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.
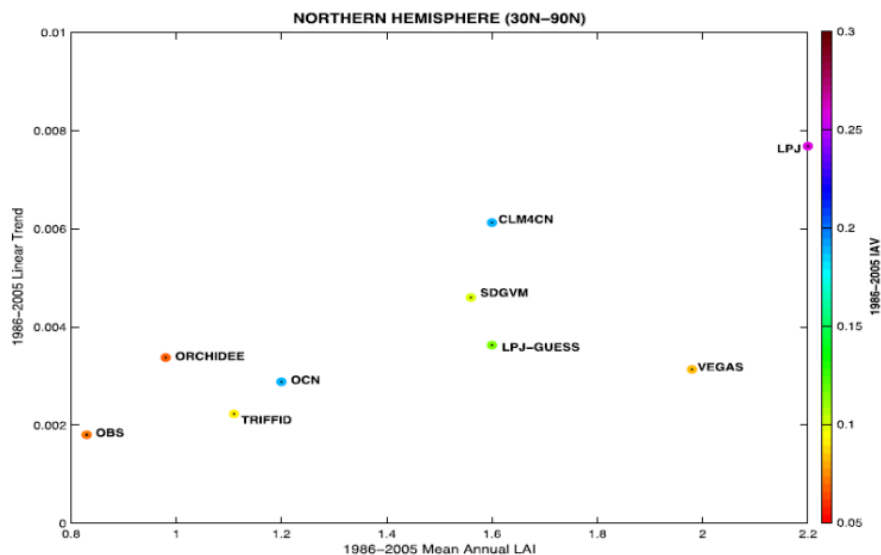
Murray-Tortarolo (2013)

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.

Oliveira, D. C. de, Oliveira, D. M. da S., Freitas, R. de C. A. de, Barreto, M. S., Almeida, R. E. M. de, Batista, R. B., & Cerri, C. E. P. 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(5), 499–508 (2021). https://doi.org/10.1080/1758300 4.2021.1977390

Heikkinen, J., Keskinen, R., Regina, K., Honkanen, H., & Nuutinen, V.. Estimation of carbon stocks in boreal cropland soils - methodological considerations. European Journal of Soil Science, 72(2), 934–945. (2021) https://doi.org/10.1111/ejss.13033

Gristina, L., Scalenghe, R., García-Díaz, A., Matranga, M. G., Ferraro, V., Guaitoli, F., & Novara, A.. Soil organic carbon stocks under recommended management practices in different soils of semiarid vineyards. Land Degradation and Development, 31(15), 1906–1914 (2020). https://doi.org/10.1002/ldr.3339

Murray-Tortarolo, G., Anav, A., Friedlingstein, P., Sitch, S., Piao, S.L., Zhu, Z. C., Poulter, B., Zaehle, S., Ahlstrom, A., Lomas, M., Levis, S., Viovy, N., and Zeng, N.: Evaluation of Land Surface Models in Reproducing Satellite-Derived LAI over the High-Latitude Northern Hemisphere. Part I: Uncoupled DGVMs, Remote Sensing, 5, 4819–4838, 2013.

Mueller, P., Ladiges, N., Jack, A., Schmiedl, G., Kutzbach, L., Jensen, K., & Nolte, S.. Assessing the long-term carbon-sequestration potential of the seminatural salt marshes in the European Wadden Sea. Ecosphere, 10(1). (2019) https://doi.org/10.1002/ecs2.2556

Zaehle, S., C. D. Jones, B. Houlton, J. F. Lamarque, E. Robertson, Nitrogen availability reduces CMIP5 projections of twenty-first-century land carbon uptake. Journal of Climate. 28, 2494–2511 (2015).

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.

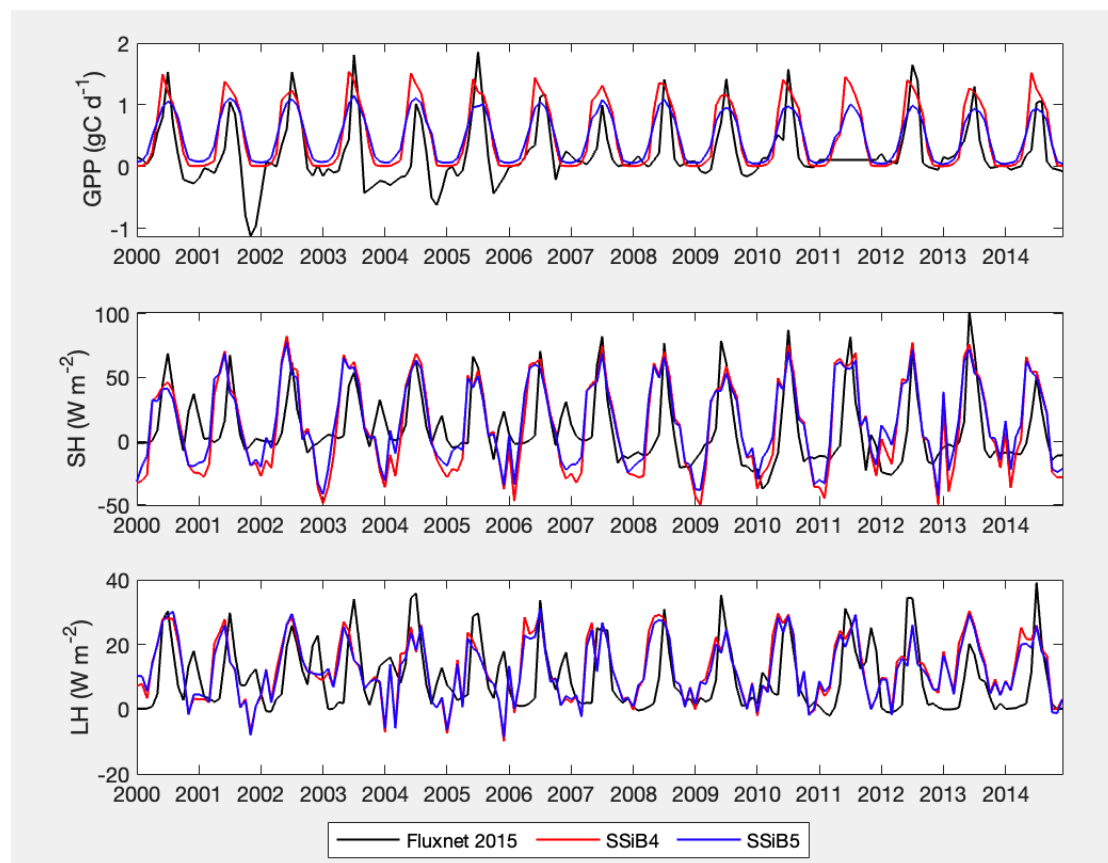| Site name	LAT	LONG	PFT	Time
Zackenberg Heath	74.47	-20.55	tundra	2000-2014

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
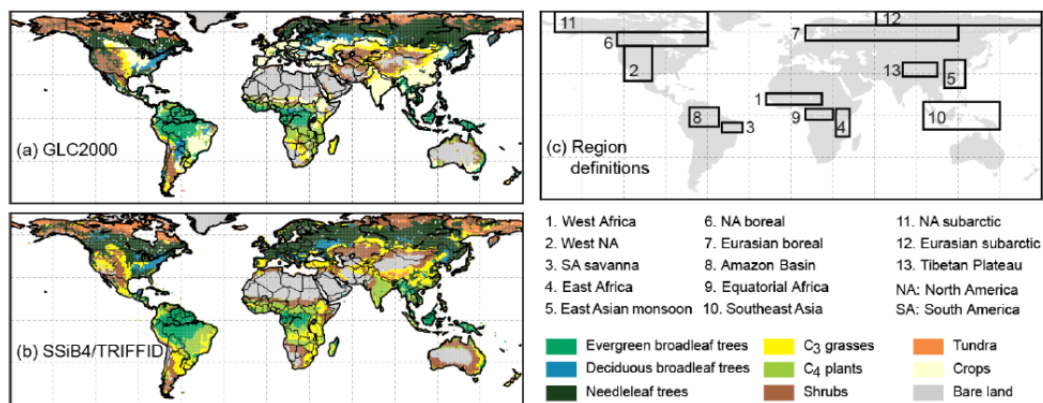

Figure 3. Dominant vegetation type comparison between **(a)** GLC2000 and **(b)** SSiB4/TRIFFID, and **(c)** region definitions.

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 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

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.