Response to Anonymous Referee #1

General comments:

This study addresses the critical need to improve the representation of nitrogen cycle in land biogeochemical processes which is a crucial aspect of modelling development. The manuscript presents a novel approach by integrating a soil organic matter and nutrient cycling model to advance simulating coupled global carbon-nitrogen cycles in a process-based dynamic vegetation model. The authors demonstrate that the coupled model version shows in general improvement of GPP, LAI, and heat fluxes validated against site-level and global observations, compared to the previous model version. It is certainly a timely work with supporting analysis, although only partially reflecting the performance of the revised model. There are several concerns that the authors should address to enhance the manuscript.

Reply: Thank you very much for your comprehensive and constructive reviews. We appreciate your effort and acknowledge your review in the paper's "Acknowledgment".

Main concerns:

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.

Reply: Thank you for the comments and suggestions. In Main Concern 5, the reviewer also complains of a related issue: "The manuscript contains erroneous, redundant, or repetitive expressions throughout. Especially in the method section, there is a substantial amount of text either already addressed in the introduction or better suited for the discussion". Since these two issues both contribute to the presentation problem of model parametrization, we address these issues together.

To more comprehensively describe the necessity of modifying the current relevant parameterizations, we have added more relevant information to Section 2.2.1-2.2.5. Moreover, because our discussions on the methodology in the previous version spread across several places in the paper, which causes repetition and redundancy but is not comprehensive in either place, we reorganize the introduction and Section 2.2.1-2.2.5 to arrange the discussion on these processes more concentrating on relevant sections.

In this way, we hope that the description for each process is much clearer and more comprehensive and eliminates some redundancies and repetitions.

In the revised introduction, we mainly emphasize the necessity of including proper N processes in the Earth System Model (ESM) in general and move more detailed discussions on relevant processes (parameterizations) to the related sections 2.2.1-2.2.3. Moreover, we have attempted to improve the accuracy of the description of the development history of N process modeling and reduce the number of citations of relevant papers, as suggested by the review.

The following are a few examples in our revisions.

In the introduction, we revised the N-process model development as follows:

New Lines 54-69: *"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). 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). 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. 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). 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 about 10 models incorporated an N cycle module (Arora et al., 2020)."*

In Section 2.2.2 "Dynamic C/N ratio", some relevant information in the Introduction has been moved to this section. We first briefly describe the C/N ratio in the natural world and the current status of C/N ratio modeling with proper citations. We then added more information for our modeling.

In Section 2.2.3 “Effects of N limitation on photosynthesis”, we also moved the discussion of the parameterization of this issue from the Introduction to this section and made many revisions to improve our description of our model.

In Section 2.2.4 “Improvement of nitrogen impact on respiration rates”, we added more information to support our approach and how our parameterization was obtained.

In Section 2.2.5 “N limitation on LAI based on plant phenology”, we added more information on why this is an important issue, which was largely missing in the previous version.

We hope that with these major revisions, the issues that concern the reviewer will be properly addressed.

2. Although it is important to evaluate the impacts on C and heat fluxes, more information on how the dynamic representation of the C/N ratios alter the N cycles would be very pertinent and interesting to report, provided the model outputs include relevant variables to describe the processes. Otherwise, there would remain a logic gap to make sense of the differences in C and heat variables between the new and old model versions.

Reply: We understand the reviewer’s idea that it is interesting to compare the fixed C/N ratio and dynamic C/N ratio effects. In fact, this was also our original consideration. However, it is difficult to design such comparisons. Our parameterizations of the effects of N limitation on three processes (photosynthesis, phenology, and respiration) (Section 2.2.3-2.2.5) are based on the dynamic C/N ratio. For a fixed C/N ratio, some of these have to be changed. As such, it is difficult to just specify a fixed C/N ratio but with other parameterizations still being associated with a dynamic C/N ratio. Simply specifying a fixed C/N ratio may mislead readers.

In the revised paper, at the beginning of Section 2.2.2, we present many studies demonstrating that the dynamic C/N ratio is a phenomenon that exists in real biogeochemical processes, which we hope can provide a background and justification for the dynamic C/N ratio approach.

3. 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.

Reply: The reviewer raises a very important issue. As an NSF Climate Dynamics Program-supported project, how the N process influences the water and energy cycle is an important subject. In fact, we have presented the latent heat flux and sensible heat flux with/without N limitation in Table 6 and Figures 6 and 7. For the 13-site average, the results with N limitation showed only slight improvement.

This is because there are three components in our model that contribute to the total latent heat flux (as shown in Fig. 7 and Table 6). They include transpiration from the canopy, direct evaporation from the leaf due to interception loss of precipitation, and soil evaporation. In the offline test, the atmospheric demand is fixed; when transpiration is reduced/increased due to the change in photosynthesis process (caused by the N limitation), soil evaporation must change to satisfy the atmospheric demand. This change is not linear because the sensible heat flux also changes. As such, with fixed demand and radiative forcing from the atmosphere, it is difficult to properly assess the effect on heat flux. At the end of this paper, we added the following lines:

New line 633-636. *"Finally, this is an offline experiment in which the atmospheric forcing (such as downward radiation) is fixed. With a fixed atmospheric demand, the heat flux response due to the N limitation effect is also limited, as shown in section 4.1. 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."*

4. The introduction made a leap from "C-only models and dynamic vegetation models generally miss the inclusion of N processes" to "this new framework not only consider N processes but also has a more realistic way to represent the processes with dynamic C/N ratios". Often dynamic vegetations include N cycles since decades, see e.g., Kou-Giesbrecht, S., et al. (2023) 10.5194/esd-14-767-2023. The current state of modelling C-N cycles is therefore misrepresented and the progress in other models is under recognized, although cited Davies-Barnard et al 2020 the authors themselves.

Reply: Thank you for pointing this out. We have revised this part of the introduction. Please see our response to Main Concerns #1.

5. The manuscript contains erroneous, redundant, or repetitive expressions throughout. Especially in the method section, there is a substantial amount of text either already addressed in the introduction or better suited for the discussion.

Reply: We apologize for these writing issues. We have revised and reorganized the paper, especially the introduction and methodology sections (2.2.1-2.2.5), to make the statement accurate and avoid redundancies. Please see our response to Main concern #1 for more information.

Minor points:

1. Suggest revising the title to make it more concise.

Reply: Done. We have revised this title to *"Development of a plant carbon-nitrogen interface coupling framework in a coupled biophysical-ecosystem-biogeochemical model (SSiB5/Triffid/DayCent-SOM v1.0") and eliminated "Its parameterization, implementation, and evaluation".*

2. The manuscript may benefit from additional analysis relating to the improved representation on N limitation for different PFTs.

Reply: We agree with the reviewer that it is a very good idea to include the analysis relating to the improved representation of N limitation for different PFTs. In fact, this information was included in our original manuscript. However, based on the editor's instructions for this paper's resubmission, the current paper is mainly focused on describing model development. The discussion on the scientific issues in the previous submission has been removed per the editor's suggestion.

3. The use of "wood" or "stems/wood" as a plant organ can be misleading. In Table 1 they are then listed as "component" where it is also false to list "wood" under grasses. Please be consistent with the common terms and stick with stem.

Reply: Done. We now use "stem" throughout the paper.

4. Suggest adding some information on tundra shrub as it is not covered in the validation sites (Table 3).

Reply: Thank you for the suggestion. Our validation sites were limited to the AmeriFlux sites. We now include a tundra site (Lund *et al*., 2012) for validation. The figure attached below is also included in the revised Section 4.1. The results are consistent with those at other sites and are shown in the revised Table 6 (previously Table 5).

Table R1. Tundra site information used for model validation.

Site name	LAT	LONG	PFT	Time
Zackenberg Heath	74.47	-20.55	tundra	2000-2014

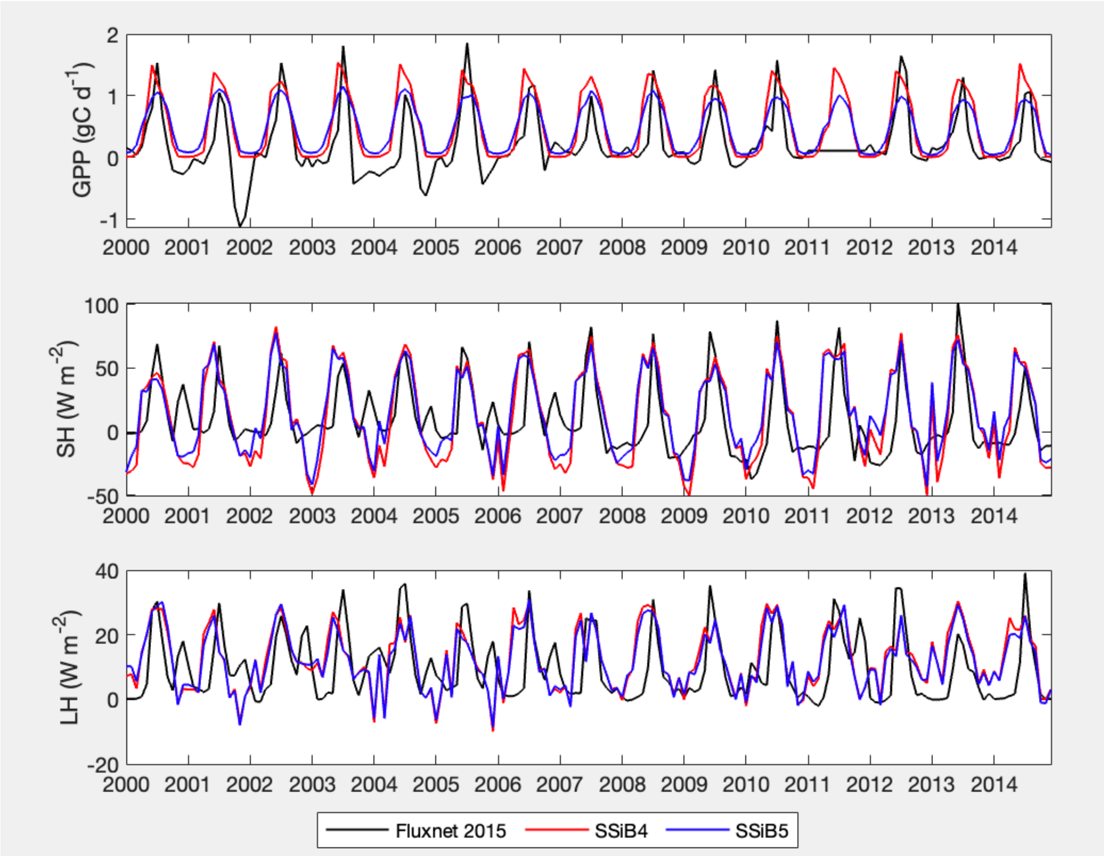


Figure R1. Simulated seasonal variations in GPP, sensible heat, and latent heat against observations at the tundra site.

References:

Lund, M., Falk, J. M., Friborg, T., Mbufong, H. N., Sigsgaard, C., Soegaard, H. and Tamstorf, M. P.: Trends in CO2 exchange in a high Arctic tundra heath, 2000–2010, J. Geophys. Res., 117(G2), G02001, 2012.

5. The figure qualities are not consistent.

Reply: To improve the figure quality, we utilized MATLAB to redraw all the figures, employed the same image resolution parameters for the output, and used the PNG format instead of the PDF format for storage.

6. When dealing with a variable having the unit of per area (e.g., Navail, g N m-2), the soil depth is essential information, however not clearly indicated in the manuscript.

Reply: Thank you for your careful review. We have added this information to the revised text as follows:

New Lines 227-228: *"The DayCent-SOM only provides the total available nitrogen (N_{avail}) for the plant within one grid box (the soil is 3.2 m in depth), which consists of several PFTs."*

7. The authors are strongly suggested to select references carefully instead of piling them up excessively, such as with the 17 citations in L54 to 57 and 12 citations in L40.

Reply: We apologize for this. We have selected references more carefully and deleted some as suggested, as shown in the example below.

New Lines 50-53: *"As such, the N cycle and its effect on C uptake in the terrestrial biosphere have been incorporated into land surface models (LSMs) of ESMs (Davies-Barnard et al., 2020; Kou-Giesbrecht et al, 2023) with various representations of N processes (Ali et al., 2015; Asaadi et al., 2021; ~~Best et al., 2011~~; Clark et al., 2011; Davies-Barnard et al., 2020; Ghimire et al., 2016; Goll et al., 2017; ~~Krinner et al., 2005;~~ Lawrence et al., 2019; ~~Matson et al., 2002;~~ Oleson et al., 2013; Smith et al., 2014; Thum et al., 2019;~~ Wang et al., 2010;~~ Wiltshire et al., 2020;~~ Yu et al., 2020; Zhu et al., 2019~~)."*

New Lines 35-39: *"To study these processes, the land surface components of Earth System Models (ESMs) have evolved from those that represent only physical processes (i.e., hydrology and the energy cycle) to those that include the terrestrial carbon I cycle, vegetation dynamics, and nutrient processes(Cox, 2001; Dan et al., 2020; Foley et al., 1998;~~ Jiang et al., 2014; Niu et al., 2020;~~ Oleson et al., 2013; ~~Pan et al., 2017;~~ Sellers et al., 1996; Sitch et al., 2003; Wang et al., 2010; Zhan et al., 2003). "*

8. Please clarify several terms in the paragraph of L70-87, including "plant resistance on photosynthesis …" (as in it does not make sense to call it resistance on photosynthesis but more like resistance on the reduction of photosynthesis capacity or potential photosynthesis rate, not to be confused with photosynthesis rate, under N limitation), "C/N interactions" (as in if it is about the C to N ratio and something else, or the interactions between some C processes and N processes), "self-adjustment" (as in how such behaviors differ from being simply considered as "responses"), and "fertility" (as in if it refers to soil fertility or plant fertility which is not a well-known term).

Reply: Thank you for your careful review. In the revised paper, “Plant resistance on photosynthesis” has been replaced with more proper words/presentations.

“C/N interaction” has been revised to “C and N process interaction”.

“Self-adjustment” is replaced by “response”, “adaptation”, or eliminated.

“Fertility” was changed to “plant fertility”.

Line-specific comments:

1. L38, suggest changing to only “physical processes” instead of “biophysical processes” as the commonly simplified land representation in ESMs does not include biological processes as the authors listed themselves.

Reply: Done.

New Line 36: *“To study these processes, the land surface components of Earth System Models (ESMs) have evolved from those that represent only physical processes (i.e., hydrology and the energy cycle) to those that include the terrestrial carbon (C) cycle, vegetation dynamics, and nutrient processes (Cox, 2001; Dan et al., 2020; Foley et al., 1998; Oleson et al., 2013; Sellers et al., 1986; Sitch et al., 2003; Wang et al., 2010). ”*

2. L47, suggest changing “Those C-only models” to “The C-only models” as in not all those models mentioned prior, i.e., land process models and dynamic vegetation models, are C-only.

Reply: Done.

New Line 45: *“The C-only models ignore significant nitrogen (N) impacts and therefore overestimate C sequestration by terrestrial ecosystems under climate change (Peñuelas et al., 2013; Zaehle et al., 2015)”.*

3. L72 “dynamic plant C/N ratio” is not necessarily a concept. According to the authors, it should be a more realistic representation than fix ratios.

Reply: Done.

New Line 73: *"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."*

4. L73-74, please revise these two sentences. Suggest removing "Due to their relative immobility". Suggest changing "A deficiency of any type of nutrient" to "Nutrient deficiency".

Reply: The sentence has been revised and moved to Section 2.2.2.

New Line 174 *"Nutrient deficiency may result in decreased plant productivity and/or plant fertility (McDowell et al., 2008; Morgan and Connolly, 2013; Stenberg and Muola, 2017)."*

5. L75-77, suggest changing "have to" to "can" and keeping fewer citations. The usage of self-adjustment is misleading in such context.

Reply: Done. The sentence has been revised and moved to Section 2.2.2 as follows:

New Line 175: "*Evidence has shown that plant CNR can change with nutrient availability (Chen and Chen, 2021; McGroddy et al., 2004; Meyer-Grünefeldt et al., 2015; Sardans et al., 2012; Smith, 1991;)* "

6. L77-79, please revise this sentence. Lipid is not a polymer. It should be "nutrient-starved". Unclear if the authors mean C/N ratios are influenced by being exposed to high light or the accumulation of C polymer are greater when exposed to high light. Please revise the term "high light".

Reply: The sentence has been revised and moved to Section 2.2.2 as follows:

New Line 176: "*Plant cell CNRs are influenced by the accumulation of C polymers, such as carbohydrates, and are greater when cells are nutrient starved or exposed to* high levels of photosynthetically active radiation (PAR) *(Aber et al., 2003; MacDonald et al., 2002; Talmy et al., 2014).* "

7. L82, suggest clarifying what the N is, such as soil N availability or plant N, and whether it is photosynthesis capacity or actual photosynthesis rate.

Reply: This sentence has been deleted in the revised version because it is redundant with other parts of the paper.

8. L96-99, please add references for the flux data and satellite-derived observational data.

Reply: Done.

New Lines 86-91: *"The coupled model is verified at thirteen flux tower sites (Lund et al., 2012; Pastorello et al., 2020) with different PFTs and is used to conduct several sets of global 2-D offline simulations from 1948 to 2007 to assess the effects of the coupling process. Model predictions of global GPP and LAI have been evaluated against satellite-derived observational data (Jung et al., 2009, Sheffield et al., 2006, Zhu et al., 2013). The results demonstrate the relative importance of different plant N processes in this C-N framework."*

9. L116, please clarify what "vegetation conditions" are. It should be "physiological".

Reply: The sentence has been revised to make it more specific.

New Lines 106-107: *"Moreover, the surface albedo and aerodynamic resistances are also updated based on the vegetation leaf area index, vegetation cover, vegetation height, and greenness."*

10. L119, suggest changing to "C4 grasses" to be precise and consistent with Tables 2 and 3. It is "tundra shrub" in Tables 1 and 2. Please clarify what is "net C availability".

Reply: Done. We have revised Tables 1 and 2 to maintain consistency. The term "Net C availability" was replaced with "NPP".

11. L125, consider listing the pools in a table to present the information more clearly.

Reply: We listed the nitrogen pools as suggested (also as new Table 1 in the paper).

Table R2. The Nitrogen Pools in DayCent-SOM

		Aboveground	**Belowground**
Mineral N pool			Soil mineral N pools
Organic N pool	non-woody litter pools	Surface structural N Surface metabolic N	Soil structural N Soil metabolic N
	woody debris pool	Surface dead N	
	kinetically defined organic matter pools	Surface active N Surface slow organic N	Soil active organic N Soil slow organic N Soil passive organic N

12. L134, please either clarity the temperature and moisture effects or remove the word "effects".

Reply: These words have been removed. The sentence has been revised as follows:

New Line 125: *"Each type of organic pool has its own intrinsic rate of decomposition, modified by temperature and moisture ~~effects~~ (Parton et al., 1994)."*

13. L140, please revise "plant life activities".

Reply: The term "plant life activity" was vague and has been deleted throughout the paper.

New Line 140: *"To represent C and N interactions, we have developed a plant C-N interface framework to couple biophysical and biochemical processes in the caron and nitrogen cycles ~~in plant life activities.~~"*

14. L141, L144, please refrain from using "/" excessively. Suggest changing "physical/biological" to "physical and biological" and "temperature/moisture" to "temperature and moisture". Please check for other "/" as well.

Reply: Done. Thank you for pointing this out. We eliminated many instances of "/" in the paper.

15. L145, please revise this sentence and clarify "surface water", "carbon fluxes" (it is not mentioned in the second half of the sentence), and "plant litter" (e.g., as in fluxes for production and decomposition or pools).

Reply: Done. These sentences have been revised as follows:

New Lines 144-146: *"The soil N dynamics model (DayCent-SOM) is directly driven by soil temperature, soil moisture, net radiation and plant C and N litter inputs into the soil organic pool, which are provided by the SSiB5/TRIFFID. ~~Because the surface water, radiation, and carbon fluxes and plant litter are calculated by SSiB5, we force DayCent-SOM with SSiB5-produced soil temperature, soil moisture, and SSiB5/TRIFFID-produced plant~~*

16. L148-150, please revise this sentence. It is unclear what the authors mean by "N effects on plant physiology from photosynthesis, ... plus a dynamic C/N ratio".

Reply: The sentence has been revised as follows:

New Lines 148-150: *"Following plant N uptake from DayCent-SOM, our plant C-N interface framework describes the effects of N on photosynthesis, plant autotrophic respiration, and plant phenology (Fig. 1). All these effects are associated with a dynamic CNR."*

17. L152-154, please revise this sentence. It reads repetitive with "not only considers N limitation ... but also emphasizes the N limitation effect ..." and "help us obtain more information to understand ..."

Reply: The sentence has been revised as follows:

New Lines 169-171 *"With consideration of the effect on phenology, the N limitation effect during the growth season is emphasized. All these considerations in the framework should help to understand the effects of N processes to the C cycle more comprehensively."*

18. L163-164, please revise the sentence to clarify the potential confusion that GPP follows autotropic respiration. Please revise "in plant life".

Reply: These sentences have similar meanings to those of other sentences and have been deleted from the revised paper.

19. L166, this might be controversial as in plants can certainly respond and adapt to lower N availability but it would be a stretch to certainly call it "adjust resource

Reply: ***New Line 180***. We changed "adjusting resources" to "respond and adapt to lower N availability".

20. L167-170, please revise this sentence. It reads contradicting with "resorb only 50%" and "cause a major internal nutrient flux".

Reply: "A major internal nutrient flux" has been eliminated. The sentences have been revised to

New Lines 181-184. *"Studies show that plants resorb only about 50% of leaf N on average (Aerts, 1996) to conserve nutrients (Clarkson and Hanson, 1980) and to increase nutrient use efficiency (Herbert & Fownes, 1999; Vitousek, 1982). These processes cause changes in the CNR to reduce the impact of N limitation (Talhelm et al., 2011; Vicca et al., 2012)."*

21. L170-172, please revise this sentence to improve clarity and avoid going in circle, such as what affect plant productivity and litter N content. Now it reads like “plant responses affect plant productivity and litter N content”.

Reply: This sentence has been deleted. This sentence tries to point out the fixed C/N ratio’s shortcoming. However, actually, it did not provide real substance.

22. L173-174, please revise this sentence to clarify “improve plant responses”.

Reply: This sentence has been eliminated from the revised paper to avoid repetition.

23. L179-196, the majority of this paragraph should fit in the introduction or discussion better.

Reply: This paragraph has been moved to Section 2.2.3 to provide background information for our parameterization of the N limitation effect on photosynthesis.

24. L197, please revise this sentence to increase clarity. For instance, NPP is part of the terrestrial C cycle.

Reply: ***New Line 162***: The sentence was revised to *“Nitrogen is not the only dominant regulator of photosynthesis and vegetation dynamics”.*

25. L199, please clarify “normal N concentration”.

Reply: In ***the new Line 164***, we changed it to “In common N concentration range”.

26. L203, please revise the sentence “Because plants need time to turnover, the plant N processes ...” for clarity and accuracy.

Reply: To reduce the number of repetitions, this and some other sentences have been removed.

27. L205, perhaps the authors mean “modulates LAI evolution, e.g., via leaf mortality?” Should it be “supplies” instead of “supplements?”

Reply: To reduce the number of repetitions, this and some other sentences have been removed.

28. L209, since C/N ratios is abbreviated as CNRs from here, why not introducing it from the start?

Reply: This is a good suggestion. We now apply the CNR from the start.

29. Regrettably, similar issues persist throughout the rest of the text. I will refrain from detailing them further until the authors have thoroughly revised the manuscript.

Reply: Please see our response to the major concern. The paper has reorganized to address this issue.

Overall, the general structure, clarity, terminology, as well as accuracy throughout the manuscript need to be substantially improved.

Response to Anonymous Referee #2

This study proposed a plant carbon-nitrogen coupling framework to improve a biophysical-ecosystem-biogeochemical model. The author ran the modified model at the site and global levels, and compared the model results with in-situ observations and remote sensing/machine learning estimations . Moreover, the authors conducted a series of experimental experiments at the global level to quantify the major effects of the N process and C-N interface coupling methodology on the C cycle. This study proposes a new approach, and considers the N limitation effects not only on photosynthesis but also on plant respiration and phenology. However, there are several significant drawbacks in this study. The reviewer has the following concerns and suggestions for the authors to consider:

Reply: Thank you very much for your comprehensive and constructive reviews. We appreciate your effort and acknowledge your review in the paper's "Acknowledgment".

Does the SSiB5/Triffid/DayCent-SOM v1.0 model consider anthropogenic N inputs (N deposition, fertilizer and manure) into terrestrial ecosystems? I guess no, since there is not no such information mentioned in the manuscript. If the model doesn't consider anthropogenic N inputs, the reported N limitation effects may be largely exaggerated because anthropogenic N inputs to terrestrial ecosystems are much larger than the vegetation N fixation in recent decades which can relief N limitation. In Figure 8 (f), the effect of N limitation is large in Eastern China and central USA, however, the anthropogenic N inputs were quite large in these regions (Tian et al., 2022), the N limitation shouldn't be large if anthropogenic N inputs are considered. This is my major concern.

Reply: The reviewer raised a very important point here. Our model includes anthropogenic N as a model input variable, and its impact is an important issue for investigation. In this paper, we did not address this issue. As a first paper for our C and N coupled model, the editor instructed us to focus on the description of model development in this resubmission. The reviewer's opinion regarding the anthropogenic effect has been well taken and included at the end of the revised paper as an important issue for further investigation (Tian *et al*., 2022).

New Lines 619-623: *"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, which can relieve N limitations (Tian et al., 2022). Due to the scope of this paper, this issue is not addressed in this paper but is an important subject for further investigation to comprehensively understand the N limitation effect."

Reference:

Tian H, Bian Z, Shi H, *et al*. History of anthropogenic Nitrogen inputs (HaNi) to the terrestrial biosphere: a 5 arcmin resolution annual dataset from 1860 to 2019[J]. Earth System Science Data, 2022, 14(10): 4551-4568.

The SSiB5/Triffid/DayCent-SOM v1.0 model performs poor in modelling the magnitude of LAI although its performance is better than SsiB4. At the global level, SsiB5 estimation is about 100% higher than the remote sensing estimation (Figure 11)! Please elaborate on how is $LAI_{balance}$ calculated in model and the vegetation carbon allocation scheme. Also, it is necessary to add one paragraph discussing the potential reasons for the overestimation of LAI and the future improvement measures.

Reply: The reviewer points out an important shortcoming in the model's LAI simulation. Recent review papers confirm that the overestimation of C sequestration and LAI is a common issue in current dynamic vegetation models (Anav *et al*., 2013; Murray-Tortarolo *et al*., 2013; Zaehle *et al*., 2015; Mueller *et al*., 2019; Gristina *et al*., 2020; Oliveira *et al*., 2021; Heikkinen *et al*., 2021).

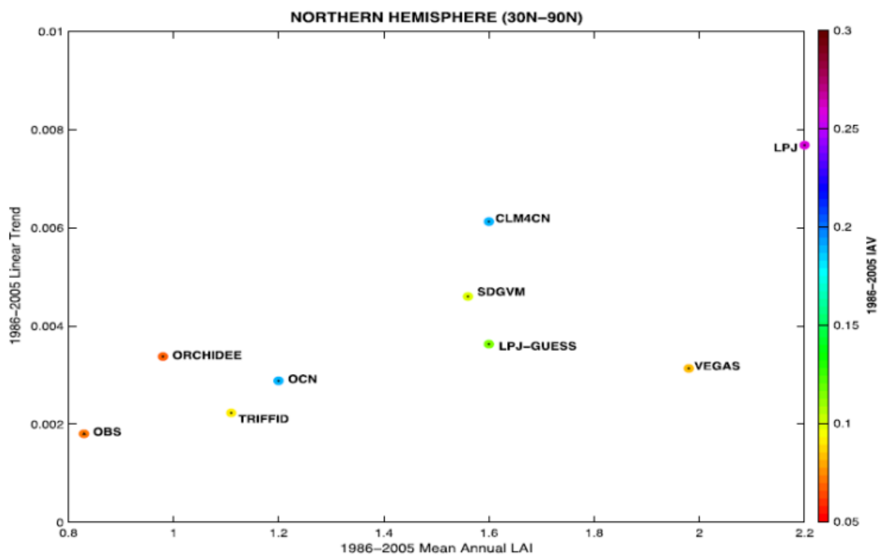
Murray-Tortarolo *et al*. (2013) and Anav *et al*. (2013) evaluated the performance of dynamic vegetation models in simulating LAI from a CMIP model intercomparison. A figure from their paper is attached below for your reference. Based on the figure, it is clear that this issue exists in most dynamic vegetation models. More recent papers, such as those cited above, also confirm this shortcoming in current dynamic vegetation models. It is important to overcome such large bias. In fact, this is one of the main motivations for us to introduce the N limitation into the Earth System Model. However, despite proper simulation of GPP after introducing N limitation, our results indicate that further efforts are still needed to improve LAI simulation. In the revised paper, we note that this is one of several issues that deserves further investigation.

Since overestimating LAI is a common problem in dynamic vegetation modeling, we only indicate that this is an issue that needs to be further investigated but did not elaborate this issue further. To understand how $LAI_{balance}$ is calculated, it needs a substantial effort (not just a couple of paragraphs), which may distract the paper's main focus. Moreover, we are not sure whether $LAI_{balance}$ is the cause of the LAI overestimation. Nevertheless, we add references after the $LAI_{balance}$ in the revised paper for additional information.

In the ***New Lines 613-618***, we added a paragraph to address this issue.

"Moreover, although the global GPP of SSiB5 was similar to that of the satellite-derived GPP, the positive bias for the LAI was still very large (Table 7). Recent review papers seem to confirm that overestimation of LAI is a common issue in current dynamic vegetation models. Murray-Tortarolo et al. (2013) and Anav et al. (2013) evaluated the performance of dynamic vegetation models in simulating LAI from a CMIP model intercomparison. The simulated LAI for almost every dynamic vegetation model is twice as large as the satellite-derived LAI. More recent studies (Zaehle et al., 2015; Mueller et al., 2019; Gristina et al., 2020; Oliveira et al., 2021; Heikkinen et al., 2021) have confirmed this shortcoming in current dynamic vegetation models. Further investigations are necessary."

Figure 2. Linear trend against average LAI for each model and satellite observations, with IAV represented as colors. The data represents the whole high-latitude Northern Hemisphere (30°–90°) for the time period 1986–2005.



Murray-Tortarolo (2013)

References:

Anav, A.; Murray-Tortarolo, G.; Friedlingstein, P.; Sitch, S.; Piao, S.; Zhu, Z. Evaluation of land surface models in reproducing satellite Derived leaf area index over the high-latitude northern hemisphere. Part II: Earth system models. Remote Sens. 2013, 5, 3637–3661

Oliveira D. C. *et al*., Depth assessed and upscaling of single case studies might overestimate the role of C sequestration by pastures in the commitments of Brazil's low-carbon agriculture plan. Carbon Management. 12, 499–508.

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There is no tundra site in site-level validation. I recommend adding at least one tundra site. Please elaborate on the calculation of PFT fractional coverage in model, and add one figure comparing model results with satellite-based land cover product to justify that model can accurately estimate PFT fractional coverage.

Reply: Thank you for the suggestion. Our validation sites were limited to the AmeriFlux sites. We now include a tundra site (Lund *et al*., 2012) for validation. The figure attached below is included in Section 4.1, and the statistics for this site are included in Table 6 (previous Table 5). The results from the new 13-site average are consistent with the previous results with the 12-site average, as shown in the revised Table 6 (previous Table 5).

Table R1. Tundra site information used for model validation.

Site name	LAT	LONG	PFT	Time
Zackenberg Heath	74.47	-20.55	tundra	2000-2014

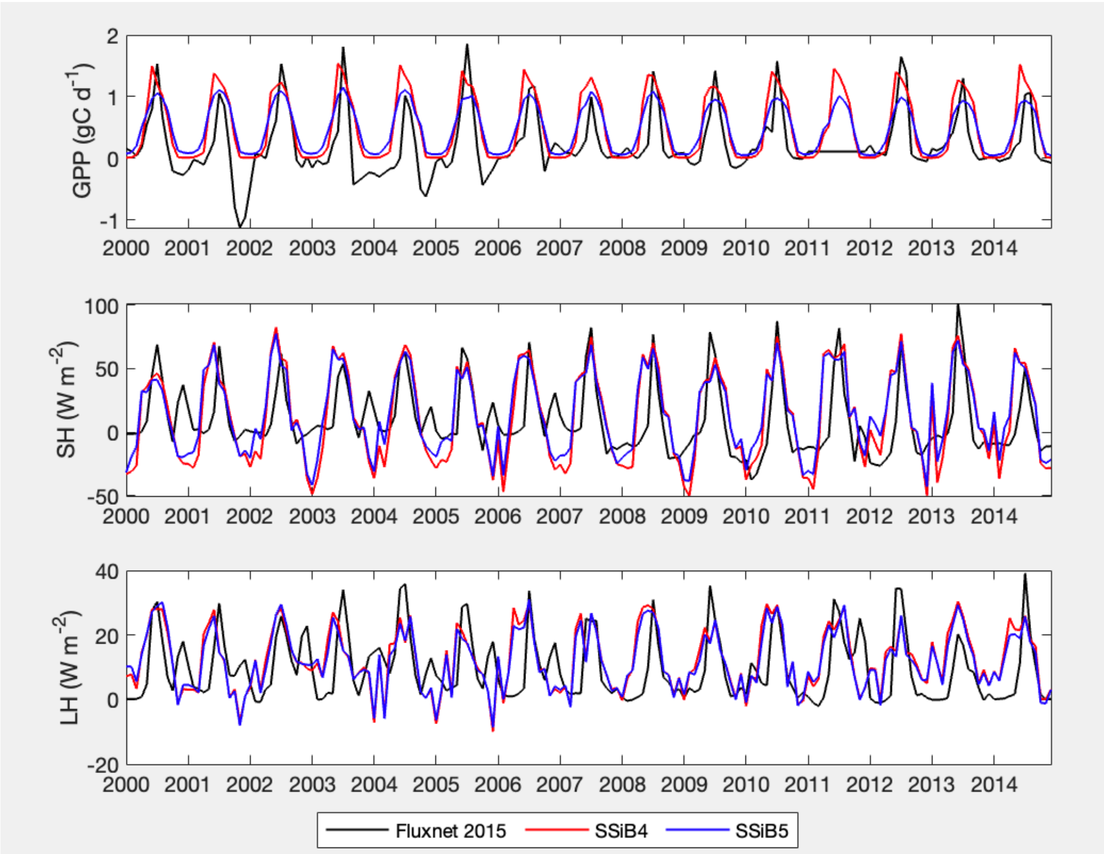

Figure R1. Simulated seasonal variations in GPP, sensible heat, and latent heat against observations at the tundra site.

As to the simulated PFT distribution issue, we had two publications (Zhang et al., 2015;

Liu et al., 2019) extensively discuss our model's simulation of the global PFT distribution and fraction coverage and compare with the satellite derived map. The simulation results are generally consistent with observation (see figure below). The SSiB5/ TRIFFID/DayCent-SOM did not produce substantial difference in the PFT distribution with a few decades of simulation.

In the revised paper, in ***new lines 494-495***, we add the following sentences: *"The SSiB4/TRIFFID-simulated global PFT distribution has been extensively discussed in Zhang et al. (2015) and Liu et al. (2019). The simulation results are generally consistent with observation. The spatial distribution from the SSiB5/TRIFFID/ DayCent-SOM did not show substantial difference and will not be discussed here."*.

Y. Liu et al.: Global vegetation variability and its response to elevated CO_2, global warming, and climate variability

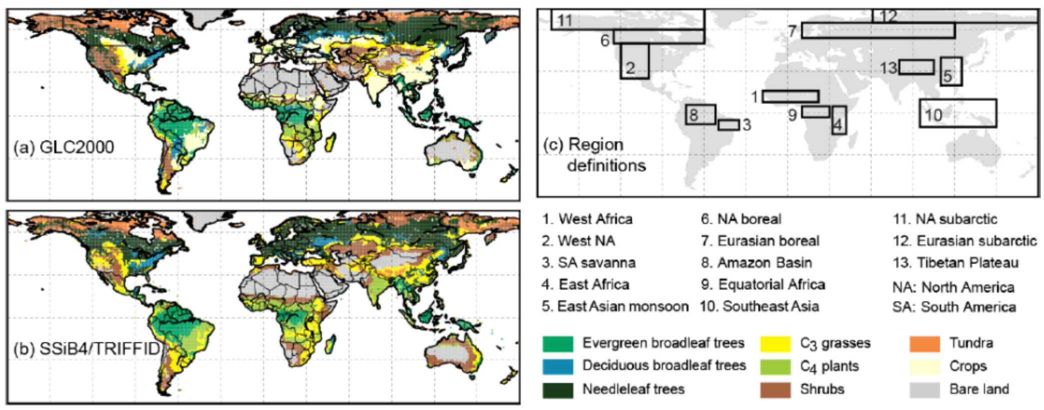

Figure 3. Dominant vegetation type comparison between **(a)** GLC2000 and **(b)** SSiB4/TRIFFID, and **(c)** region definitions.

References:

Liu, Y., Xue, Y., Macdonald, G., Cox, P., and Zhang, Z.: Global vegetation variability and its response to elevated CO 2 , global warming, and climate variability - A study using the offline SSiB4/TRIFFID model and satellite data, Earth Syst. Dyn., 10, 9–29, https://doi.org/10.5194/esd-10-9-2019, 2019.

Lund, M., Falk, J. M., Friborg, T., Mbufong, H. N., Sigsgaard, C., Soegaard, H. and Tamstorf, M. P.: Trends in CO2 exchange in a high Arctic tundra heath, 2000–2010, J. Geophys. Res., 117(G2), G02001, 2012.

Zhang, Z., Xue, Y., MacDonald, G., Cox, P. M., and Collatz, G. J.: Investigation of North American vegetation variability under recent climate: A study using the SSiB4/TRIFFID biophysical/dynamic vegetation model, J. Geophys. Res., 120, 1300–1321, https://doi.org/10.1002/2014JD021963, 2015.

I suggest list equations that calculate key processes and variables in carbon and nitrogen cycles such as GPP, SOC/SON decomposition, plant N fixation, plant N uptake, and N mineralization.

Reply: The reviewer suggested listing the major equations for the coupled model. This is a very good suggestion that should help readers understand the results. However, SSiB5, TRIFFID, and DayCent-SOM are process-based models that involve numerous equations to obtain variables such as GPP and decomposition. After several attempts, we realize that it is difficult to select a proper set of equations to provide brief and useful information for readers to have a basic understanding of the major physical, biophysical, and ecological processes in the model. A handbook is needed to accomplish this task. We apologize that we had difficulty accomplishing this task. To have a very basic understanding as a starting point, we suggest reading Zhan *et al*. (2003, Ecological modeling), Cox (2001, Hadley Tech note), and Parton (1994, a Textbook, see reference in the paper).

The manuscript needs modifications on the structure. From my point of view, it is better to move line 164-176 and line 179-191 to the Introduction part, and the order of section 3.3 and 3.2 should be reversed.

Reply: Thank you for your constructive comments and suggestions. We agree that the paper structure needs to be improved. Per your and another reviewer's suggestion, we have rearranged parts of the Introduction and Sections (2.2.1-2.2.5), which describe the model development. For lines 164-176, we have moved to "Section 2.2.2 Dynamic C/N ratio based on plant growth and soil nitrogen storage" to provide background information on why we need a dynamic C/N ratio and why we parameterize the C/N ratio this way. This will provide better presentation flow and avoid repeating (i.e., similar things appear in both Introduction and relevant sections). Similarly, we have moved lines 179-191 to Section 2.2.3 to provide background information for our parameterization of the N limitation effect on photosynthesis.

In the order of Sections 3.2 and 3.3, the following is the reason why we present Section 3.2 first. In model development, introducing a realistic process does not necessarily improve the results due to model deficiencies. Validation is necessary to confirm the model's reliability. In Section 3.2., we demonstrate that after introducing a very complex N-processing model and N limitation effect, compared with the site measurement data, the original ability of the SSiB5/TRIFFID model to simulate seasonal and interannual variability in heat fluxes is intact, even with slight improvement. This provides some confidence for our next long-term 2-D simulation presented in Section 3.3.

Moreover, more discussions on the limitations of the SSiB5/Triffid/DayCent-SOM v1.0 model and potential future developments are needed.

Reply: As discussed earlier, we noted the anthropogenic N input and large LAI bias issues for further improvement at the end of the paper. In addition, we also note the limitations of the offline simulation.

Please show some results of NlResp and NlPhen, otherwise, you should delete the descriptions of these experiments.

Reply: Thank you for this comment. We have discussed the effects of NIResp and NIPhen, but the previous presentation in the text is unclear. We conducted four experiments, namely, the NIResp, NIPhen, NIPSN, and SSiB5 experiments, in this research. Exp. SSiB5 showed a total effect, and another three experiments tested the effect of individual processes. However, only Exp. NIPSN and Exp. SSiB5 showed statistically significant results. Therefore, we mainly show the NIPSN and SSiB5 results individually, not the NIResp and NIPhen. However, the sum of these two effects also has a substantial effect on many parts of the world. Instead of showing individual results, we present the sum of these two effects. In Fig. 13b, we added a subtitle indicating that the figure shows NIPhen + NIPesp effects. In the new lines 586-590, we also added a much clearer discussion on the effects of NIResp and NIResp.

"The results from Exp. NlResp or Exp. NlPhen individually did not show a statistically significant impact. However, the sum of these two N limitations still has substantial impacts on many parts of the world, as displayed in Fig. 13b, mainly in tropical rainforests and some midlatitude regions. In addition, the differences between Exp. SSiB5, which includes three limitations, and Exp. NIPSN, as displayed in Figs. 10 and 11, also delineate the characteristics of the global impacts of these two effects at seasonal and interannual scales."

Line-specific comments and suggestions:

Line 86: Please list these plant N metabolism processes.

Reply："Metabolism" is not a proper word here. This sentence, however, has been deleted from the revised paper.

Line 104: 1982-2007 rather than 1948-2007.

Reply: Thank you for your careful review. We have corrected this.

Line 126: eight types rather than six types?

Reply: There were six pools in DayCent-SOM rather than eight. We listed the six nitrogen pools here for clarity (also as new Table 1 in the paper).

Table R2. The Nitrogen Pools in DayCent-SOM

		Aboveground	**Belowground**
Mineral N pool			Soil mineral N pools
Organic N pool	nonwoody litter pools	Surface structural N	Soil structural N
		Surface metabolic N	Soil metabolic N
	woody debris pool	Surface dead N	
	kinetically defined organic matter pools	Surface active N	Soil active organic N
		Surface slow organic N	Soil slow organic N
			Soil passive organic N

Line 197: delete " and terrestrial CX cycles"

Reply: The sentence was modified in the new line 162 as follows: *"Nitrogen is not the only dominant regulator of photosynthesis and vegetation dynamics".*

Lines 268-270: I suggest delete line 268-270 to avoid misinterpretation

Reply: Done. These lines have been deleted.

Line 284: I didn't find the paper: Yang et al., 1992

Reply: We added this paper to the References section.

Line 320: temporal resolution of vegetation dynamics is ten-day, is it too coarse for phenology (especially for the boreal forests and tundra)?

Reply: Many dynamic vegetation models use much longer time steps, such as 1 month and 1 year. For instance, the Orchidee model (as shown in Fig. 2 above) uses a 1-year time step. In SSiB5/TRIFFID/DayCent-SOM, SSiB5 provides GPP, autotrophic respiration, and other physical variables, such as canopy and soil temperatures and soil moisture, every 3 hours for TRIFFID. However, TRIFFID accumulates the GPP from SSiB5 and produces biotic C, PFT fractional coverage, vegetation height, and LAI every ten days, which are then used to update surface properties in SSiB5, such as albedo, surface roughness length, and aerodynamic resistances. Our model's time step is relatively shorter than that of many other dynamic vegetation models. The ten-day accumulation of TRIFFID occurred because, if the time step is too short, changes in vegetation conditions may be even smaller than the noise, which may cause computational instability. We performed sensitivity tests with a 5-day time step, and the results were similar. Therefore, in this study, we retained a 10-day time step to save computer time. When applying this model for fire studies, we may have to use shorter time steps.

Line 395: How do you set up the equilibrium rum at the site level? The same with global run?

Reply: Yes. The equilibrium run at the site level is the same as the global run. We added one sentence to the revised paper to clarify this.

New Line 404: *"This approach was applied for both site and global 2-D simulation".*

Line 405: Four sets of sensitivity experiments rather than six sets?

Reply: Thank you for your careful review. We have corrected this.

Figure 7: SSiB5 is higher than in (g) and (k). In these two sites, SSiB5 has lower GPP than SSiB4, why its evapotranspiration (latent heat) is higher? This doesn't seem to make sense.

Reply: Thank you for your careful review. There are three components in our model that contribute to the total latent heat flux, as shown in Fig. 7. These factors include transpiration from the canopy, direct evaporation from the leaf due to interception loss of precipitation, and soil evaporation. In the offline test, the atmospheric demand is fixed; when transpiration (and GPP in general) is reduced/increased, soil evaporation must change to satisfy the atmospheric demand. This change is not linear because the sensible heat flux also changes. As such, the latent heat change in very few cases may not be consistent with the change in GPP due to the change in soil evaporation. However, for the 13-site average, the changes in GPP and total evapotranspiration are still consistent.

Figure 9: IS LAI the mean value of GIMMIS and GLASS?

Reply: We used the GIMMS LAI in this figure and added a note as follows.

New Line 509: *"Note: OBS in LAI is GIMMS LAI."*

Line 542-543: Is there any observational evidence for this vegetation transition?

Reply: To our knowledge, there is no observational evidence for this vegetation transition.