE 'expert', but even I am aware of the amount of research that has concluded that nutrient cycling is critical to understanding ecosystem-level response to higher atmospheric CO₂. I found it very suspicious that nutrients were excluded from the study. Why, when there is this large body of work demonstrating the nutrient cycling is critical to understanding CO₂ enrichment, would nutrients be turned off in the model? The authors claim that nutrients were turned off for 'simplicity', but the obvious answer, and one that I suspect to be the truth, is that the authors did run CABLE with nutrient cycling, and model pathology and/or unrealistic results ensued.

Response: We thank the reviewer for the critical comments and his/her insistence on the necessity of nutrient-coupled simulations. We absolutely agree with the reviewer that the CO₂ fertilization effect (or β factor) could be more realistically represented with nutrient limitations considered. However, we feel sorry that the reviewer might not understand the purpose of our study properly. This study was designed to diagnose important model processes that cause divergence in β factor. The large uncertainty of β values remains a big challenge in CMIP5 models, many of which don't have carbon-nitrogen interactions. For the sake of diagnosis, we turned off the nutrient interactions to identify the most critical carbon-cycle processes in the CABLE model,

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which has not been attempted in previous studies. We found β factors at canopy and ecosystem levels in C-only simulation diverge in a way that is largely attributable to variations in LAI responses within and across C3 vegetation types in the CABLE model. Our new results from CABLE-CN (carbon-nitrogen) and CABLE-CNP (carbon-nitrogen-phosphorus) simulations suggest nutrient effects add more variations to β values at ecosystem level compared with C-only simulation (Fig. 1 in this response letter). However, the results from the CABLE-CN and CABLE-CNP simulations add more layers of complexity to understand the primary mechanisms underlying the divergence of β factors at different levels and in different ecosystems albeit the conclusion is similar with that reached from running carbon-only CABLE.

Besides, we feel the reviewer's conjecture: "and one that I suspect to be the truth, is that the authors did run CABLE with nutrient cycling, and model pathology and/or unrealistic results ensued" is too speculative, largely due to her/his incomplete understanding of our study. Per the suggestions from the two reviewers, we tested whether the patterns and mechanisms for the variability of β factors for C-only simulation still hold for nutrient-coupled simulations. We will add results from C-N and C-N-P coupled simulations of CABLE in the revised manuscript.

Until now, our new results from the C-N and C-N-P coupled simulations support our previous conclusions that at leaf-level β factors do not vary much for different vegetation types (Fig. 1 in this response letter). But at canopy and ecosystem levels, β factors diverge because the responses of LAI and nutrient limitations differentiate among vegetation types (Fig. 2 in this response letter). Please see more details about the mechanism of nutrient limitations in the CABLE model and our responses to the comments by Dr. De Kauwe.

Reviewer 2: It may have been possible to evaluate a nutrient run, even if the results were unrealistic, and evaluate how atmospheric CO₂ levels and nutrients interact in CABLE. The results may have provided an opportunity to evaluate or comment on the divergence of models in their predictions of atmospheric CO₂ levels and source/sink

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strength (e.g. Friedlingstein et al., 2006, 2014). By not including the critical nutrient interaction, I'm not sure that the results presented here give the reader any insight into how ecosystems might realistically respond to increasing future CO₂ levels in the atmosphere.

Response: We agree with the reviewer that CO₂ and nutrient interactions could cause the divergence of models. Our new results with CABLE-CN and CABLE-CNP show that CV of β is much higher than that with CABLE-C only for NPP and total carbon storage (Fig. 1 in this response letter). However, the objective of our study is not to evaluate nutrient effects on carbon cycle under CO₂ fertilization. As we have stated before, our study is to identify mechanisms underlying expanding CV from biochemical and leaf levels to canopy GPP, ecosystem NPP and carbon pool. All the three versions of CABLE point to the same mechanism, which is LAI as the major source of variability in modelled CO₂ fertilization.

Reviewer 2: Problem #2: Without carbon-climate feedbacks and nutrient cycles, I don't think a model actually has to be run to determine CO₂ fertilization. You can probably perform the calculation directly from the equations in the code. Between models there will be some differences: – Is the model an enzyme-kinetic model (Farquhar et al., 1980; Michaelis-Menten kinetics), or light-response (e.g. VPRM, Mahadevan et al., 2008)? – how is stomatal conductance calculated? Does it use Ball-Berry, with a dependence on relative humidity, or Leuning, which uses VPD? How is transpiration coupled to photosynthesis? – What are the parameter values for V_{cmax} for a given PFT? – What determines phenology? Is allocation static, or, if it is dynamic, how does it change during the year and in response to what? I believe it would be possible to determine the constraints on CO₂ fertilization for a suite of models without actually running any of them.

Response: We agree with the reviewer that these assumptions and processes are key to modelling terrestrial carbon-cycle responses to eCO₂. The reviewer is very knowledgeable to identify those key ecosystem carbon-cycle processes. In this comment

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alone, the reviewer mentioned more than 10 processes that influence photosynthesis. We were very curious how the reviewer could “perform the calculation directly from the equations in the code” to evaluate all those 10 processes and to gain a mechanistic understanding of what causes the change of β values. Obviously, we did not understand how to perform the calculation of β factors directly from the model equations as the reviewer suggested. The reviewer was apparently very confident for doing so. Even if we could calculate based on several equations, the results might not truly reflect model mechanisms for variabilities of the CO₂ fertilization effects within and across vegetation types. Because carbon-cycle processes are tightly coupled with radiation transfer, energy balance, nutrient interactions and water cycles in a land surface model. For example, leaf temperature and intercellular CO₂ concentration are two important variables for leaf-level β values, which are collectively controlled by air temperature, radiation transfer and humidity. We were not sure if the reviewer meant to construct a simplified model or emulator to mimic the complex land surface models, it is worthy trying but we were not confident that the simplified approach can reveal model mechanisms.

Nevertheless, we ran a well-evaluated land surface model and outputted process-level variables such as intercellular CO₂ concentration, LAI, GPP, NPP, and ecosystem carbon storage for all land cells, as many analyses have done based on C4MIP and CMIP5. Combining previous theoretical analysis, we have shown that CV of β is small for biochemical and leaf-level photosynthesis but large for canopy GPP, ecosystem NPP and carbon pools.

Reviewer 2: It is axiomatic that leaf-to-canopy scaling (LAI) is critical to total CO₂ fertilization amount. Every model that I am aware of calculates biophysics on a per-unit-area basis and then scales to the canopy level either by summing over sunlit/shaded leaves (and PFTs) or integrating from leaf to canopy scale along the lines of Sellers (1985, 1992)(OK, a gap model like ED2 may be a little different). Canopies with an LAI close to 1 (think of grasslands) will not see much difference from unit- to canopy-scale,

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more dense canopies (like forests) will.

Response: We are happy that the reviewer agrees with us that LAI is critical for plant productivity. Many models exhibit increasing LAI trends under CO₂ fertilization (Zhu et al., 2016). However, to what extent the increasing LAI feeds back to ecosystem response to eCO₂ is not clear. Our study for the first time calculated β factors from leaf biochemical level to ecosystem level, and found the LAI response to eCO₂ is the dominating factor for variabilities of the CO₂ fertilization effects at canopy and ecosystem levels within and across C3 vegetation types, namely the global CO₂ fertilization effects are very sensitive to the LAI responses. The value of our study is that it can urge modelling groups to improve the representation of LAI in land surface models, for example by calibrating allocation coefficients and specific leaf area (SLA) based on FACE experimental results (De Kauwe et al., 2014), so as to realistically predict concentration–carbon feedback.

Reviewer 2: If there is a large divergence between models in LAI (and GPP) for a given PFT, or if there is a large trend in one model's LAI for a given PFT during a climate run, then these might be valid topics of analysis. Finding that LAI is critical to canopy-level CO₂ fertilization (without nutrients being considered) does not really bring anything new to the field.

Response: Our results may not be much new for this reviewer but the key message from our study is still crucial for the community to improve land modeling. Actually, in our manuscript we have cited a paper showing CMIP5 models have simulated diverse GPP and LAI values for different regions during 1985-2006. And both GPP and LAI have been overestimated for most CMIP5 models according to observations (Anav et al., 2013) (Line 329-330). Satellite and modelled LAI both have experienced significant increasing trends during historical period as reported by Zhu et al. (2016). However, how the uncertainty and increasing trend of LAI contribute to modelled plant productivity and ecosystem carbon storage have not been discussed in previous research. Our study fills this gap and indicates the CO₂ fertilization effects are very sensitive to LAI

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responses. We agree with the reviewer that the CO₂ fertilization effects on GPP and LAI will be more realistically presented with nutrient limitations. Our new results show GPP and LAI still positively respond to eCO₂ under nutrient limitations but with reduced magnitudes. The responses of LAI still dominate the change of ecosystem responses across vegetation types in C-N and C-N-P coupled simulations (Fig. 2 in this response letter). The merit of our study is that we systematically diagnose model processes and find LAI is the most important factor in modelling the CO₂ fertilization effects, to which modelers should pay greater attentions and efforts in the future research.

Reviewer 2: Sunlit and shaded leaf partitioning is fairly well-constrained and sunlit LAI can never get much above 1 to 1.5 or so even under the most direct-sun conditions. Solar angle and leaf angle distribution make it possible to exceed an LAI value of one. I know that CLM has had issues with shade leaf LAI becoming excessively large. The authors do not discuss total LAI in CABLE during their fertilization runs, and this makes me suspicious-if their shade-leaf LAI is becoming unrealistically large, that might be a reason why fertilization strength decreases with time; increase in the amount of sunlit leaf may result in large change in GPP, but once sunlit LAI is filled, any additional canopy growth will be as shade LAI, and GPP increase will be attenuated.

Response: We appreciate the reviewer for the insightful comments. Actually, we did analyze total LAI change in the submitted supplementary material Fig. S6. LAI value of evergreen broadleaf forest increases with time but gradually saturates at the prescribed maximum value. LAI values of other plant types also increase but are far below the prescribed maximum values at 2100. To address the reviewer's concern about the magnitudes and changes of sunlit and shaded leaf LAI (we called the scaling factors in our manuscript according to the standard definition in the CABLE model), we plotted temporal trends of the scaling factors for sunlit leaf and shaded leaves in CABLE-C only simulation (Fig. 3 in this response letter). Results show that the magnitudes of the scaling factors for shaded leaves are greatly larger than those for sunlit leaves for all C3 plants. This is because in models it is usually defined that portion of sunlit leaves

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decreases exponentially with increasing LAI ($f_{\text{sun}} = \exp(-k_b \text{ LAI})$) (Dai et al., 2004). The scaling factors for sunlit leaves are below 1 as the reviewer stated. And the scaling factors for sunlit leaves of evergreen broadleaf forest, evergreen needleleaf forest and deciduous broadleaf forest gradually saturate with $e\text{CO}_2$.

The increasing portion of shaded leaves will lead to the attenuation of GPP increase as the reviewer mentioned. And we believe that saturation of GPP is jointly controlled by biochemical enzyme kinetics and canopy closure. In our submitted manuscript, we have stated that the saturation of GPP in response to $e\text{CO}_2$ is not only regulated by the leaf-level response, but also by the response of the scaling factors to $e\text{CO}_2$ (Fig. 6 in submitted manuscript; Line 324-325). The mechanisms for leaf-level saturation have been discussed in detail in Luo et al. (1996) and Luo and Mooney (1996).

Reviewer 2: I just don't think there's anything new here. Without nutrient cycling the CO_2 fertilization results don't have much meaningful application, and the fact that leaf-to-canopy scaling is important has been known for a long time.

Response: The reviewer's assertion about the scientific contributions of our study is partly due to her or his incomplete understanding of our research objective. We have run CABLE with coupled carbon-nitrogen-phosphorus cycles. Our original conclusion still stands. Although leaf-to-canopy scaling has been known for a long time, no study has done before as we did in this study to evaluate variation of β factors from biochemical and leaf levels to canopy, ecosystem scales. The leaf-to-canopy scaling is a basis of our study but the conclusion of our study goes far beyond it.

Here, we will strengthen our contributions through the following ways:

Analyzing the CO_2 fertilization effects at different levels with C-N and C-N-P interactions for different C_3 vegetation types in the CABLE model to evaluate whether our conclusions are still valid under nutrient limitations.

In the introduction and discussion part, we will clarify that it is the large uncertainty of

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concentration-carbon feedbacks produced by CMIP5 models that motivates our work. Our mechanistic study, for the first time, shows that β factors vary at different hierarchical levels across C-fluxes and stocks, and across PFTs in a way that is largely attributable to variations in LAI dynamics across PFTs at canopy and ecosystem levels. Our study can be useful by urging different modeling groups to quantify the CO₂ fertilization effects at different levels as we did and output leaf-level diagnostics for the next CMIP.

Our finding about the dominant role of LAI can stimulate modelling groups to focus more on uncertainty arising from processes related to LAI, and use FACE experiments to narrow the uncertainty of land model predictions.

Reviewer 2: Specific comments: “English prose and grammar, while readable, need attention. There are multiple places, too many to list, where errors exist.

Response: We will carefully revise the manuscript and improve the language in the revised version.

Reviewer 2: “There is no explanation for what eCO₂ is (elevated CO₂). Don't assume all your readers know the definition.

Response: We will add the following sentences in the first paragraph: “Persistent increase of atmospheric CO₂ concentration will stimulate plant growth and ecosystem carbon storage, forming a negative feedback to CO₂ concentration (Long et al., 2004; Friedlingstein et al., 2006). This concentration-carbon feedback (β), also called CO₂ fertilizing effect, has been identified as a major uncertainty in modeling terrestrial carbon-cycle response to historical climate change (Huntzinger et al., 2017)”.

Reviewer 2: “There is no definition of ‘gamma’ either.

Response: According to another reviewer's comments, we will remove γ -related contents in the revised manuscript.

Reviewer 2: “In many of the equations the equals sign is obscured. More effective

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spacing will make these equations easier to read

Response: Agree.

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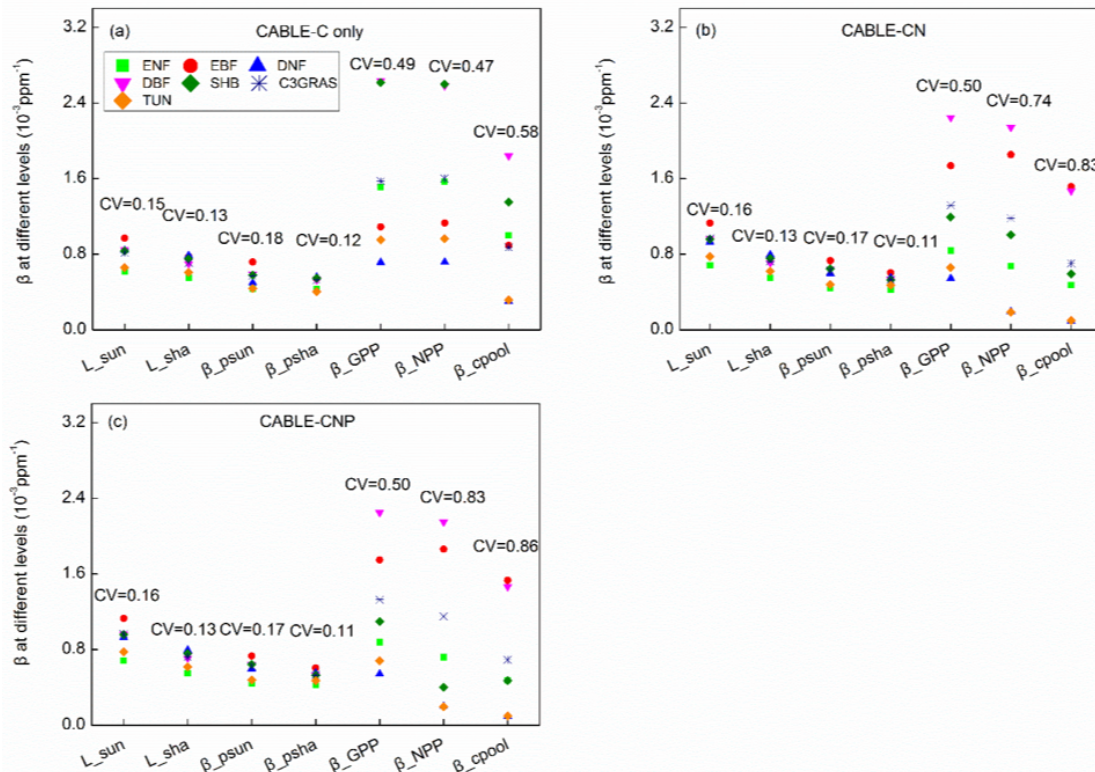


Fig. 1. β values at different levels for various C3 plants at the year 2023 from CABLE-C only(a), CABLE-CN (b) and CABLE-CNP (c) simulations.

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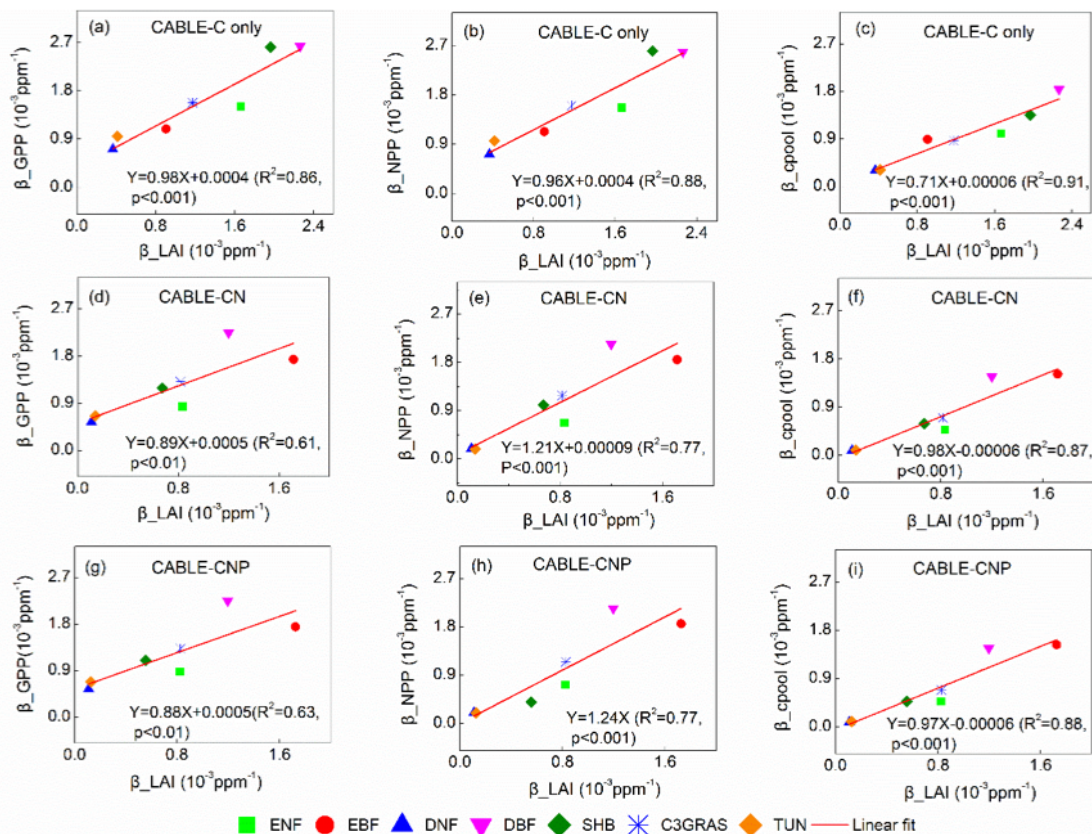


Fig. 2. Correlations between β_{GPP} and β_{LAI} , β_{NPP} and β_{LAI} , β_{cpool} and β_{LAI} from CABLE C-only (a)~(c), CABLE-CN (d)~(f) and CABLE-CNP (g)~(i) at the year 2023 across C3 plants.

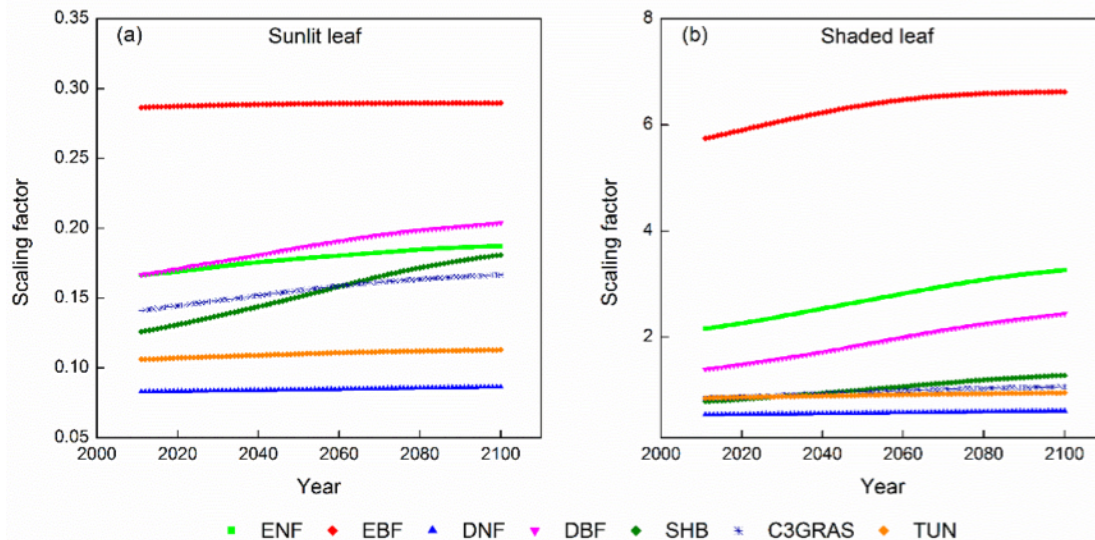


Fig. 3. Temporal trends of the leaf-to-canopy scaling factors for sunlit leaves and shaded leaves of different C3 vegetation types from the CABLE-C only model from 2011 to 2100.

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