Biogeosciences
Discussions

Open Access

EGU

Interactive comment on "Leaf Area Index identified as a major source of variability in modelled CO$_2$ fertilization" *by* Qianyu Li et al.

Qianyu Li et al.

lqy14@mails.tsinghua.edu.cn

Received and published: 21 August 2018

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 pervious conclusion with C only simulations. We will clarify the motivation and contribution

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 1: Li et al. use the CABLE model to explore the role of LAI in variability in the CO2 fertilisation response. The analysis has some interesting aspects which I'm sure will be of interest to the modelling community, in particular I thought fig 5 was interesting. However, I think the manuscript could be carefully revised for greater impact and insight. I have a number of specific points below but also 4 key issues with the analysis as presented:

Response: We thank the reviewer for the positive comments. His suggestions are very important for improving our manuscript.

Reviewer 1: 1. I don't understand the logic of using a model which simulates N and P cycles and then switching this functionality off to understand the CO2 fertilisation response? In my eyes, this is one of the great strengths of this model. So to not compare C against N and P, or C against N, is a missed opportunity. Whilst I'm realistic enough to envisage the authors won't rethink this strategy, I do feel this requires some further justification.

Response: We appreciate the reviewer's critical comments. This concern has been raised by two reviewers. The reason why we didn't originally include nitrogen and phosphorus cycles in our previous study is that we tried to find the most important factor causing the variations of β within and across different vegetation types with minimal confounding effects of other processes.

However, we totally agree with the reviewers that carbon-nutrient interactions should be considered when studying β effects. The respective effects of N and P can be calculated through the difference in the carbon uptake between C-N and C-only or C-N-P and C-N coupled simulations. Wang et al. (2012) and Zhang et al. (2013) provided details explaining how nutrient limitations are incorporated into carbon cycle in CASA-CNP

module in the CABLE model. In brief, NPP is calculated as: NPP=(GPP(L,V_cmax (N_l),J_max (N_l))-$\sum$ _iR_mi(N_i) − R_g(N_l/P_l)) ∗ x_npup(1)

Where L represents leaf area index, V_cmax and J_max are maximum carboxylation rate and maximum rate of electron transport of the top leaves, respectively, both are linearly dependent on leaf N (g N m-2) according to the relationships developed by Kattge et al. (2009) for different plant functional types. R_mi is maintenance respiration rates of plant tissue (i=leaf, wood and root) and contingent on nitrogen amount in each part of plant. R_g is growth respiration, which is described as a function of leaf nitrogen to phosphorus ratio. x_npup is the nutrient uptake limiting factor. x_npup will become less than 1 when the available nutrients (N or P) amount is less than the minimal amount of nutrient required by plants for a given NPP (Wang et al., 2010). Heterotrophic respiration (Rh) is limited by the mineral N pool required for microbial soil C decomposition (Wang et al., 2010). Net ecosystem productivity (NEP = GPP – Ra – Rh) is the amount of C that is either sequestered or lost from ecosystems, and is controlled by N and P availability via abovementioned C-N-P interactions.

Since effects of N and P on terrestrial carbon under CO2 fertilization in different regions in the CABLE model have been evaluated in Zhang et al. (2011), we will not elaborate on this point. Instead, we focus on the variations of β values at different scales across C3 vegetation types. The new results are plotted in Fig. 1b and Fig. 1c (in this response letter) to show β values at different levels for different vegetation types with carbon-nutrient interactions in this response letter. Our new results indicate that variations of β factors at leaf level in C-N and C-N-P coupled simulations are as small as that in C-only simulation, because the normalization will eliminate the influence of nitrogen-related V_cmax and J_max in calculating leaf-level response (Luo et al., 1996), and estimates of intercellular CO2 concentration (C_i) with nutrient limitations are comparable to those without nutrient limitations. β factors at canopy level (β_GPP) in C-N and C-N-P coupled simulations greatly diverge across vegetation types, which is similar to that with C-only simulation. However, unlike in C-only simulation, β_NPP

values are reduced for most C3 vegetation types and diverge more compared with β_GPP values in nutrient-coupled simulations, because the different nutrient-limiting effects on autotrophic respiration and plant growth as shown in Eq. (1) introduce additional variation across different vegetation types. Coefficients of variation of β_cpool in nutrient-coupled simulations exceed 0.8, larger than that in C-only simulation. It is noteworthy that in the current version of CABLE, P limitation is quite weak under present condition (Zhang et al., 2011). Therefore, the results of CABLE-CN are quite similar to those of CABLE-CNP.

We then found the linear relationships between β_GPP and β_LAI, β_NPP and β_LAI, β_cpool and β_LAI in C-only simulation (Fig. 2a~2c in this response letter) still hold in the C-N and C-N-P coupled simulations (Fig. 2d~2i in this response letter). From these results, our previous conclusion that LAI identified as a major source of variability in modelled CO2 fertilization is still valid under nutrient-limiting situations. We will add the above results into our revised manuscript.

Reviewer 1: It is stated that CABLE is largely RuBP-limited (line 179) and this point is given no further analysis. This is interesting and it isn't clear why this would be the case? Do the authors envisage that this is also true of other models? I would suggest it isn't but would be keen to read the authors thoughts on this. Surely this shapes the analysis (responsiveness to CO2)? So it warrants more than a single sentence that simply says "not shown" ...

Response: We thank the reviewer for this comment. It is an important prerequisite in our study. We agree it should be clarified in our manuscript. The formulation of leaf-level β factors depends on the intercellular CO2 concentration (Farquhar et al., 1980). Generally, photosynthesis rate is RuBP-regeneration limited (limited by light) when CO2 concentration exceeds a certain level. And we coded a variable indicating which process (Rubisco activity, RuBP regeneration or sink) limits photosynthesis rate at each running step in the original CABLE code. Then we outputted this variable. We found photosynthesis rates are almost all limited by RuBP-regeneration process

globally since 2011 when CO2 concentration is 391 ppm. Then leaf-level biochemical β factor can be expressed as an equation of intercellular CO2 concentration and CO2 compensation point. We didn't show the results because of the large volume of data (56560 model grids $\times$ 8760 hours in a year in total). We will clarify this in our revised manuscript.

Moreover, theoretical analysis by Luo and Mooney (1996) showed that leaf-level β values are similar for either Rubisco- or RuBP-limited photosynthesis. We will also add this point in the revised manuscript.

Reviewer 1: The paper is about CABLE but surely the aim is to make the result general (otherwise the title would have the word CABLE...)? However, I wonder if I was developing JULES or CLM, (etc) what my take home messages would be? The authors urge other modelling groups to repeat their analysis, but could they also make suggestions as to the implications for other modelling groups? How do these results help us to understand model responses to CO2? The CMIP5 concentration-carbon feedback factor?

Response: We thank the reviewer for the suggestion to highlight the take-home messages more clearly. In the introduction and discussion part, we will clarify that it is the large uncertainty of concentration-carbon feedbacks produced by CMIP5 models that motivates our work. To understand the source of uncertainty, Koven et al. (2015) found the large uncertainty of equilibrium terrestrial carbon change in response to elevated CO2 (eCO2) across the CMIP5 models is mainly caused by the variation of change in productivity, rather than by the variation of change in turnover time. Although the authors suggested that it probably results from the unrealistic representations of allocation and mortality processes in the current generation of models, in-depth understanding of what causes the divergent β_NPP across models will still be conducive to narrow the large uncertainty of β factors at ecosystem scale. Our study tried to understand β factors from a more mechanistic way than previous studies by analyzing CO2 fertilization from leaf biochemistry to ecosystem levels in a land surface model. Although our

analysis was conducted with CABLE, we believe the results will be applicable to other models because most land surface models employ Farquhar photosynthesis model to represent leaf biochemical response to eCO2. The biochemical properties of Farquhar photosynthesis model have determined that the basic responses of C3 plants to eCO2 under a certain CO2 concentration are almost constant (Luo et al., 1996; Luo & Mooney, 1996). However, the leaf-canopy scaling methods, allocation schemes, vegetation dynamics and soil modules among models are divergent (Arora et al., 2013; He et al., 2016). Our analysis shed new lights on mechanisms underlying model-model differences in estimated β factors and offers new diagnostics to be added in the next intermodal comparison project to help disaggregate the uncertainty of β_NPP. We will add the above discussions in the revised manuscript.

Reviewer 1: I didn't take much in the way of insight from the current section on this topic, i.e. section 4.3. For example, the authors assert that "It can be inferred that normalized leaf-level ÃřËŹI ËŹ ZÂą values would diverge little across different land surface models as long as they use ...". Is that true? If the models had different levels of water stress (which they almost always do) they would get very different values of Ci even with the same model assumptions. As the authors also show, leaf temperature affects gamma_star, so I see no reason to assume that models would predict similar leaf temperatures. Leaf temperature itself is dependent on a whole range of assumptions. I've never seen any evidence that models with different architectures, with different assumptions about leaf –to-boundary conductance, etc, would predict similar leaf temperatures. If the authors disagree they should support these assumptions. The authors cite the Hasegawa et al study as an example of a consistent result of their conclusion. But wouldn't a number of the other model CO2 paperls that point to marked divergence argue otherwise. My sense is their conclusion here is too simplistic.

Response: Thank the reviewer for pointing out this issue. We agree with the reviewer that different models have diverse levels of water stress on photosynthesis (De Kauwe et al., 2017). Water stress is applied to regulate stomatal conductance in many models

(Rogers et al., 2017; Wu et al., 2018). For example, the CABLE model represents water stress by an empirical relationship based on soil texture and limits the slope of the coupled relationship between photosynthesis rate and stomatal conductance (Eq. S11). The influence of water stress is reflected by intercellular CO2 concentration (C_i). Our results show modeled ratio of C_i to atmospheric CO2 concentration (C_a) is relatively constant for each PFT with eCO2 and varies little among PFTs. This modeling result is consistent with the concept of homeostatic regulations through photosynthetic rate and stomatal conductance (Pearcy & Ehleringer, 1984; Evans & Farquhar, 1991). Wong et al. (1979) showed plant stomata could maintain a constant C_i/C_a across wide range of environmental conditions, including water stress condition. Different models might have similar C_i for a given C_a but this assumption deserves further test. Moreover, Luo and Mooney (1996) found that changing C_i/C_a ratio from 0.6 to 0.8 caused less than 15% variation in sensitivity of leaf photosynthesis to a unit of increase in C_a, which will not affect our conclusion about LAI as a major source of uncertainty. We will add the above discussions into our revised manuscript.

It's also true that different model might simulate different leaf temperatures as the reviewer pointed out. Sensitivity analysis in previous study has shown that a ± 5âĎČ of leaf temperature changes caused approximately ± 7 ppm changes in Γ_*, leading to coefficient of variation (CV) of 0.12 to leaf-level β (Luo & Mooney, 1996). The overall variation of leaf-level β caused by variation in leaf temperature is still quite small compared with that of β_GPP.

Based on our literature review, only few studies like Hasegawa et al. (2017) have explored why different models simulated diverse responses of plant productivity to eCO2. We will greatly appreciate it if the reviewer can show us some related references.

Reviewer 1: The authors argue for the importance of LAI but don't really consider the role of allocation or turnover in great detail. Surely this is the key reason different models arrive at different LAI values? Even if you ignore changes in allocation/turnover due to CO2, this impacts on the scaling terms that the authors focus on.

Response: The reviewer made a great point. Changes in LAI are related to changes in allocation/turnover under eCO2. The response of allocation to eCO2 will influence β in two ways. The first way is through altering the portion of carbon allocated to leaf, then changing LAI, which we have discussed in Discussion 4.2 (Line 348-351). The second way is by changing the allocation pattern to plant organs with different lifespan, thereby altering carbon turnover time in plants and soil. It has been briefly discussed through the difference between β_NPP and β_cpool (Line 363-373). We will discuss more about the first way in the revised manuscript: "Second, diverse allocation schemes will influence the responses of LAI for different plants. And, results from two FACE (Duke Forest and Oak Ridge) experiments indicate that the carbon allocated to leaves is decreased and more carbon is allocated to woods or roots at higher CO2 concentration (De Kauwe et al., 2014). Unfortunately, CABLE has fixed allocation coefficients and likely overestimates LAI response, leading to overestimated responses of GPP, NPP and total carbon storage".

Reviewer 1: 4. The results are considered on a PFT level, but presumably they vary in interesting ways within a PFT (i.e. in space). Would this be worth showing or exploring further?

Response: We have analyzed within-PFT variations of β at different levels in Table 1, Results 3.3, and Fig. S1-S3 in the previously submitted manuscript.

Reviewer 1: Specific comments ================== - Line 43: Could you explain the CO2 fertilising effect further? The text as written expects the casual reader has significant background knowledge for the second sentence of your manuscript.

Response: Agree. 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 1: - Line 48: 4 or 4.5? What does that mean, do you mean 4 to 4.5? How can it be OR?

Response: Sorry for the ambiguity. Actually, the contribution of β is 4 times larger than that of carbon-climate feedback factor γ in Gregory et al. (2009) and Bonan and Levis (2010), but is 4.5 times larger in Arora et al. (2013). We will change this sentence to "Some studies pointed out that the contribution of β is 4 to 4.5 times larger, and more uncertain, than carbon-climate feedback factor (γ) (Gregory et al., 2009; Bonan & Levis, 2010; Arora et al., 2013)".

Reviewer 1: - Line 49: the reference to the Smith et al. paper ignores a technical comment on this paper: De Kauwe et al. (2016). Satellite based estimates underestimate the effect of CO2 fertilization on net primary productivity. Nature Climate Change, 6, 892-893. This is important as the authors are using this study to leverage their question. See also point on line 340.

Response: We thank the reviewer for pointing to the related comment paper by De Kauwe et al. (2016). It is indeed an important reference to supplement the point we were trying to make. We have modified the last sentence in the first paragraph to "Though satellite products they used may underestimate the effect of CO2 fertilization on net primary productivity (De Kauwe et al., 2016), the large disparity between models and FACE experiments gives us little confidence in making policies to combat global warming".

Reviewer 1: - Line 51: it isn't "reality" - the satellite estimates are also model estimates.

Response: Agree. See the response above.

Reviewer 1: - Line 54: "increasing temperature in models" why is temperature being introduced as a factor here? Isn't the focus solely on the CO2 fertilisation effect rather than the than carbon-climate feedback factor? There are further studies cited in this

paragraph which should be removed if the focus of this paper does not consider the carbon-climate feedback factor.

Response: Agree. We will remove the γ-related part in the revised manuscript.

Reviewer 1: - Line 67: Despite models using apparently similar photosynthesis models, Rogers et al. (A roadmap for improving the representation of photosynthesis in Earth system models. New Phytologist, 213, 22-42.) showed some important differences. It would be worthwhile highlighting this study in the context of the section of the text.

Response: We thank the reviewer for sharing us this important reference. We will adjust the sentence to a more accurate one: "The leaf-level CO_2 fertilization for C3 plants is generally well characterized with models from Farquhar et al. (1980), and the basic biochemical mechanisms have been adopted by most land surface models although some models implement variants of Farquhar et al. (1980) (Rogers et al., 2017)". We will discuss more about how those different implementations influence photosynthetic response in the Discussion: "Some models use variants of Farquhar photosynthesis model such as co-limitation approach described by Collatz et al. (1991). The absolute values of photosynthetic response to eCO_2 in these models are diverse mainly due to model divergence in inflection point from Rubisco- to RuBP- limited processes (Rogers et al., 2017). However, the relative photosynthetic responses will converge to a small range because the normalized photosynthetic response to eCO_2 only depends on estimates of intercellular CO_2 concentration (C_i), Michaelis-Menten constants (K_c, K_o) and CO_2 compensation point in the absence of day respiration (Γ_*), and relative leaf-level responses are similar for either Rubisco- or RuBP-limited photosynthesis (Luo et al., 1996; Luo & Mooney, 1996)".

Reviewer 1: - Line 72: what does carbon storage have to do with this sentence?

Response: Thanks for pointing out what we have missed. Besides NPP, allocation and carbon turnover process can influence carbon storage. We will change this sentence to "However, the CO_2 fertilization effects are considerably more variable at canopy- and

ecosystem-level than at the leaf-level, because a cascade of uncertain factors, such as soil moisture feedback (Fatichi et al., 2016), nutrient limitation (Zaehle et al., 2014), allocation (De Kauwe et al., 2014), and carbon turnover process (Friend et al., 2014) influence the responses of GPP, NPP and carbon storage".

Reviewer 1: - Line 76/7: seems a narrow characterisation of the literature, the De Kauwe et al. 2014 study that the authors cite, explored these issues in depth.

Response: We will add related references as the reviewer suggested: "Models generally predict that LAI dynamics will respond to eCO2 positively due to enhanced NPP and leaf biomass (De Kauwe et al., 2014). Zhu et al. (2016) has attributed global increases in satellite LAI primarily to increased CO2 concentrations. But how the increasing LAI in turn feeds back to ecosystem carbon uptake as a result of more light interception has not been discussed in previous research".

Reviewer 1: - Line 81: Why would a high "basic" (delete basic) NPP necessarily lead to tropical regions having the highest stimulation by CO2? Wouldn't the opposite be expected? These regions have a high LAI and so would predominantly be light-limited and so have a more limited capacity to respond to CO2? Either way, the authors need to expand on this assertion.

Response: We agree this sentence is not very clear. We are going to change the first sentence in this paragraph into "The largest absolute CO2 fertilization effect has been found in tropical area where already has the highest initial NPP (Joos et al., 2001; Peng et al., 2014). But with gradual eCO2, relative response in tropical area might not be very high owing to canopy closure (Norby et al., 2005)".

Reviewer 1: - Line 89: Improved on what?

Response: We will change this sentence to: "CABLE (version 2.0) is the Australian community land surface model (Kowalczyk et al., 2006) and incorporates CASA-CNP to simulate global carbon, nitrogen and phosphorus cycles (Wang et al., 2010; Wang

et al., 2011)".

Reviewer 1: - Line 124: The assumption that Jmax25 = 2 x Vcmax25. Did the authors consider varying this assumption? Other models would make quite different assumptions about this ratio.

Response: It's true that the ratio of the maximum electron transport rate ($J_{(max,25)}$) to maximum photosynthetic capacity ($V_{(cmax,25)}$) are different in models (Rogers et al., 2017). But difference of this ratio will not change the conclusion because β factors in our study are normalized values, irrespective of $J_{(max,25)}$ or $V_{(cmax,25)}$. In terms of the variation of this ratio due to eCO2, we have discussed the downregulation of $J_{(max,25)}$ and $V_{(cmax,25)}$ in the manuscript Line 308-313.

Reviewer 1: - Line 155: is there a citation, web link for "Community Climate System Model (CCSM) simulations"

Response: We will add a citation "Hurrell, J. W., Holland, M. M., Gent, P. R., Ghan, S., Kay, J. E., Kushner, P. J., ... & Lipscomb, W. H. 2013. The community earth system model: a framework for collaborative research. Bulletin of the American Meteorological Society, 94 1339-1360".

Reviewer 1: - Line 168: the definition of S (line 171) needs to be moved up to this line.

Response: Agree.

Reviewer 1: - Line 215: just to clarify when the authors say total carbon storage - do they mean the soils too? Or just the plant? Or just the foliage pool? The equation isn't very clear. This also makes Fig 1 hard for me to interpret as I'm unclear what is being shown, I'm going to assume it is total plant carbon...

Response: Total carbon storage is the sum of plant, litter and soil carbon pools. We'll make it clearer in the revised manuscript.

Reviewer 1: - Fig 1. Does it make sense to normalise these PFT lines? The authors

say they decline but the magnitudes differ, the point is that the initial starting points are different too. This makes it hard for the eye to gauge.

Response: Indeed, the CO2 fertilization effects at different levels in our manuscript are all normalized values. See Eq.19, 20, 23, 24, 27,28.

Reviewer 1: - As a general comment the results need work, particularly in terms of transition text. For example 3.1 talks about the temporal trend in Bcpool and then switches immediately to the Ci/Ca ratio in 3.2? It is hard to follow the logic of the transition, is there is meant to be any connection for the reader?

Response: Section 3.1 is about β factors at ecosystem level, showing β factors are diverging for different PFTs through time. It stimulates our following study that calculating β values from leaf biochemical level to canopy level in order to identify the key processes. We will add one transition sentence at the beginning of 3.2: "To reveal which processes cause the large disparity of β factors across vegetation types as shown in Fig. 1, we first compared biochemical parameters: intercellular CO2 concentration and CO2 compensation point, which are critical parameters for leaf-level biochemical response".

Reviewer 1: - What is the point of Fig. 2? It isn't clear what this figure has to do with the story of the paper?

Response: Please see the above response.

Reviewer 1: - The text around line 261 which refers to Fig 4 could do with further explanation. I personally don't find this particularly surprising, but the reader isn't offered much as the way of explanation. Presumably the change in slope as you move from B_GPP to B_NPP relates to respiration assumptions and then to B_cpool, allocation/turnover assumptions? I think the authors could go further in assisting the reader with interpretation. As currently written, the text simply highlights that the slope changes.

C13

Response: After thinking carefully about this concern, we agree that the slopes of the three fitting lines are not making much sense so we will remove this sentence in the revised manuscript.

Reviewer 1: - I think figure 5 is very interesting.

Response: Thank the reviewer for the positive comment.

Reviewer 1: - Line 290: I think this discussion of Fig S5 is interesting but I'm not sure I follow the interpretation? The LAI is the emergent outcome of the model assumptions - 1 leaf, 2leaf, multi-layer. Of course this assumption will lead to differences? But why you do the analysis on the leaf-level? Surely you're interested in the emergent outcome – the LAI. Most likely I simply misunderstood this point but I think it could also be explained further as it seems like an important point the authors are making.

Response: What we would like to discuss here is that CMIP5 model outputs have limited information for identifying mechanisms for model uncertainty since there are no leaf-level process outputs. We will reorganize the first paragraph in Section 4.1: "By contrast, CMIP5 model outputs have limited information in identifying mechanisms for model uncertainty since there are no leaf-level process outputs. In Hajima et al. (2014), they used GPP divided by LAI as a proxy to represent leaf-level photosynthesis for CMIP5 models. In our study, we also compared the sensitivities of GPP/LAI to eCO2 with our calculation of leaf-level β values which are derived from C_i and Γ_* for different vegetation types. Results from former calculation are greatly underestimated for trees and slightly overestimated for C3 grass and tundra (Fig. S5). The divergence of sensitivities of GPP/LAI across vegetation types is larger compared with that of our mechanistic calculation of leaf-level β. The bias is not only derived from the complex canopy structure used by each model (two-leaf or multiple-layer), but also from the nonlinear effect of LAI on GPP. Thus, the relatively large divergence of the sensitivities of GPP/LAI to eCO2 in Hajima et al. (2014) may not indicate diverse leaf-level photosynthesis responses among CMIP5 models. This comparison confirms the urgent

C14

need to include leaf-level diagnostics in the next intermodal comparison project".

Reviewer 1: - Line 295: I don't fully follow that interpretation? Your differences in Ci/Ca were small across PFTs? And the differences in leaf temp would be expected between PFTs? Certainly, fig 2 doesn't show any within PFT variation.

Response: Yes, we think the reviewer's understanding is correct. We would like to express that the leaf-level ðİŻ¡ computed in our study can be mechanistically traced back to intercellular CO2 concentration and leaf temperature. Since Fig. 2 shows the results across different PFTs, we'll change this sentence to: "Another advantage of our calculation of leaf-level ðİŻ¡ is that the reason for the divergence of leaf-level ðİŻ¡ across vegetation types can be traced back to differences in ðİŘűðİŚŰ and leaf temperature as shown in Fig. 2".

Reviewer 1: - Line 362: This is an assumption of the model and might not necessarily be true!

Response: Agree. We will add the following sentences in the manuscript: "FACE experimental results indicate that CUE values under eCO2 are not changed in N-limited Duke site (Hamilton et al., 2002; Schäfer et al., 2003), increase in fertile POPFACE site (Gielen et al., 2005) or decrease in fertile ORNL site (DeLucia et al., 2005). Thus, representations of nutrient limitations on GPP and autotrophic respiration in land surface models should be carefully calibrated with experimental data (DeLucia et al., 2007)".

Reviewer 1: Technical corrections =====================

- Abstract: "vegetation types is 0.15-0.13", presumably you meant 0.13 to 0.15? Also, why don't the other variables (e.g. BetaGPP) have ranges too?

Response: Yes, we meant 0.13 and 0.15 for shaded leaf and sunlit leaf, respectively. At canopy level, we did not differentiate sunlit leaves and shaded leaves, so there is only one value for β_GPP.

Reviewer 1: - First line of the introduction, makes no sense. You can't start a sentence

with Terrestrial carbon sink and then a comma.

Response: Agree. We will change the first sentence to: "Terrestrial ecosystems take up roughly 30% of anthropogenic CO2 emissions, and is of great uncertainty and vulnerable to global climate change (Cox et al., 2000; Le Quéré et al., 2017)".

Reviewer 1: - Line 45: In Coupled -> In the Coupled

Response: Agree.

Reviewer 1: - Line 138: In CABLE model -> in the cable model

Response: Agree.

References

Arora, V. K., Boer, G. J., Friedlingstein, P., Eby, M., Jones, C. D., Christian, J. R., ... & Hajima, T. (2013). Carbon–concentration and carbon–climate feedbacks in CMIP5 Earth system models. Journal of Climate, 26(15), 5289-5314.

De Kauwe, M. G., Medlyn, B. E., Walker, A. P., Zaehle, S., Asao, S., Guenet, B., ... & Lu, X. (2017). Challenging terrestrial biosphere models with data from the longâĂŘterm multifactor Prairie Heating and CO2 Enrichment experiment. Global change biology, 23(9), 3623-3645.

DeLucia, E. H., Moore, D. J., & Norby, R. J. (2005). Contrasting responses of forest ecosystems to rising atmospheric CO2: implications for the global C cycle. Global Biogeochemical Cycles, 19(3).

DeLucia, E. V. A. N., Drake, J. E., Thomas, R. B., & GONZALEZâĂŘMELER, M. I. Q. U. E. L. (2007). Forest carbon use efficiency: is respiration a constant fraction of gross primary production? Global Change Biology, 13(6), 1157-1167.

Evans, J.R., Farquhar, G. D. (1991). Modeling canopy photosynthesis from the biochemistry of the C3 chloroplast. Modeling crop photosynthesisâĂŤfrom biochemistry

to canopy, (modelingcroppho), 1-15.

Fatichi, S., Leuzinger, S., Paschalis, A., Langley, J. A., Barraclough, A. D., & Hovenden, M. J. (2016). Partitioning direct and indirect effects reveals the response of water-limited ecosystems to elevated CO2. Proceedings of the National Academy of Sciences, 113(45), 12757-12762.

Friedlingstein, P., Cox, P., Betts, R., Bopp, L., von Bloh, W., Brovkin, V., ... & Bala, G. (2006). Climate–carbon cycle feedback analysis: results from the C4MIP model intercomparison. Journal of climate, 19(14), 3337-3353.

Gielen BC, Calfapietra C, Lukac M, Wittig VE, DeAngelis P, Janssens IA, Moscatelli MC, Grego S, Cotrufo MF, Godbold DL, Hoosbeek MR, Long SP, Miglietta F, Polle A, Bernacchi CJ, Davey PA, Ceulemans R, Scarascia-Mugnozza GE (2005). Net carbon storage in a poplar plantation (POPFACE) after three years of free-air CO2 enrichment. Tree Physiology, 25, 1399-1408.

Hamilton, J. G., DeLucia, E. H., George, K., Naidu, S. L., Finzi, A. C., & Schlesinger, W. H. (2002). Forest carbon balance under elevated CO 2. Oecologia, 131(2), 250-260.

He, Y., Trumbore, S. E., Torn, M. S., Harden, J. W., Vaughn, L. J., Allison, S. D., & Randerson, J. T. (2016). Radiocarbon constraints imply reduced carbon uptake by soils during the 21st century. Science, 353(6306), 1419-1424.

Huntzinger, D. N., Michalak, A. M., Schwalm, C., Ciais, P., King, A. W., Fang, Y., ... & Hayes, D. (2017). Uncertainty in the response of terrestrial carbon sink to environmental drivers undermines carbon-climate feedback predictions. Scientific Reports, 7(1), 4765.

Kattge, J., Knorr, W., Raddatz, T., & Wirth, C. (2009). Quantifying photosynthetic capacity and its relationship to leaf nitrogen content for globalâĂŘscale terrestrial biosphere models. Global Change Biology, 15(4), 976-991.

Koven, C. D., Chambers, J. Q., Georgiou, K., Knox, R., Negron-Juarez, R., Riley, W.

J., ... & Jones, C. D. (2015). Controls on terrestrial carbon feedbacks by productivity versus turnover in the CMIP5 Earth System Models.

Long, S. P., Ainsworth, E. A., Leakey, A. D., Nösberger, J., & Ort, D. R. (2006). Food for thought: lower-than-expected crop yield stimulation with rising CO2 concentrations. Science, 312(5782), 1918-1921.

Luo, Y., Sims, D. A., Thomas, R. B., Tissue, D. T., & Ball, J. T. (1996). Sensitivity of leaf photosynthesis to CO2 concentration is an invariant function for C3 plants: A test with experimental data and global applications. Global Biogeochemical Cycles, 10(2), 209-222.

Luo, Y., & Mooney, H. A. (1996). Stimulation of global photosynthetic carbon influx by an increase in atmospheric carbon dioxide concentration. In Carbon dioxide and terrestrial ecosystems (pp. 381-397).

Norby, R. J., DeLucia, E. H., Gielen, B., Calfapietra, C., Giardina, C. P., King, J. S., ... & De Angelis, P. (2005). Forest response to elevated CO2 is conserved across a broad range of productivity. Proceedings of the National Academy of Sciences, 102(50), 18052-18056.

Pearcy, R. W., & Ehleringer, J. (1984). Comparative ecophysiology of C3 and C4 plants. Plant, Cell & Environment, 7(1), 1-13.

Schäfer, K. V., Oren, R., Ellsworth, D. S., Lai, C. T., Herrick, J. D., Finzi, A. C., ... & Katul, G. G. (2003). Exposure to an enriched CO2 atmosphere alters carbon assimilation and allocation in a pine forest ecosystem. Global Change Biology, 9(10), 1378-1400.

Wang, Y. P., Law, R. M., & Pak, B. (2010). A global model of carbon, nitrogen and phosphorus cycles for the terrestrial biosphere. Biogeosciences, 7(7), 2261-2282.

Wang, Y. P., Lu, X. J., Wright, I. J., Dai, Y. J., Rayner, P. J., & Reich, P. B. (2012). Correlations among leaf traits provide a significant constraint on the estimate of global gross primary production. Geophysical Research Letters, 39(19).

Wong, S. C., Cowan, I. R., & Farquhar, G. D. (1979). Stomatal conductance correlates with photosynthetic capacity. Nature, 282(5737), 424.

Wu, D., Ciais, P., Viovy, N., & Vicca, S. (2018). Asymmetric responses of primary productivity to altered precipitation simulated by ecosystem models across three long-term grassland sites. Biogeosciences, 15, 3421-3437.

Zhang, Q., Wang, Y. P., Pitman, A. J., & Dai, Y. J. (2011). Limitations of nitrogen and phosphorous on the terrestrial carbon uptake in the 20th century. Geophysical Research Letters, 38(22).

Zhang, Q., Pitman, A. J., Wang, Y. P., Dai, Y. J., & Lawrence, P. J. (2013). The impact of nitrogen and phosphorous limitation on the estimated terrestrial carbon balance and warming of land use change over the last 156 yr. Earth System Dynamics, 4(2), 333-345.

Zhu, Z., Piao, S., Myneni, R. B., Huang, M., Zeng, Z., Canadell, J. G., ... & Cao, C. (2016). Greening of the Earth and its drivers. Nature Climate Change, 6(8), 791-795.
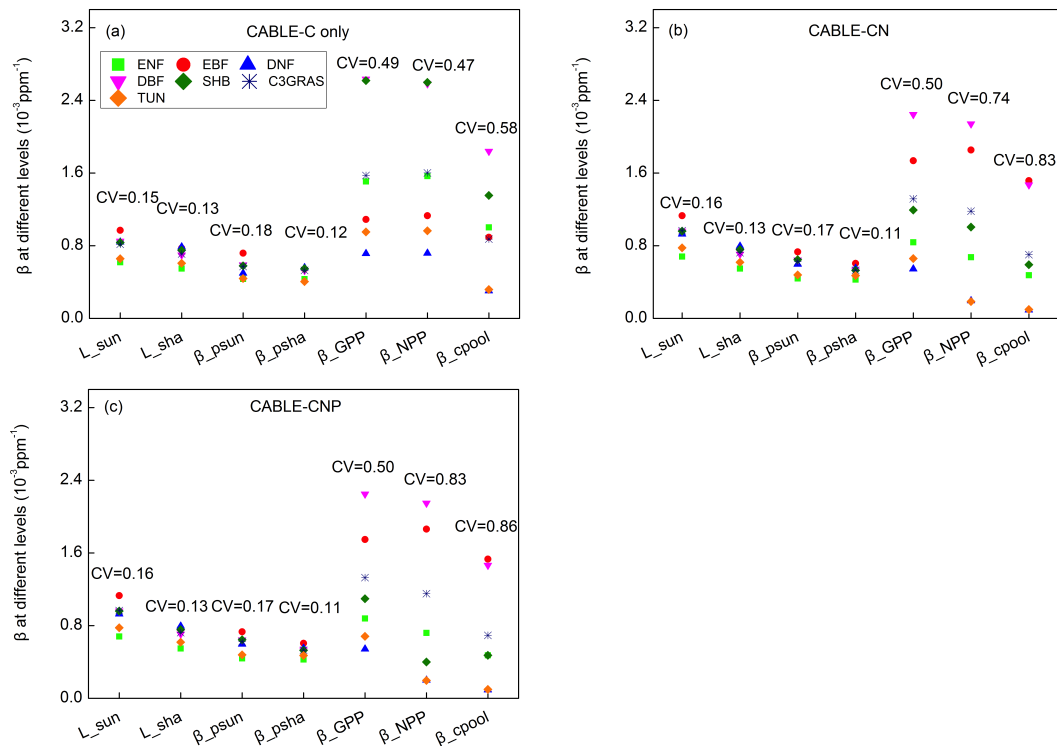

Fig. 1. β values at different levels for various C3 plants at the year 2023 from CABLE-C only(a), CABLE-CN (b) and CABLE-CNP (c) simulations.

C20
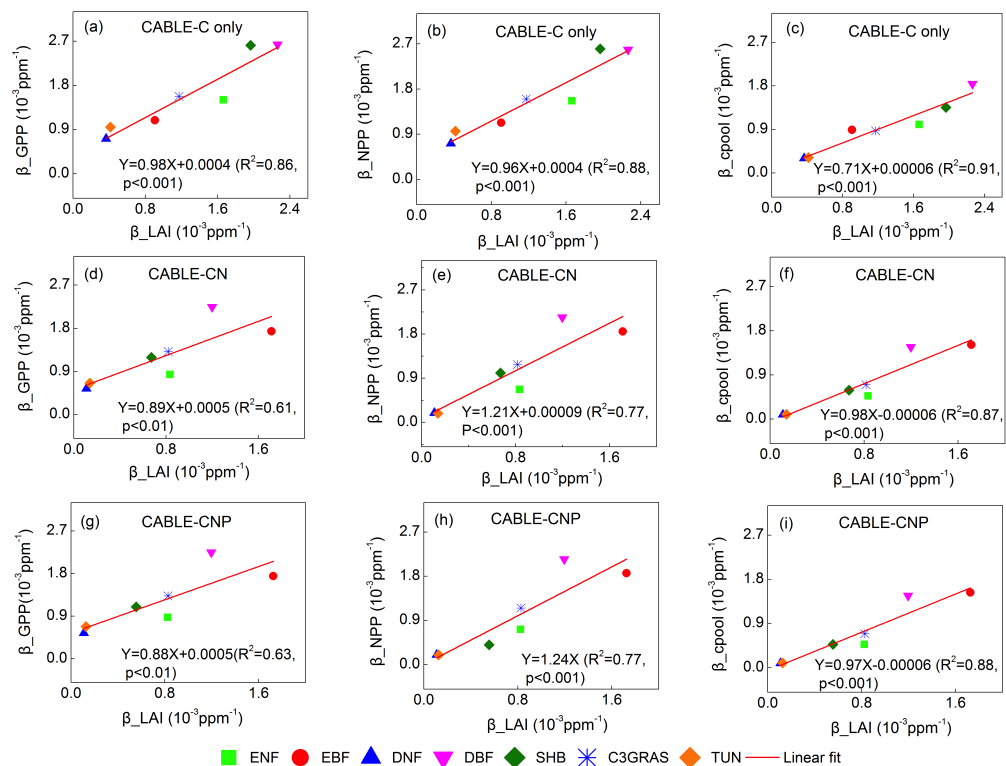
Fig. 2. Correlations between β_GPP and β_LAI, β_NPP and β_LAI, β_cpool and β_LAI from CABLE C-only (a)$\sim$(c), CABLE-CN (d)$\sim$(f) and CABLE-CNP (g)$\sim$(i) at the year 2023 across C3 plants.