Response to Anonymous Referee #1

General comments:

The authors successfully addressed some of the issues raised in the last round of review and have substantially improved the structure as well as the language of the manuscript. Although I believe the manuscript can still benefit greatly from carefully revising the text for readability, logic, and consistency.

Reply: Thank you very much for your encouragement and comments.

Main concerns:

- Regarding some of the main concerns for the previous manuscript version: ***1."The framework implemented in the study revolves around processes in terrestrial N cycles, more specifically about plant N demand and stress. However, the relevant processes are overly simplified when describing the necessity to modify current representations in the models."*** In introduction, there is still the same issue that too much text is focused on the disadvantage of C-only models, while the focus should be the limitation of C-N models with fixed CNR. Especially that after talking about ecosystem N processes that regulate C cycles, it circled back to only a small portion of CMIP6 models include N cycles. As such, the paragraph mainly stresses out that C-only models cannot account for N limitations being problematic in potential overestimation of terrestrial C sequestration,

whereas the performance of C-N models with fixed CNR is not mentioned at all. This creates an obvious logic gap between "C-only models does not consider N limitations" and "this new coupling framework reduces the impact of N limitations which is an advantage (e.g., lines 775ff)". Why does the impact of N limitations need to be reduced in the first place?

Reply: Thank you for the comments/suggestions!

We would like to clarify here:

(1). How to properly introduce the N process to the C-only model is still outstanding research, as demonstrated in recently published papers, such as Davies-Barnard et al., 2020 and Kou-Giesbrecht et al. 2023, which claim that "a disconnect between the carbon and nitrogen cycles" is a major issue in current terrestrial biosphere modeling.

(2). The objective of this study is to improve C–N coupling in the Earth System Model, as indicated in the paper's introduction: *"presents a recently developed process-based plant C–N coupling framework with a consistent coupling strategy between biophysical and biogeochemical processes. The framework mainly focuses on the effects of N limitation on plant photosynthesis (Section 2.2.3), plant respiration (Section 2.2.4), and plant phenology (Section 2.2.5) with a dynamic C/N ratio (CNR, Section 2.2.2). The dynamic plant CNR is a more realistic representation than the fixed plant CNR in assessing the effect of N limitation on plant C processes and interactions between plant C and N processes."* (***new lines 69-73***)

As such, “reducing the impact of N limitation” is not a subject of this study. The quoted word “advantage” in line 775ff is not used for “reduced N limitation effect” but refers to our approach “does not linearly or instantaneously respond to the available N content”. Sorry, our statements in lines 775ff caused confusion. We have revised this paragraph in the “Summary” section (previous Discussion and Conclusion section) to avoid confusion. “Advantage” is a subjective assessment. To avoid controversy, we deleted the sentence with “advantage”.

(3). In Kou-Giesbrecht et al. (2023) and other recent C-N model intercomparison papers, various configurations in these coupled models are listed (see the attached tables below). Among the many coupling components, one item is whether to use the use of a fixed CNR or flexible CNR. These papers did not conclude whether “fixed” or “flexible” CNRs should be used or whether models with flexible CNRs are required to test fixed CNRs first. In our paper, we emphasize that the flexible CNR approach more realistically represents the ecological process. It is not our task to prove that a model with a fixed CNR cannot properly simulate coupling. To test the fixed CNR in our model, it is not simple to set the CNR as a constant but must change many other parameterizations, which actually requires setting a new coupling framework and is beyond the scope of this paper.

(4). The reviewer questions why “the impact of N limitations need to be

reduced in the first place". As discussed above, this is never a subject in our study. Our goal is to introduce a more realistic C–N coupling process. In this paper, we indicate that a flexible CNR reduces the N limitation effect compared with a fixed CNR. This finding does not mean our goal is to reduce the N-limitation effect through flexible CNR.

Another reviewer of this paper suggested that we add N deposition in future studies, which will reduce the N limitation effect in some areas. Apparently, that reviewer's suggestion is to include a more realistic process; reducing the N limitation is not the purpose of adding N deposition.

To avoid any confusion, we have deleted "reduce the N-limitation effect" in the paragraph in the Summary section. The paragraph has been revised as follows (***new lines 634-643***):

"The new C-N coupling framework takes a consistent coupling strategy between biophysical and biogeochemical processes and mainly focuses on the effects of N limitation on plant photosynthesis, plant respiration, and plant phenology. The dynamic plant CNR is used to represent plant resistance and response to N stress, which allows adaptations in the stoichiometry of C and N. This approach increases nutrient use efficiency and takes into account N remobilization and resorption; the N limitation effect does not linearly or instantaneously respond to the available N content. A linear relationship between the N limitation factor and available N is valid only when N availability is not sufficient for the minimum N

demand for new growth."

Table 1. Key nitrogen cycle algorithms applied by the models. C is Carbon; N is Nitrogen; GPP is gross primary productivity; NPP is net primary productivity; and PFT is plant functional type.

	CLM4.5	CLM5	JSBACH	JULES-ES	LPJ-GUESS
Key references	Oleson et al. (2010)	Lawrence et al. (2019)	Goll et al. (2017), Mauritsen et al. (2019)	Wiltshire et al. (2020)	Smith et al. (2014)
N effect on GPP	Downregulation of GPP to match stoichiometric constraint from allocable N	Leaf N compartmentalised into different pools to co-regulate photosynthesis according to the LUNA model	No direct effect	No direct effect	Reduction of Rubisco capacity in the case of N stress
N effect on autotrophic respiration	N content-dependent tissue-level maintenance respiration	Updated PFT-specific N-dependent leaf respiration scheme	No direct effect	N content-dependent maintenance respiration for roots and stems	N content-dependent maintenance respiration for roots and stems; leaf respiration reduced under N stress
Vegetation pool C : N stoichiometry	Fixed for all pools	Flexible for all pools	Fixed for all pools except labile	Flexible leaf stoichiometry from which root and stem C : N are scaled with fixed fractions	Flexible for leaves and fine roots; fixed otherwise
Retranslocation of N from shed leaves	Fraction of leaf N moved to mobile plant N pool prior to shedding; fraction depends on PFT-specific fixed live leaf and leaf litter C : N ratios	Fraction of leaf N moved to mobile plant N prior to shedding via two pathways: a free retranslocation or a paid-for retranslocation dependent on PFT-specific dynamic leaf C : N range and minimum leaf litter C : N as well as available carbon to spend for extraction in the FUN model	Fraction of leaf N moved to mobile plant N pool prior to shedding	Fraction of leaf N moved to labile store with PFT-specific retranslocation coefficient	Fraction of leaf N moved to mobile plant N pool prior to shedding; fraction depends on N stress
Biological N fixation	Monotonically increasing function of NPP	Symbiotic N fixation according to the FUN model; asymbiotic N fixation linearly dependent on evapotranspiration	Non-linear function of NPP	Linear function of NPP, 0.0016 kg N per kg C NPP	Linear function of ecosystem evapotranspiration, 0.102 mm yr^{-1} ET + 0.524 per kg N ha^{-1} yr^{-1}

Table in T. Davies-Barnard, et al. 2020

Table 1. Terrestrial biosphere models in the TRENDY-N ensemble and descriptions of their representations of N limitation of vegetation growth, biological N fixation, vegetation response to N limitation (i.e., strategies in which vegetation invests C to increase N supply in N-limited conditions), and N limitation of decomposition.

	Reference	N limitation of vegetation growth	Biological N fixation	Vegetation response to N limitation	N limitation of decomposition
CABLE-POP	Haverd et al. (2018)	$V_{cmax} = f$(vegetation N) Flexible C : N stoichiometry	Time invariant	Static	N invariant
CLASSIC	Melton et al. (2020)	$V_{cmax} = f$(vegetation N) Flexible C : N stoichiometry	f(N limitation of vegetation growth)	Dynamic (biological N fixation)	N invariant
CLM5.0	Lawrence et al. (2019)	$V_{cmax} = f$(vegetation N) Flexible C : N stoichiometry	f(N limitation of vegetation growth)	Dynamic (biological N fixation, mycorrhizae, re-translocation)	f(soil N)
DLEM	Tian et al. (2015)	GPP = f(vegetation N)	f(soil temperature, soil moisture, soil C, soil N)	Dynamic (root allocation)	f(soil N)
ISAM	Shu et al. (2020)	GPP = f(vegetation N)	f(ET)	Static	f(soil N)
JSBACH	Reick et al. (2021)	NPP = f(vegetation N)	f(NPP)	Static	f(soil N)
JULES-ES	Wiltshire et al. (2021)	NPP = f(vegetation N)	f(NPP)	Static	f(soil N)
LPJ-GUESS	Smith et al. (2014)	$V_{cmax} = f$(vegetation N) Flexible C : N stoichiometry	f(ET)	Dynamic (root allocation)	N invariant
LPX-Bern	Lienert and Joos (2018)	NPP = f(vegetation N)	Derived post hoc to simulate a closed N cycle	Static	N invariant
OCNv2	Zaehle and Friend (2010)	$V_{cmax} = f$(vegetation N) Flexible C : N stoichiometry	f(N limitation of vegetation growth)	Dynamic (root allocation)	f(soil N)
ORCHIDEEv3	Vuichard et al. (2019)	$V_{cmax} = f$(vegetation N) Flexible C : N stoichiometry	Time invariant	Static	N invariant

Table in Kou-Giesbrecht, S., et al. 2023

2.“The need to evaluate plant C processes under the modified N processes is well motivated in the introduction. However, the connection between N processes and heat fluxes is absent”, I intended to remind adding some information on how ecosystem N processes interact with heat fluxes and why it is important to look at these variables (as how terrestrial C sink hinges on ecosystem N processes). It remains missing in the introduction and discussion as the first time “heat flux” is brought up is in Methods while being a main part of the Results. I would suggest adding a few sentences in the end of the introduction justifying the choice of all the variables.

Reply:

The reviewer raises a very important point here. We agree with the reviewer that the impact on heat flux is important! We de-emphasized this in this paper because the offline experiment did not have a significant impact on the heat flux. Per the reviewer’s suggestion, we added a few sentences to justify our variable selection in this paper.

New Lines 90-94: *“In addition, the effects of N limitation on heat fluxes are also preliminary assessed with station data (Section 4.1). The results indicate that because the atmospheric forcings (such as downward radiation) in our offline experiment are the same for both the control and sensitivity runs, the heat flux response due to N limitation is limited. In this paper, we mainly focus on the GPP and LAI. A comprehensive assessment*

of the effect of N limitation on heat fluxes and atmospheric circulation needs to be conducted in a fully coupled atmosphere–land model."

3. It is nice to see how including more dynamic N processes mostly brings modelling results closer to the observations at global as well as site levels. However, it is curious that the amplitude of mean seasonality of GPP (Figure 10) is much dampened with NIPSN and SSiB5 compared to SSiB4 which seems closer to the observations. In this sense, instead of the claim of "improvement in the simulation of the seasonal cycle in SSiB5 (lines 672ff)", it only shows that mean monthly GPP is improved to different extents by months. This result should be explained potentially together with the changes in spatial patterns Figure 8 and Table 7. See the following studies on comparing modelling and observations for seasonality or seasonal biases of GPP:

- Lin S, Hu Z, Wang Y, Chen X, He B, Song Z, Sun S, Wu C, Zheng Y, Xia X, et al. 2023. Underestimated Interannual Variability of Terrestrial Vegetation Production by Terrestrial Ecosystem Models. Global Biogeochemical Cycles 37(4): e2023GB007696.

- MacBean N, Scott RL, Biederman JA, Peylin P, Kolb T, Litvak ME, Krishnan P, Meyers TP, Arora VK, Bastrikov V, et al. 2021. Dynamic global vegetation models underestimate net CO2 flux mean and inter-annual variability in dryland ecosystems. Environmental Research Letters

16(9)

Reply: Thank you for pointing out the seasonality issues.

After a more careful evaluation, we realized that averaging seasonality ***globally*** can be misleading due to the opposite seasonal patterns in the Northern (NH) and Southern Hemispheres (SH). Therefore, we have redrawn the seasonality separately for the NH and SH in Figures 10 and 11 and modified the discussion. In this analysis, we excluded high-latitude regions (50°N-60°N) due to less reliable satellite data records (Gonsamo et al., 2019) to ensure a more proper comparison. After those modifications, the simulated seasonality in our model runs showed a general consistency with the satellite products.

In ***new lines 532-539***, we added the following discussion:

"Furthermore, the interannual variability and annual cycle are also assessed. The correlation for interannual variability (Fig. 10a) in SSiB4 is already very high (0.98). SSiB5 continues keeping the high correlation as SSiB4. However, the standard deviations for the observations of SSiB4 and SSiB5 are 14.7, 26.7, and 19.9, respectively. SSiB5 is closer to the observations. The underestimation of interannual variability in terrestrial vegetation production by terrestrial ecosystem models (Lin et al., 2023: MacBean et al., 2021) does not appear in this study. The temporal correlation coefficients between the observed and simulated monthly mean GPPs for the Northern and Southern Hemispheres increased from

0.73/0.50 (Exp. SSiB4) to 0.75/0.55 (Exp. SSiB5), respectively (Figs. 10b and c), showing improvement in the simulation of the seasonal cycle in SSiB5".

4. The shift in regional GPP biases to negative by SSiB5 shown in Table 7 requires more description and explanation in discussion which is largely omitted (e.g., lines 668ff, 786ff). It might be too much work at this point, however I wonder if it is possible to include some maps for NPP and autotropic aspiration (SSiB4 vs SSiB5) to show how to attribute the improvement in GPP. NPP and respiration (please specify autotropic, heterotrophic, or both) are also mentioned in the discussion without presenting any data (line 794). Although NlPSN, NlResp, and NlPhen are showing the effects of each process, the interactive effects on NPP and autotropic aspiration may provide some information on biases in spatial patterns and seasonality of GPP.

Reply:

The reviewer requests a more in-depth discussion on the spatial patterns and seasonality of GPP biases and the role that each process plays. At the end of the review (question for Lines 785ff), the reviewer also raises the significant test issue.

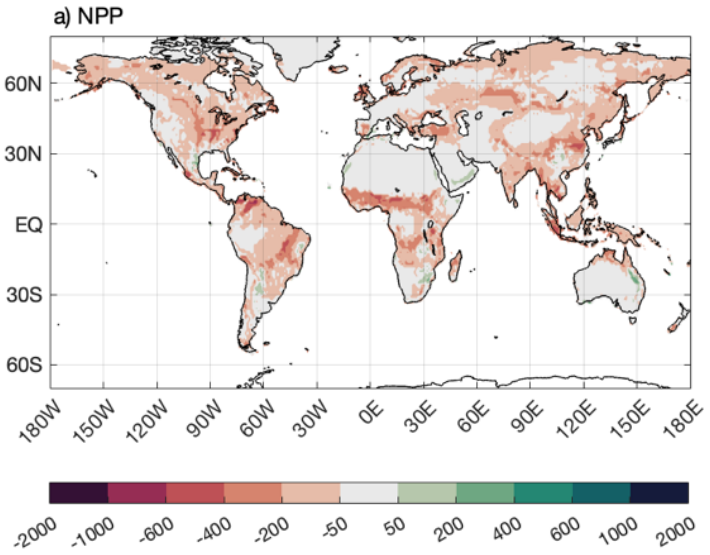
We apologize. In the previous version, the significance test was not clearly addressed. In the revised paper, in ***new lines 470-471***, we add a statement

that *"the improvement in the SSiB5 model bias compared to SSiB4 that are presented in Table 6, are all statistically significant at the α = 0.05 level of the t test values."*. We also add note in Tables 7 and 8 for the results' statistical significant.

This paper mainly presents the climatological results from model development. Only Figures 10 and 11 very briefly show the corrected seasonality results. As indicated in the last review, per the editor's instructions, the current paper version is focused mainly on describing model development. The discussion on scientific issues is not the subject of this paper. As such, in this submission, we only include basic validation for model development (some discussion on scientific issues has been removed in this submission). This paper already has 13 figures plus 8 tables. Any comprehensive discussion on seasonality and more detailed roles for each process is beyond the scope of this paper.

Since the reviewer wants to see the NPP figure, we have attached the difference in the NPP between SSiB5 and SSiB4 for reference. This result is generally consistent with that difference in GPP. For respiration, as indicated in the last review that, although the differences between SSiB5 and SSiB4 and between NIPSN and SSiB4 are statistically significant. The differences between the total respiration effect and that of SSiB4 were not significant. Therefore, we only present the results for SSiB5-SSiB4, NIPSN-SSiB4, and (SSiB5-NIPSN)-SSiB4, which shows the effect of

transpiration plus phenology. The individual respiration results, therefore, are not discussed separately.



I think it is great that the discussion has been expanded to additional N input. However I found a conclusive statement missing towards the end of the manuscript.

Reply:

Thank you for the suggestion. This issue is rooted in the model structure. We now separate the "Discussion and Conclusion" to the "Discussion" section and the Summary section. We have added the following statement at the end of the Summary section.

New Lines 656-661. *"Although significant progress has been made in recent years in incorporating the N cycle and its effect on the C cycle in the terrestrial biosphere in a number of ESM LSMs (with various representations of N processes), our and other relevant studies suggest that*

there are still many important outstanding issues, some of which were discussed in Section 5, and further efforts in improving terrestrial biosphere modeling that represents the coupled C–N cycle are imperative for realistic process representation (Davies-Barnard et al., 2020; Kou-Giesbrecht et al., 2023) to better simulate N/C/climate interactions and future projections. We hope our efforts presented in this paper can stimulate more effort to work in this direction."

Minor points:

Regarding the number of coupled models in CMIP6 and models with N cycle, it is unclear and potentially misleading as "11 out of 112 models include N cycle" (lines 81ff). Please clarify if you are focusing on the land vegetation models and the portion of them with interactive N module.

Reply:

Thank you for your enquiry. We revised this part to clarify the statement.

New Lines 62-68. *"In the latest Coupled Model Intercomparison Project Phase 6 (CMIP6, Eyring et al., 2016), although there were 112 different coupled ESMs with various land surface models from 33 institutions, only 6 ESMs that incorporated an N cycle module contributed to the CMIP6 model intercomparison study on carbon concentration and carbon–climate feedback (Arora et al., 2020). In CMIP5, there were only 2 ESMs with N cycle modules included in the same model intercomparison study (Arora et*

al., 2013). The coupling of N processes in ESM is still an important area of model development (Ghimire et al., 2016; Yu et al., 2020).”

Please note that in our last version, we only indicate “fewer than 10” models. We now provide a more precise number, which is 6.

If keeping results and discussion separated, please consider restraining from discussing results and referring to other studies in the result section.

Reply:

Thank you for this very good suggestion. Several changes have been made.

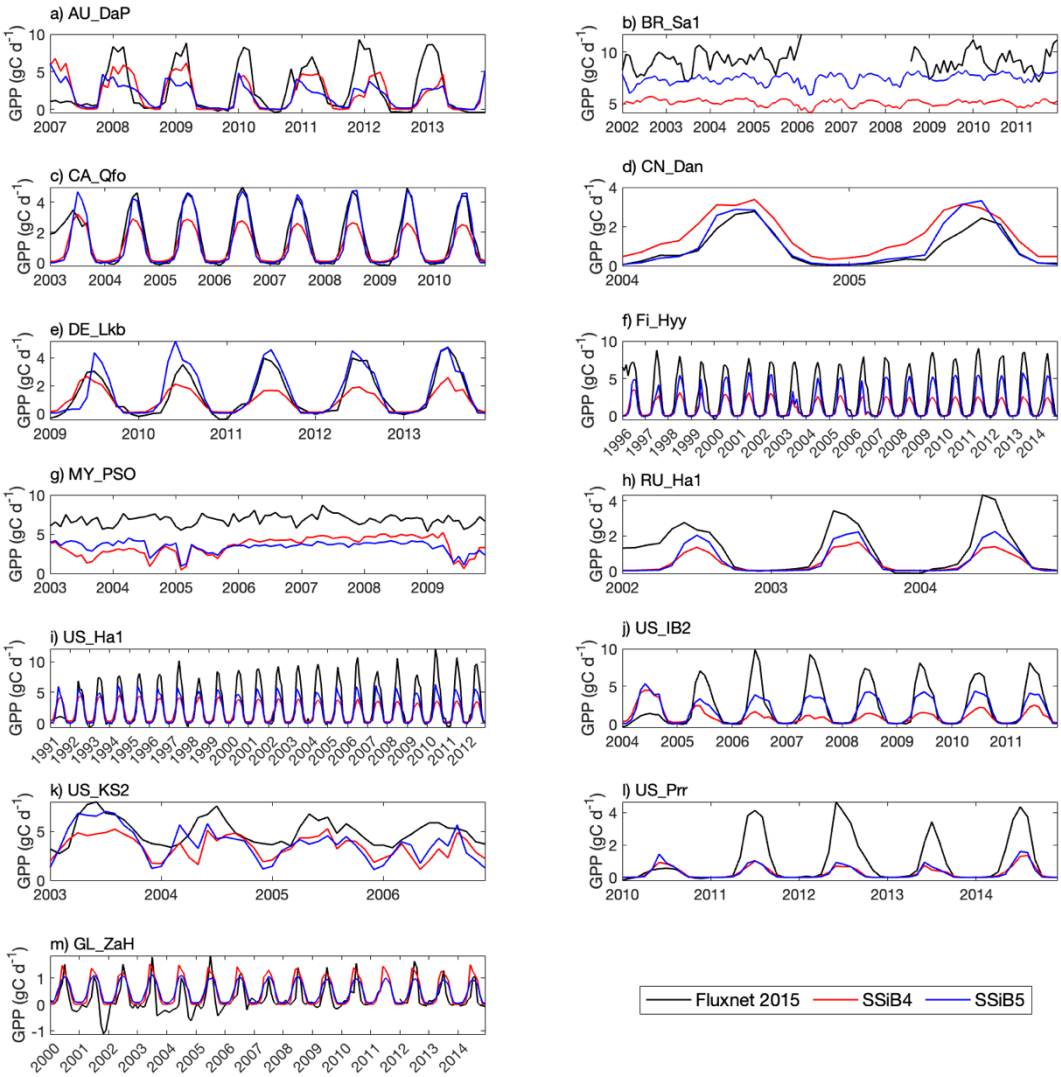
(1). In the “Results” section, we have deleted the sentence to discuss the effect of phosphorous.

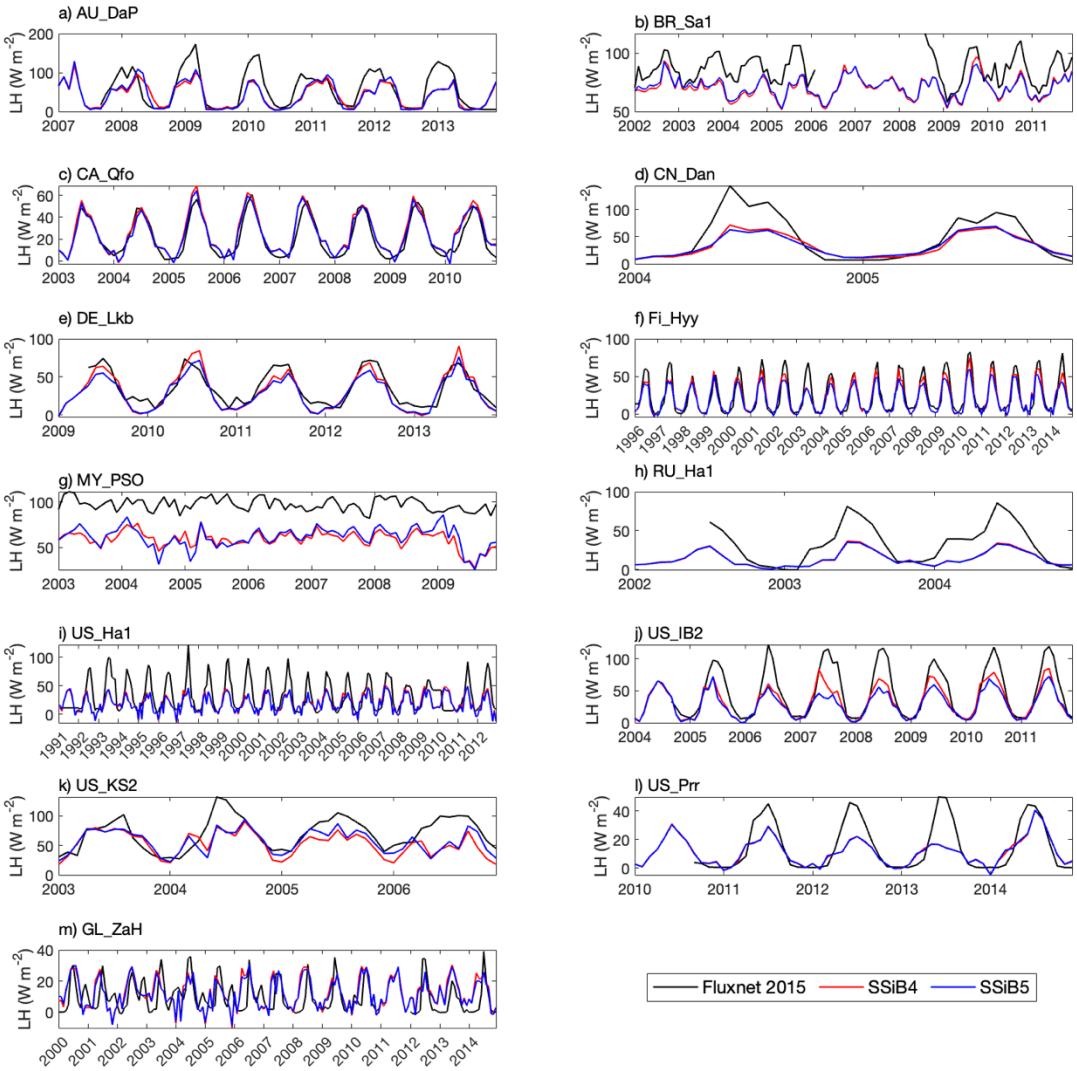
(2). We separated the “Discussion and Conclusion” section into a discussion section and a summary section. In the summary section, we more concisely summarize our major results to avoid simple repeating. We also added a concluding paragraph to the end of the paper (to respond to the reviewer’s comment above).

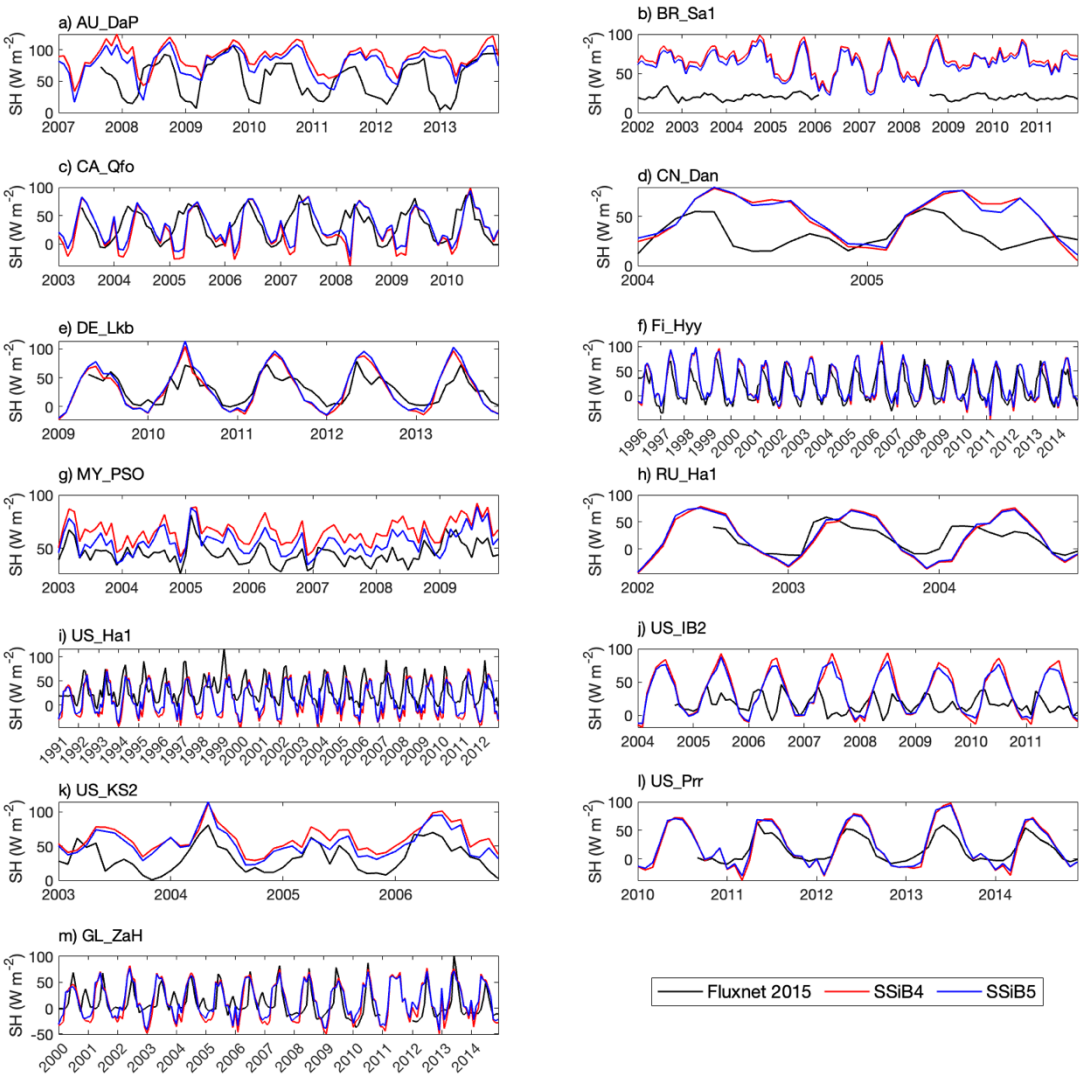
Thank you for adding the site comparison for tundra. In Figure R1, the flatline for GPP 2011-2012 seems to be connecting the missing data which should be corrected. Please check other figures as well, e.g., Figure 5m, Figure 6b, and several plots starting with flatlines.

Reply:

Thank you for your careful review. It is indeed that some site data are missing in Fluxnet 2015. We have replaced the flat lines with blanks.




a) AU_DaP
b) BR_Sa1
c) CA_Qfo
d) CN_Dan
e) DE_Lkb
f) Fi_Hyy
g) MY_PSO
h) RU_Ha1
i) US_Ha1
j) US_IB2
k) US_KS2
l) US_Prr
m) GL_ZaH
LH (W m-2)
Fluxnet 2015
SSiB4
SSiB5



Please revise the captions to be independent and self-explanatory instead of "same as Figure x…"

Reply: Done. We have deleted "same as Figure XXX" and revised these with independent and self-explanatory figure captions (Figures 6, 7, 11, and 12).

Please check all the table and figure captions and if they are referred to correctly (e.g., line 806 should be Table 8).

Reply: Done.

Please make sure all the abbr. are explained first, including the ones in tables.

Reply: Thank you for your careful review. We have checked and revised the manuscript to ensure that it is accurate and explained first (in ***new lines 20, 37, 46, 59, and 106***).

Please be precise with terms such as "plant N processes" vs "ecosystem N processes", "simulation" vs "prediction" (not recommended), "Vmax" vs "Vcmax" vs "Vc, max" etc.

Reply: Thank you for your careful review. In the revised paper,

"ecosystem N processes" has been replaced by "plant N processes".

"Model prediction" has been replaced by "model simulation".

"Vmax" and "Vcmax" have been replaced by "$V_{c,\ max}$".

Not all "C/N ratios" were replaced by "CNRs".

Reply: In the revised paper, all "C/N ratios" have been replaced with "CNR".

I suggest the authors again to restrain from citing excessively. Please select the most representative references carefully instead of accumulating all the

citations for a well-established or well-recognised statement. For instance, new lines 54ff: “Adequate C-N coupling in plant N processes has been indicated as an area that still needs intensive investigation (Thum et al., 2019; Ghimire et al., 2016; Goll et al., 2017; Yu et al., 2020; Zaehle et al., 2015; Zhu et al., 2019)” does not need all six citations to back up the need of the research (which is then repeated multiple times unnecessarily).

Reply: Thank you for your suggestion. We selected the most representative references as suggested.

New Lines 66-68: *“The current status of C-N coupled models in the CMIP model intercomparisons and knowledge gaps and divergent theories in C-N coupling parameterizations suggest coupling of N processes in ESM is still an important area of model development (~~Thum et al., 2019;~~ Ghimire et al., 2016; ~~Goll et al., 2017;~~ Yu et al., 2020 ~~Zaehle et al., 2015; Zhu et al., 2019~~).”*

Line-specific comments (correspond to the pdf file with tracked changes):

Throughout the manuscript, it remains common for the sentences with redundancy, lack of precision, unclear language, and logical inconsistency. For instance:

Lines 73ff: “Some key plant N processes, such as N limitation on GPP, the effect of biomass N content on autotrophic respiration, plant N uptake,

ecosystem N loss, and biological N fixation, have been introduced into LSMs with various complexities to determine the effects of N limitation in current land models", from biological N fixation as one source of N input into the ecosystem and excessive N for plant use leaving the ecosystem are not necessarily plant N processes; as the effect of N on autotrophic respiration is specified as biomass N content, what about impact of N limitation on GPP? Leaf N content? Implication of such processes in LSMs is not intended to determine the effects of N limitation on models, but on C-N cycles using models… Please revise and add citations.

Reply: We agree that this part is not closely associated with the text before and after. It is rather confusing. We have deleted this part and replaced it with *"Several parameterizations have been developed in LSMs with various complexities to determine the effects of N limitation"* (***new line 57***).

Lines 78ff: "These methods include, for instance, using N to scale down the photosynthesis parameter V(c, max) (Ghimire et al., 2016; Zaehle et al., 2015) or potential GPP to reflect N availability (Gerber et al., 2010; Oleson et al., 2013; Wang et al., 2010), defining the C cost of N uptake (Fisher et al., 2010) and optimizing N allocation for leaf processes (Ali et al., 2015)", do you mean using N availability or N stress to scale down Vcmax and potential GPP? It reads like suggesting N availability can be reflected by how Vcmax and potential GPP are scaled down by N (also, what N? Soil

N or plant N uptake?), which is a logic loop; do you mean the carbon cost for BNF by Fisher et al. 2010a? Please revise and specify.

Reply: We have modified this paragraph as follows:

New Lines 58-61: *"These methods include, for instance, using leaf N availability to scale down the photosynthesis parameter* $V_{c,max}$ *(Ghimire* et al.*, 2016; Zaehle* et al.*, 2015) or potential GPP* ~~*to reflect N availability*~~ *(Gerber* et al.*, 2010; Oleson* et al.*, 2013; Wang* et al.*, 2010), defining the energetic cost of N uptake (Fisher* et al.*, 2010) and optimizing N allocation for leaf processes (Ali* et al.*, 2015)".*

The sentence for C cost is from Fisher et al.'s paper. Introducing BNF needs a lot of explanation plus BNF is not that closely associated with this paper, which would cause confusion.

Lines 81ff: "The wide variety of assumptions and formulations of N cycling processes and C-N coupling reflects knowledge gaps and divergent theories, and further investigation is imperative (Kou-Giesbrecht, S., et al. 2023)", "The coupling of N processes is still an area of model development", "In the latest Coupled Model Intercomparison Project Phase 6 (CMIP6, Eyring et al., 2016), although there were 112 different coupled models with various land surface models from 33 research teams, only 10 models incorporated an N cycle module (Arora et al., 2020)", and 54ff: "Adequate C-N coupling in plant N processes has been indicated

as an area that still needs intensive investigation" are repetitive. Please revise and rearrange.

Reply: The sentence "Adequate C-N coupling in plant N processes has been indicated as an area that still needs intensive investigation" was deleted, and revisions were made to improve the presentation flow. The paragraph has been modified as follows:

New Lines 54-68: *"The fundamental aspects of N cycling for terrestrial biosphere models, such as N limitation of vegetation growth, strategies in which vegetation invests C to increase the N supply under N-limited conditions, and N limitation of decomposition, have been identified as important challenges for representing N cycling in terrestrial biosphere models (Meyerholt et al., 2020; Peng et al., 2020; Zaehle et al., 2015). Several parameterizations have been developed in LSMs with various complexities to determine the effects of N limitation. These methods include, for instance, using leaf N to scale down the photosynthesis parameter Vc,max (Ghimire et al., 2016; Zaehle et al., 2015) or potential GPP (Gerber et al., 2010; Oleson et al., 2013; Wang et al., 2010), defining the energetic cost of N uptake (Fisher et al., 2010) and optimizing N allocation for leaf processes (Ali et al., 2015). There are wide variety of assumptions and formulations of N cycling processes and C-N coupling in land models. Furthermore, in the latest Coupled Model Intercomparison Project Phase 6 (CMIP6, Eyring et al., 2016), although there were 112*

different coupled ESMs with various land surface models from 33 institutions, only 6 ESMs that incorporated an N cycle module contributed to the CMIP6 model intercomparison study on carbon concentration and carbon–climate feedback (Arora et al., 2020). In CMIP5, there were only 2 ESMs with N cycle modules included in the same model intercomparison study (Arora et al., 2013). The current status of C-N coupled models in the CMIP model intercomparisons and knowledge gaps and divergent theories in C-N coupling parameterizations suggest coupling of N processes in ESM is still an important area of model development (Ghimire et al., 2016; Yu et al., 2020).”

Table 1: I am not sure about “dead N”.

Reply: “Dead N” refers to woody debris N pools generated from the death of large wood, fine branches, and coarse roots. A note has been added to Table 1.

Line 214: change the “to” to “on” in “effects of N processes to the C cycle”.

Reply: Thank you. ***New Line 176*** has been revised to *“All these considerations in the framework should help to understand the effects of N processes ~~to~~on the C cycle more comprehensively”.*

Line 217: what do you mean by plant fertility and how does it differ from

plant productivity?

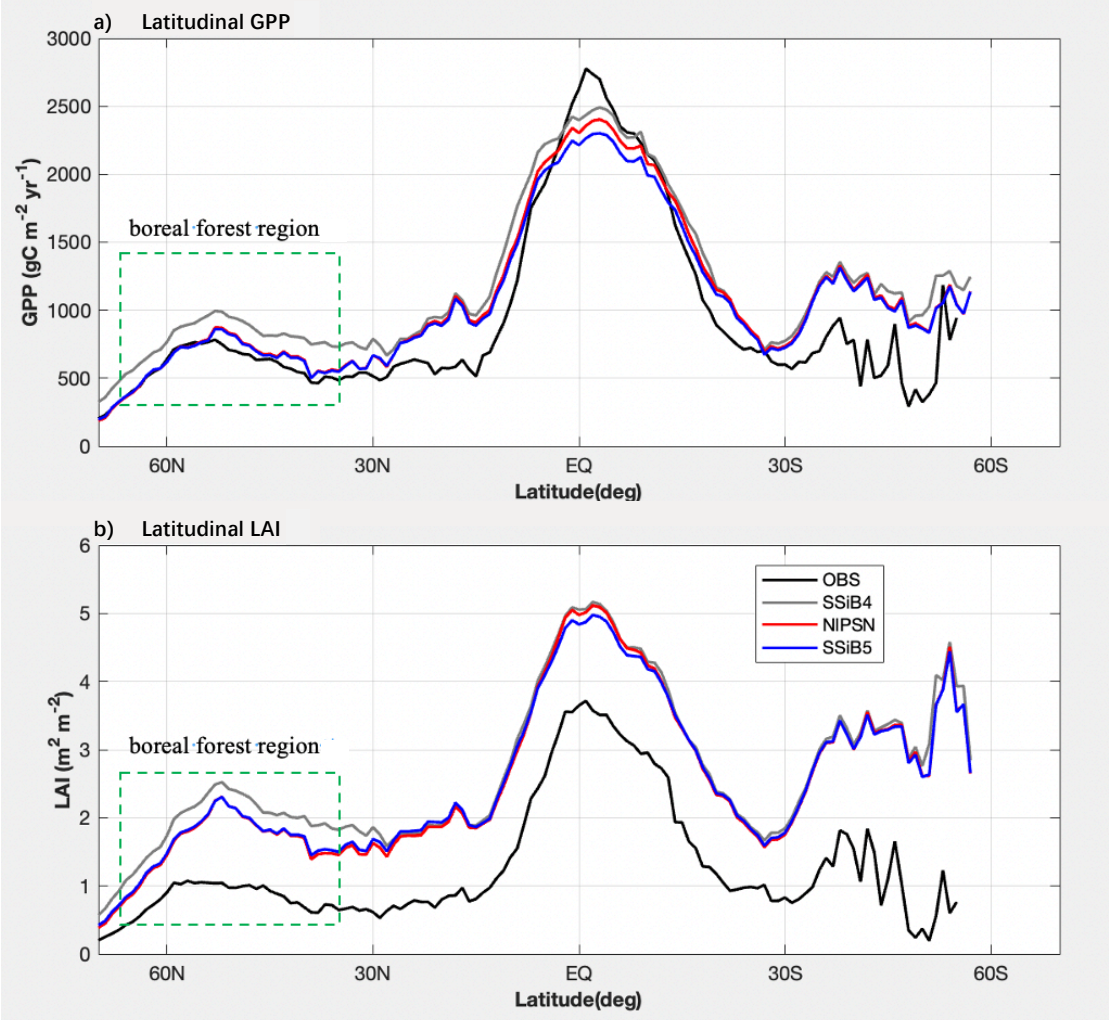
Reply: This sentence has been revised to *"Nutrient deficiency may result in decreased soil fertility and/or plant productivity."* (***new line 180***)

Figure 4: site names are difficult to read.

Reply: We have changed to lager font size in the figures to help readability.

Figure 9: the formatting marks are visible for the texts.

Reply: The formatting marks have been removed.



Tables 7 and 8: do MTE and GIMMS need a column for bias?

Reply: Thank you for your suggestion. The first column of bias was removed.

Lines 771ff: “This study presents improvements in modeling the C cycle by introducing plant N processes into SSiB5/TRIFFID/DayCent-SOM, using DayCent-SOM to obtain the amount of N available to plants and plant soil N uptake”. Please clearly specify the improvement is only regarding the previous SSiB/Triffid model version.

Reply: This has been revised as follows:

New Line 634: *"This study presents improvements in modeling the C cycle, compared to that of SSiB4/TRIFFID, by introducing plant N processes specifically into SSiB5/TRIFFID/DayCent-SOM. The DayCent-SOM provides the amount of N available to plants and plant soil N uptake."*

Lines 774ff: please specify that the dynamic CNR is for different PFTs (e.g., not for soil) and to what the plant resistance and responses are referred to (e.g., N stress).

Reply: The sentence has been changed as follows:

New Lines 639-640: *“The dynamic plant CNR is used to represent plant resistance and response to N stress, which allows adaptations in the stoichiometry of C and N.”* In the next sentence of the original text, we

explained "resistance": *"This approach increases nutrient use efficiency and takes into account N remobilization and resorption; the N limitation effect does not necessarily linearly or instantaneously respond to the available N content".*

Lines 775ff: just because "these processes can increase nutrient use efficiency and reduced the impact of N limitation" and "a linear relationship … is only valid when N availability is not sufficient for the minimum N demand for new growth", it is not clear to me how it is an advantage.

Reply: "Advantage" is a subjective assessment. To avoid controversy, we deleted the sentence with "advantage".

Line 780: I don't think "the state of plant growth" is used correctly here. It is also never mentioned elsewhere.

Reply: Thank you for pointing this out. The phrase "the state of plant growth" has been replaced with "N sufficiency". The sentence has been revised as follows:

New Lines 643-644: *"With the new model structure, the impacts of N on GPP are predicted directly but not linearly with leaf N content, which is affected by ~~the state of plant growth~~ N sufficiency, autotrophic maintenance and growth respiration, and plant phenology."*

Lines 782ff: it is questionable that "by comparing site-level results" can be evidence for "enhanced global model performance". Especially that only a few sites showed noticeably improved results compared to observations. Please revise.

Reply: The sentence has been changed to *"encourage us to carry out assessments of global performance"*.

Lines 785ff: "… produced significantly less absolute bias for GPP and LAI" is not tested statistically.

Reply: See our response to main concern 4.

Comments from referee #2

The authors have carefully revised the manuscript and addressed most of my suggestions. Since SSiB5/Triffid/DayCent-SOM v1.0 model has anthropogenic N inputs, the authors should introduce the N input data (fertilizer, manure, atmospheric deposition) used to drive the model. If N inputs data were not used to drive the model, the authors should write a paragraph to discuss that the reported N limitation effects may be largely exaggerated. This is an important issue and should be clearly stated. For LAI, according to my experience, most LSMs don't overestimate this much (100%), I still think it is important to discuss the potential improvement measures.

Reply: Per the reviewer's suggestion, we have extended our original discussion on the effect of anthropogenic N to one full paragraph.

New Line 624-628: *"Anthropogenic N input is one of the major factors affecting C–N coupling and N limitation. The anthropogenic N inputs to terrestrial ecosystems have been much greater than the vegetation N fixation in recent decades in some areas, such as eastern China and the central USA. As such, anthropogenic N input can relieve N limitations there (Tian et al., 2022). Due to the scope of this paper, we did not use anthropogenic N inputs to drive our model. This is an important issue for further investigations to comprehensively understand the effect of N limitation."*