

Response to Anonymous Referee #2

The reviewer has raised many issues in his/her comments. All the questions and comments brought forward by the reviewer are carefully considered and responded to point by point. The revisions for the paper have been made accordingly in the resubmitted manuscript.

However, a number of comments/questions are repeated several times. We respond to those repeated/similar ones once, where we deemed them to be most appropriate, to make the answers more concise and easier to follow. In this way, we can provide a more comprehensive response for those questions with a clear overall picture rather than answer piece by piece spreading everywhere, which should help reviewer and editor easily check whether we properly address the comments.

For this purpose, we add numerical numbers, such as 1., 2., etc., to each of the reviewer's "General comments", so the reviewers/editor can easily find out where our answers are located for those repeated questions.

General comments

General Comment #1. Introducing a prognostic nutrient cycle, here the nitrogen cycle, into a land surface model (LSM) or a dynamic global vegetation model (DGVM) is a challenging task. As the importance of nutrient limitation on productivity has been clear for a while and we have gone from one LSM with a prognostic N cycle in CMIP5 to several in CMIP6 this is a step all LSM are and need to take. So, for undertaking this task and finishing an LSM that has included the N cycle I congratulate the authors. But, I'm very disappointed in the complexity and description of the N cycle that has been incorporated into SSiB/TRIFFID. Incorporating an N cycle myself into a model more than 10 years ago, I expect that current implementations should be of a higher standard. I have reviewed several N implementations over the years and expect a gradual improvement with the newest findings, especially from different MIPs like the Davies-Barnard et al. (2020) study that you have cited in your manuscript and newer model

description papers (e.g. Wiltshire et al. 2021, Asaadi and Arora 2021). Also, there is no validation against any N-related properties or fluxes, only against GPP, LAI (difficult quantity to measure), SH, and LH. I also think that many of your statements in the introduction on the current state of N cycling in LSMs and DGVMs are wrong as are some of the interpretations of your results.

Reply:

The General comment #1 contains the Reviewer's main concerns. Based on the CMIP5 and the CMIP6, the Reviewer acknowledged that introducing the nitrogen cycle to a LSM and GDVM is a great challenge and congratulates our work. But then the reviewer raised several major issues: (1) Disappointment concerning the complexity and description of the N Cycle that we introduced to SSiB/TRIFFID; (2) Expecting improvement with newest findings compared to recent model-intercomparison paper Davies-Barnard et al. (2020) and others; (3) No validation against any N-related properties or fluxes, only GPP, LH, and SH; (4) Improper statement in the Introduction on current state of N cycling in LSM; (5) Wrong interpretation of our results.

Since for some of the above issues, the review itself includes more detailed explanations in general comments #2-#5 and/or specific comments, we will provide our response when we respond to the comments there. For instance, Issue (4) and specific comments (#1, #3, #5, #6, #15) will be addressed in our response to General Comment #3, where the reviewer more clearly indicates what he/she believes are wrong statements; Issue (5) and specific comments (#14, #23, #24, #26, #29, #31,#32) will be answered in General Comment #2, where the reviewer gave a clear indication, which results are not properly interpreted. For Issue (1), we agree with the reviewer that we need to improve the presentation for our methodology and will indicate where we will improve throughout the entire response. For the "complexity" issue, we will address in our response to special comment #3 where the reviewer more clearly indicates what this mean. Here we only respond to issues (2) and (3), which are related to the key parts of this paper.

In issue (2), the recent papers, Davies-Barnard et al. (2020), Asaadi and Arora (2021),

and Wiltshire et al. (2021) are specifically mentioned as examples to be compared with our model development. We will refer to these three papers as DB2020, AA2021, and W2021 for simplicity hereafter. Table 1 of DB2020 checks 7 aspects in C-N coupling that they list as the most important aspects. We provide our response to the reviewer's comments based on these 7 aspects to see if our approach provides some new development.

(a). "N effect on GPP". This is listed as the first aspect in coupling in the DB2020 Table 1. In our paper, this is also considered as a major issue. DB2020 Table 1 lists 2 out of 5 models have no direct effect, one downregulates GPP based on N allocation, and another uses quite different approach from us. Only the LPJ-GUESS models N effect on GPP through regulating Rubisco capacity, similar to our method. The following discussion outlines the development that we made to implement the N effect on GPP in the C-N coupling:

(a.1). Our approach applies the N-limit to Rubisco capacity, which is different from all but one (LPJ-GUESS) of the models listed in DB2020 Table 1. Moreover, when calculating the N-limit to Rubisco capacity, we introduce a dynamic C/N approach to influence the N-limit effect on photosynthesis, while others in DB2020 Table 1 only used a C/N ratio to affect the N process (not used for the explicit effect on the GPP based on the information on DB2020 Table 1). AA2021 also applies the C/N ratio to impose the N-limit effect on photosynthesis. However, its approach and parameterization are completely different from ours. In W2021, the C-N coupling seems not even explicitly presented.

(a.2) Because plants can adjust the relative allocations of C and N during N uptake via N remobilization and resorption to reduce the impact of N limitation, we assume that C/N ratio varies within certain available N range, and N limit effect on photosynthesis only applies when the N availability is lower than the minimum amounts of N necessary for the potential *NPP*. As such, our approach is different from any other model listed in DB2020 and AA2021.

(a.3). Furthermore, we tested three different approaches that used the N-limit to affect the GPP or NPP. The test results demonstrate that two approaches, which directly

apply the N-limit to GPP or NPP that have been used in some LSMs (Ghimire et al., 2016; Zaehle et al., 2015; Gerber et al., 2010; Oleson et al., 2013; Wang et al., 2010), produced large errors against observation. Our approach yields the best results among these three methodologies. As such, our paper has provided useful information for future research in our community on this subject.

(b). N effect on autotrophic respiration is also listed in DB2020 Table 1 as an important aspect for C-N coupling. As indicated by DB2020 Table 1, most models' maintenance respiration is related to plant N content. In our approach, the maintenance respiration is associated with the photosynthesis process. Therefore, the C/N ratio that affects the photosynthesis will also affect the maintenance respiration. Furthermore, since the respiration rates at any common N concentration were consistently lower in leaves than in stems or roots on average (Reich et al., 2008), we introduced two PFT-specific parameters ($ResA_s$, $ResA_R$) based on field observations (Wang et al., 2006; Yang et al., 1992) to adjust root and stem respiration relative to the leaf respiration. This approach is also different from some models listed on DB2020 Table 1. Both AA2021 and W2021 did not address this issue.

(c). For plant N uptake. Models in DB2020 Table 1 are either determined by an N model, such as FUN, or by some parameterization. This N uptake is mainly determined to maintain optimal leaf N for photosynthesis but is limited by soil inorganic N availability. In our approach, actual N uptake by plants is determined by soil inorganic N availability predicted by DayCent-SOM and plant N status.

(d). several other issues listed in DB2020 Table 1 include: vegetation pool C:N stoichiometry; re-translocation of N from shed leaves, and biological N fixation and ecosystem N loss (i.e., denitrification, leaching, and fire). Although these N processes are not shown in the figure of our C-N coupling framework (Figure 1 of our manuscript), DayCent in our coupled model takes care of them (except re-translocation of N from shed leaves). Some models in DB2020 Table 1 also use DayCent soil organic matter cycling algorithms. We have no knowledge of how the DayCent version in these models deals with these issues. However, these issues are NOT the focus of this paper.

Based on the above discussion on key coupling aspects (a) and (b) when compared with recently published papers, we have taken different approaches from all other models and our C-N coupling strategy and methodology that clearly have some unique features with improvements. Especially, we include the effect of N limitation on the phenology, which is not listed on DB2020 Table 1 and we believe it is an important aspect in C-N coupling, particularly during the growing season. That said, our framework not only considers the N limitation on the general plant photosynthesis process but also emphasizes the N limitation effect during the growing season, which is a new approach in our framework. As such, we respectively disagree with the reviewer's statement that our approach has no improvement compared with others, in particular when comparing our paper and recently published papers in C-N coupling, such as SS2021 and W2021 that the reviewer referred to for comparison.

However, we do agree with the reviewer that our presentation needs to be improved (Issue (1)) to highlight these new developments. In the revised paper, the presentations in Sections 4.2 and 4.3 have been revised to make these points much clear. On line 363, 386-388, 424-430, 455-456, we have revised the paper and emphasize our new approach.

For Issue (3), we agree with the Reviewer that validation against N-related properties or fluxes is very important. However, the global observation of N-deposition or soil N are still unavailable. DB2020, AA2020, and W2021 all have no such comparison/validation. On other hand, our paper provides validation against LAI, SH, and LH, which have not been done by most other C-N coupling papers. Nevertheless, the validation of these components is crucial for the SSiB5/TRIFFID/DayCent-SOM because SSiB5/TRIFFID is mainly used in a coupled atmosphere-ocean-land model for the climate and climate change studies with the focus on the carbon and water cycles. We do not understand why this progress cannot even be considered as 'a gradual improvement' and becomes an issue for the reviewer. Meanwhile, the reviewer asks us to do something with data that are even not available yet. Even the reviewer-suggested

papers also have not done the validation that the reviewer requested from us.

In this part, the reviewer indicates that “LAI (difficult quantity to measure)”. We feel this criticism of LAI is unwarranted. Every surface variable measurement has uncertainty associated with it. The remote sensing of LAI is a big scientific field and hundreds of related papers have been published. If the reviewer tried to say the LAI measurement should not be used, then solid evidence based on the peer reviewed papers should be provided. We aren’t sure that a journal should allow the use of a personal feeling/preference as a criterion.

References:

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General Comment #2. The focus of the experiments and the analysis of them is confusing to me. The highlight of this study is the implementation of dynamic C:N ratio (eqn 1), N limitation of V_{max} (eqn 7-8), R_a (eqn 11), and phenology (eqn 14) and the results in figures 8f, 13a and 13b. These should be highlighted much more. Especially the results in figures 8f, 13a and 13b should be put together in one figure and explained in depth! Why you decide to introduce the strange N limitation directly on NPP and GPP and let it take such a large part of the study is puzzling to me. It makes the study confusing. I would exclude these and especially as you mention yourself that this isn't the way N limitation works (lines 382-384).

Reply:

The reviewer raises two issues here: (a). Why discuss the N direct effect on GPP and NPP; The reviewer feels the test “strange” and “confusing”. (b). Why not put Figures 8f, 13a and 13 b in one figure.

For the first issue, in section 3.3 “Global 2-D offline control run and sensitivity runs”, we introduce three sensitivity studies including the test for direct effect on GPP and NPP. The paper indicates that “these approaches have been commonly used in recent C-N coupling for the N limitation as discussed in Introduction (Ghimire et al., 2016; Zaehle et al., 2015; Gerber et al., 2010; Oleson et al., 2013; Wang et al., 2010)”. That’s why it is worth making a comparison for these approaches with ours. (Line 256-258). In section 5.3, we present the comparison of these three results, and show that direct effect on GPP and NPP produced large errors thus this is not a proper approach for the SSiB5/TRIFFID. Because these are common practices in current C-N coupling, we hope that our information on these testing results could be useful for the community’s future practice regarding this issue.

Based on the reviewer’s comments, we revised the paper to make those points clearer. In the revision, in Section 3.3, we modify the title to “Experimental Design of Global 2-D offline control run and sensitivity runs”. On line 253-255, we indicate that the Exp. GPP and Exp. NPP are common approaches in some models, and we will compare

these approaches against observation. We have added more discussions on the comparison between these three approaches and possible reasons why these differences occur (Line 597-600).

For the second issue: why not put Figures 8f, 13a and 13 b in one figure. Because each figure, such as Figure 8c or Figure 13c, represents an effect of introducing one process, such as limitation on photosynthesis, limitation on phenology, etc., when we discuss each factor's effect, we have to use several panels in one figure to show the results and error with/without this N-limit. It is impossible to make a presentation with only one set of figures for all these effects. Furthermore, if putting all these into one figure, the readers have to keep going back and forth looking at the figure and the text. It is very inconvenient. Of course, when readers finish reading all three effects and want to make a comparison between these three, putting figures together seems more convenient. We understand the reviewer's concern. However, overall, the way that we present is still relatively easy for readers to follow than putting all these figures together.

We agree with the reviewer on one point, i.e., more in-depth discussions are needed. So in line 634-643 we add more discussions as following:

“N limitation on photosynthesis, which is shown in the difference between Exp. SSiB4 and Exp. NIPSN, results in a dominant decrease in tropical Africa and boreal regions (Figs. 13a, c), but N impacts on phenology and respiration dominate the decline in GPP in tropical forests (Figs. 13b, d). Moreover, studies show that there is a transition to N limitation at higher elevations in some lower-latitude regions (i.e., the Tibetan Plateau) (Du et al., 2020). This pattern is also captured in this study (Figs.13a, c). Water and nutrient limitation jointly affect leaf phenology, leaf traits, and water use strategies (Geekiyana, 2019). For both C3 and C4 grasses, N addition caused a strong increase in biomass and resulted in more severe drought stress, leading to a change in the dominant photosynthetic limitation during the growing periods (Zhong et al, 2019). Although N addition increased antioxidant enzyme activities (which can be seen as Vmax) and protective solute concentrations, the phenology did not fully recover to pre-drought levels by the end of the re-watering period (Zhong et al, 2019). This may

explain why tropical areas affected by Ra and phenology N limitations but not photosynthesis (Fig. 13)."

General Comment #3. The tone and description of existing models with N limitations are a shock to me. The feeling I get when reading the study is that all other models have various deficiencies (L56-57) and here is a model that has resolved this. I hope this wasn't the message the authors were looking for, but many of the statement about other models in the study is wrong and misleading.

Reply:

To make things clear, we copied the text from Line 54-57 here: "In the latest 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) with various deficiencies. Among these models including N processes, most of them still focus on microbial N dynamics in soil. The consideration of C-N coupling in plant N processes is not sufficient (Ghimire et al., 2016; Goll et al., 2017; Thum et al., 2019; Yu et al., 2020; Zaehle et al., 2015; Zhu et al., 2019)."

In a model development paper, the introduction normally points out the issues in current model development on this subject. Otherwise, there is no need for the model development. But that does not mean the paper's authors will develop a super model to solve every problem. The statements here actually are very general and use the words "some deficiencies" and "not sufficient". The word "deficiencies" was used in the review paper (Arora et al. 2020) Why did the reviewer feel this statement indicated our model solves all the problem? We do not understand why the reviewer felt so irritated. The reviewer apparently is a modeler. Could the reviewer claim his/her model has "no deficiency" and is "sufficient" to solve every problem? No one/no model in the world can claim it solves every problem. A paper always **TRIES** to find an approach to solve some issues (not every issue).

This paper is based on a student's Ph.D. dissertation. Most materials in the introduction were prepared before 2020. In the revised text, we include the latest progress in 2021

and 2022 to emphasize the consideration of C-N coupling not only in soil but also in plant N cycle. We have revised lines 51 to 62 in Introduction to more properly present the current model advances in C-N coupling by including the latest model developments.

General Comment #4. For the global comparison of GPP and LAI any limitation on GPP and LAI would improve the model. The important thing with introducing N limitation is to get it in the right locations. Otherwise, your model is missing something. Just reducing GPP and LAI as a global average can be done by just lowering your standard PFT Vmax value. Could probably get similar results just by optimising it.

Reply:

The reviewer's comments here are not justified. Our goal is to pursue a process-based approach to get the right answer for the right reason. That's why we introduce more physically/biologically based parameterization to better simulate carbon-water-nitrogen cycle. The reviewer also claimed "The important thing with introducing N limitation is to get it in the right locations". In our response to General Comment #2, we already discussed this issue.

However, to prove it is a right approach, we have to prove this approach produces the right answer. Since there is no global observation for the N flux, we compare our results with GPP, LAI, SH, and LH observation for the simulation in the carbon and water cycle. We do not understand what's wrong with this validation.

To further demonstrate the right answer is from the right approach, in two of our designed experiments, the N-limit is directly applied to GPP and NPP, which actually is tuning the results and is what precisely the reviewer criticized: lower the results to get wright answer. Our experiment shows that this tuning did not yield adequate results, which demonstrates that due to complex feedback processes in the Earth system, if improper processes are used, the optimization may not be able to easily get a right answer. This is the message that we are trying to deliver and in the revised paper, we further emphasize our idea on lines 256-258 to avoid misunderstanding and confusion.

General Comment #5. In general, it would have been nice to see some perturbation experiments to see how SSiB4 vs SSiB5 would react to e.g. increased CO₂ instead of the N limitation on NPP and GPP experiments.

Reply:

It is a good suggestion; but we feel perturbation experiments are out of the scope of this paper. Conducting such perturbation experiments and analyzing results would require a substantial amount of effort. We had a paper (Liu et al, 2019) that discussed the effect of CO₂ increase on the vegetation dynamics, including GPP. But that work was completely different from the issue that we want to address here. In the future we will plan simulations to evaluate CO₂ effects on C-N coupling.

References:

- Liu, Y., Y. Xue, G. MacDonald., P. Cox, Z. Zhang, 2019: Global vegetation variability and its response to elevated CO₂, global warming, and climate variability - A study using offline SSiB4/TRIFFID model and satellite data. *Earth System Dynamics*, 10, 9-29, <https://doi.org/10.5194/esd-10-9-2019>

I think this model description paper has many deficiencies. Several N-related processes aren't documented and described properly. I would like to see a major revision and I hope my comments will be of some help.

Response:

This comment is confusing. If several N-process are “not documented” in our paper, then they would not be “described improperly”. The reviewer did not indicate clearly which N-related processes are not documented and that should be added. We are also unclear which N-processes are described improperly. In our paper, all the N-process that are not directly related to the C-N coupling are not presented. The Century and DayCent models have had hundreds of papers describing their representation of C-N cycling within soil organic matter. Anyway, we have revised the DayCent Section (Lines 139-150), added citations, and hope that will help.

Specific comments

(1) L20-26 – Here you describe your “new” approach of N cycle modelling with dynamic C:N ratio in plant tissue, N limitation on productivity, growth (phenology), and autotrophic respiration. Models have had these features for at least 10 years now (Table A1 in Zaehle et al 2014 describes models participating in the FACE-MIP that have flexible C:N ratio and how N limitation affects growth: CABLE – Wang et al. (2011), DAYCENT – Parton et al. (2010); EALCO – Wang et al. (2001); GDAY – Comins & McMurtrie (1993); ISAM – Yang et al. (2009); LPJ-GUESS – Smith et al. (2014); OCN – Zaehle & Friend (2010); TECO – Weng & Lou (2008)). So, I don’t think you can call this a new approach.

Reply:

In our response to the reviewer’ general comment #1, we have responded to the reviewer’s comments: what’s new in our approach. The reviewer raises the same issue again here and suggests that what we report in this paper was all reported in Table A1 in Zaehle. (2014). Table A1 in Zaehle lists 6 important issues: Plant N turnover, Soil N turnover, Ecosystem N losses, Plant C acquisition, Plant N acquisition, and Plant growth. Most of them are included in Table 1 of DB2020. None of these 6 issues are closely relevant to what we presented for the C-N coupling, but more focused on other N-processes . Please see our response to General Comment #1 Issue 2.

As to the dynamic C/N ration, Zaehle (2014) mentioned the “flexible C/N ratio” in the text with some qualitative description, no detail regarding the “flexible”. In our paper, we have several equations to quantitatively present what’s the dynamic C/N ratio is in our model and how to apply this concept for N-limit in photosynthesis and phenology. One cannot reject a paper discussing a concept, say variable C/N ratios, because other models also mention flexible C/:N ratios for various calculations. One must look at the substance. In the revised text’s line 163-164, 292-295, we also modify the sentence indicate the flexible C:N ratio has been used in modeling vegetation growth.

(2) L30-31 – Here you mention that you have more consistent respiration compared to observations. I can’t find any results related to respiration in the manuscript.

Reply:

Sorry for the mistake and thanks for pointing this out. We have deleted “respiration” and the sentence now is as following in line 29-30:

“The results show a general improvement with the new plant C-N coupling framework, with more consistent emergent properties, such as GPP, and leaf area index (LAI), ~~and respiration~~, compared to observations.”

(3) L39-47 – Here you state that “current” LSMs are oversimplistic and overestimate C sequestration under climate change. The references for these statements are very old and have been dealt with by the model developments (e.g. nutrient cycles). The Hungate et al (2003) estimates have been discussed in many studies (e.g. Smith et al 2014). I would say that a more current issue is that the models are getting too complex. So, I don’t agree with this section.

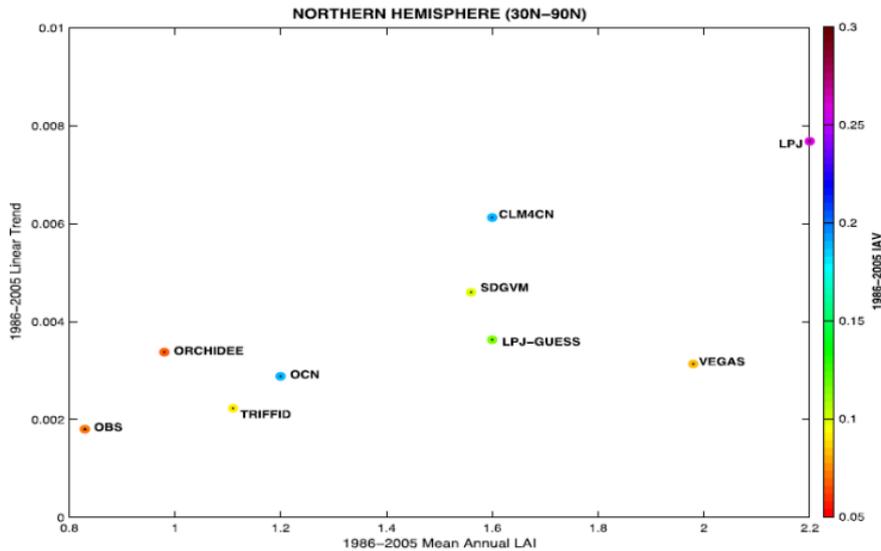
Reply:

First, we have to point out that the statements using “oversimplistic” were cited from other papers. We have provided the citation and added text to line 42. Furthermore, the word “oversimplistic” in the paper means the parameterization of some processes in the model are too simple (unrealistic). The term “complex” in the reviewer’s statement, per our understanding, refers to too many processes in current models. These are two different issues. In the revised paper, we cite additional papers that show the progress in recent years in line 51-59.

For the statement “overestimation”, recent review papers seem to confirm that overestimation of C sequestration is still an issue with some 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 dynamic vegetation models’ performance from a CMIP model intercomparison and a figure from their paper is attached below. Based on the figure, it is very clear the issue is quite serious with very large bias. More recent papers as cited above also confirm this shortcoming. So it is important to overcome such

large bias, which is also the motivation for us to introduce the N process into the Earth System Model.

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.



Anav et al. (2013)

Furthermore, the N-C coupling is a complex process. It is hard to use “too complex” without substance to support this declaration. The goal of development of science is always to pursue the truth, which may need to make the model more complex or simpler. It seems no one uses “simple” or “complex” to guide scientific development. Meanwhile, we want to emphasize that in model development it is very common to point out/suggest some issues to be improved for further development. This is by no means to criticize other models and/or show our model is superior and has solved all the problem.

We do not understand the logic here: the reviewer only disagrees with two sentences, yet says “disagree whole section”. Anyway, we have modified the text as following in line 40-45 to avoid misunderstanding.

“Current land surface models have large uncertainties in predicting historical and

recent C exchanges (Beer et al., 2010; Richardson et al., 2012; Zaehle et al., 2015). The parameterization of some processes have been criticized for being oversimplified from an ecological point of view (Ali et al., 2015; Lawrence et al. 2019; Reich et al., 2006) and the dynamic vegetation models tend to overestimate terrestrial C sequestration (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.”

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
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- J. Heikkinen, R. Keskinen, K. Regina, H. Honkanen, V. Nuutinen, Estimation of carbon stocks in boreal cropland soils - methodological considerations. *European Journal of Soil Science.* **72**, 934–945 (2021).
- L. Gristina et al., Soil organic carbon stocks under recommended management practices in different soils of semiarid vineyards. *Land Degradation and Development.* **31**, 1906–1914 (2020).
- 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.
- P. Mueller et al., Assessing the long-term carbon-sequestration potential of the semi-natural salt marshes in the European Wadden Sea. *Ecosphere.* **10** (2019), doi:10.1002/ecs2.2556.
- S. Zaehle, 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).

- (4) L50-54 – Here you refer to Davies-Barnard et al. (2020) study, but then you don't mention the LSMs in that study when you list models with various representations of N processes e.g. Lawrence et al. 2019; Wiltshire et al. 2020; Smith et al. 2014.

Reply:

We have added these references to the revised paper in line 51-55.

“the N cycle and its effect on C uptake in the terrestrial biosphere has been incorporated in land surface models (LSMs) of ESMs (Davies-Barnard et al., 2020) with various representations of N processes (Ali et al., 2015; Asaadi and Arora 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, 2021; Yu et al., 2020; Zhu et al., 2019).”

- (5) L64-66 – I don't agree with the statement “N limitation is represented as instantaneous down-regulation of potential photosynthesis rates based on soil mineral N availability” as most models use the current leaf status of nutrients to regulate potential photosynthesis rates and not the current mineral N availability in the soil. But I think this study does exactly that with eqn 8. If I'm not misunderstanding something, this statement criticises other models for something that they don't do, but that this study then does.

Reply:

The reviewer questions why we criticize “something that is this study does”. The reviewer did not understand the main point we were trying to make. The issue that we try to emphasize in this sentence is “*instantaneous down-regulation*” not whether using N status in soil or leaf for calculation, which from our point of view, is not an issue. In the revised text, we modify the word from “based on soil” to “based on soil or vegetation”. In special comment #17, the reviewer seems to understand the issue is

“instantaneous down-regulation”. We will have more response regarding “Instantaneous down-regulation” in special comment #17.

(6) L75-77 – Again, most models do have a flexible tissue C:N ratio. See Davies-Barnard et al. (2020) and Zaehle et al (2015) for a large selection of models with flexible C:N ratios.

Reply:

Please see our response in [General Comment #1](#) and [Specific Comments #1](#).

(7) L104-106, L146-148 – As SSiB/TRIFFID is a dynamic vegetation model it would be interesting to know how the N cycle has affected the PFT competition and hence the PFT fractional coverage.

Reply:

It is a good suggestion. To show statistically significant results for the PFT competition and PFT cover changes, we would need to have hundreds of years’ simulation. It will be a subject for another study.

(8) L128 – Only two litter pools (metabolic and structural)? Is this sufficient for forest ecosystems with woody litter? DayCent have been mainly developed for agroecosystems and when used for the forested system then the addition of additional litter pools (fine and coarse woody litter pools) has been suggested by e.g. Kirschbaum et al (2002). How do you split woody litter into metabolic and structural litter and how are the litter pools compared to observation/estimates? The residence time of woody litter would be very short with only metabolic and structural litter pools.

Reply:

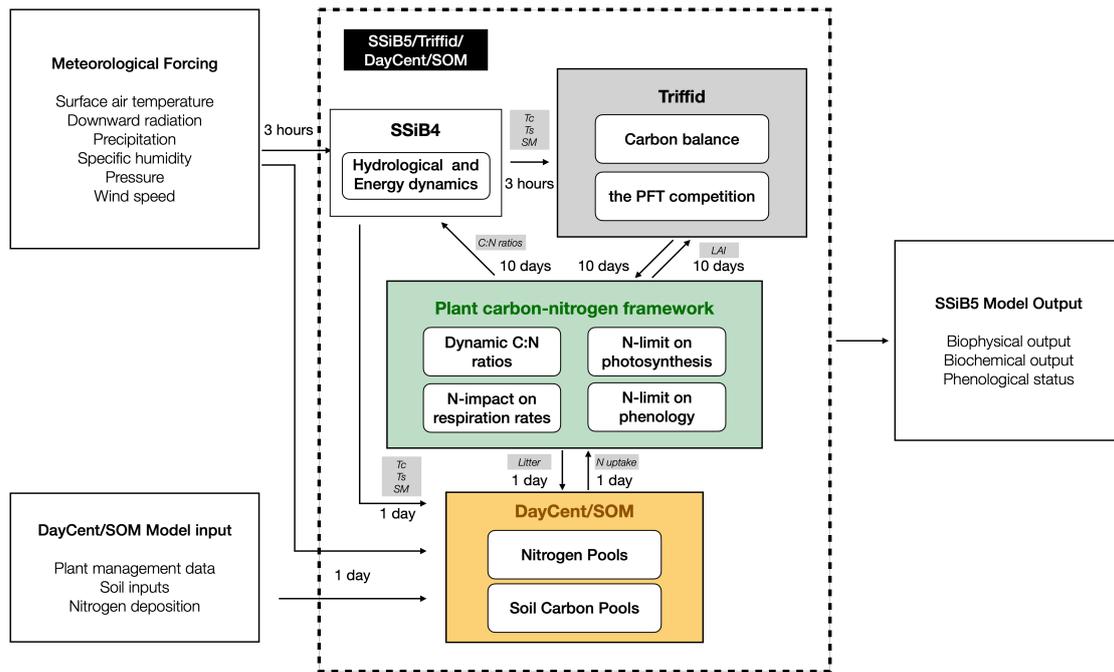
The reviewer is an expert in N modeling and interested in some details in N modeling. For this coupling paper, we only emphasize processes that directly link to the coupling. Any specific processes in SSiB, TRIFFID, and DayCent, cannot be explained in detail here, because there are hundreds if not thousands of biophysical, ecological, and

biogeochemical processes in this coupling system. But per the reviewer's comment, we have modified the DayCent-SOM part as following with more information as reviewer asked in line 139-150:

“DayCent-SOM, a subset of DayCent that excludes the plant growth, soil hydrology, and soil temperature subroutines, consist of soil mineral N pools (ammonium and nitrate) and six types of organic C and N pools consisting of two non-woody plant litter pools (metabolic and structural), three coarse woody debris pools (from death of large wood, fine branches, and coarse roots), and three kinetically defined organic matter pools, (active, slow, and passive); all types of organic pools except the passive pool have both above-ground and below-ground counterparts. Non-woody plant litter is partitioned into structural (lignin + cellulose) and metabolic (labile) litter based on the lignin:N ratio of plant material (Parton et al. 1994). The coarse woody debris pools decay in the same way that the structural pool decomposes with lignin and cellulose going to the slow soil organic matter pool and the labile fraction going to active soil organic matter pool. Each type of organic pool has its own intrinsic rate of decomposition, modified by temperature and moisture effects (Parton et al. 1994). Additionally, the decomposition rates of the structural material and coarse woody debris pools are functions of their respective lignin fractions. DayCent's litter decay model has been validated using extensive data from the LIDET litter decay experiments from all over the world (Bonan et al. 2013).”

(9) Figure 1 – From the figure, it seems like SSiB4 doesn't receive any information from any other part of the model, but the N cycle does affect GPP. Is the GPP from SSiB4 only affected by N limitation after it is calculated? If this is the case, how would this then affect the water demand for photosynthesis? Wouldn't WUE be wrong if GPP is downregulated after the water demand is met? Too much water is used then as GPP is lowered afterwards.

Reply: This is a very good comment. In Figure 1, we missed that part and now the feedback path has been included.



(10)L159 – Why validate against sensible and latent heat instead of some N variable?

Reply:

Please see our response in General comments #1.

(11)L205-207 – Did you experience any runaway effects on soil C/N pools during the 2000 year offline spinup of the soil? And if so, how was it handled? How did the initial condition differ between the C-only and C:N versions of the model? Litter input and water usage (soil moisture), affecting decomposition, should be different depending on N limitation.

Reply:

Both SSiB4/TRIFFID and DayCent-SOM have well-developed methodology for initializations. For instance, Zhang et al., (2015) and Liu et al (2019) discussed the SSiB4/TRIFFID initialization methodology/procedures in detail. DayCent soil C and N pools were initialized for each grid cell by running the model for thousands of years assuming grid-cell specific native vegetation and climate inputs, an established process that has been used to initialize both Century and DayCent for many years (for example, Schimel et al. 1996). Both SSiB4/TRIFFID and DayCent-SOM established equilibrium

conditions before they were coupled. Since the initialization methodologies were well developed, they were not presented in this paper. Moreover, we also indicated in the paper that although the model runs were from 1948 to 2007, we only present the results from 1982-2007 to avoid some possible transient behavior (line 271-274) for SSiB4/TRIFFID/DayCent-SOM when the model is first run in a coupled mode. In our simulation, we did not find any runaway situations.

Reference:

- Schimel, D.S., B.H. Braswell, R. McKeown, D.S. Ojima, W.J. Parton, and W. Pulliam. 1996. Climate and nitrogen controls on the geography and timescales of terrestrial biogeochemical cycling. *Global Biogeochemical Cycles* **10**:677-692.

(12)L209-213 – This whole section I have no idea what you are talking about. Most models only focus on the long-term effect when doing global studies? I would definitely argue the opposite. Just look at already cited papers and e.g. Peano et al. (2021), Boysen et al. (2021).

Reply:

The following is our statement: “Most current Dynamic Global Vegetation Models (DGVMs) are **mainly focused on** long-term (decadal to thousands of years or even longer) simulations at global scale”, which is in reference with the studies using biophysical models, which mainly focus on hourly, daily, seasonal, interannual, or decadal scales. The Reviewer indicates the opposite is true. But we sincerely doubt that many people will agree that dynamic vegetation models and biogeochemical models mainly focus on diurnal and seasonal scales. We are not clear what’s “the opposite” in the reviewer’s mind. The Boysen et al. (2021) study and many other papers that reviewer lists in his/her comments confirmed our statement. Peano et al. (2021) presents the seasonal cycle, just like our paper did, but this is just one exception.

(13)L217-218 – Why not compare the results to any N-related properties?

Reply:

Please see our response in [General comments #1](#).

(14)L241-243, Table 2 – It can be a little confusing what you mean by all four C-N coupling processes. First, you introduce 3 sets of experiments where $f(N)$ limits V_{max} (1, eqn 7), NPP (2, eqn 9a), and GPP (3, eqn 9b), then you say that the fourth experiment is including all four C-N coupling processes (Dyn C:N ratio, V_{max} , R_a , and phenology). Normally you would expect all experiments to be connected so you can get the difference between them to get information on the feedback between them, but here you do some very strange things. I don't understand why you do experiments 2 and 3 when you state that this is not the correct way of doing it (lines 382-384). It would be much more interesting if experiments 2 and 3 were looking at R_a (eqn 11) and LAI (eqn 12-14) N limitations separately.

Reply:

The following is our clarification:

- (1). For the reason why we need to test the direct effect on GPP (eq. 9a) and NPP (eq. 9b), we have provided explanation in our response to General Comment #1 under (a.1).
- (2). We did the individual experiment for eqn. 11(Exp. NLResp) and eqns. 12-14(Exp. NLPhen). But although Exp. SSiB5 (the total effects, eqs. 1-14) and Exp. NIPSN (photosynthesis, eqs. 7) produced significant difference, the individual results for Exp. Resp or Exp. Phen over many areas are not that statistically significant. Therefore, we did not present these experiments individually. However, from the difference between exp. SSiB5 and Exp. NIPSN, readers should have ideas about the magnitude and major areas of their effects.

In the revised paper, we add the experiments that test eq. 11 and eq. 12-14 in line 261-264-and Table 2 as following. We also include some discussions on the major effects based on the difference between the effects from SSiB5 and SSiB NIPSN.

100-year equilibrium <i>Initial condition</i> →	Real-forcing simulation 1948-2007
<i>Fixed climatology forcing</i>	<i>Transient forcing</i>
Control experiment	SSiB4: Control experiment NLPSN: Nitrogen limitation on photosynthesis (Vmax), Eq.7 NLNPP: Nitrogen limitation on photosynthesis(NPP), Eq.9a NLGPP: Nitrogen limitation on photosynthesis(GPP), Eq.9b NLResp: Nitrogen impact on Respiration rate, Eq.11 NLPhen: Nitrogen limitation on Phenology, Eq. 14 SSiB5: including all four nitrogen processes

(15)L263-264 – I don't agree with anything written in this section. The authors can't really read any other modelling description papers on how the N cycle has been implemented to be able to make this statement.

Reply:

Sorry, we do not understand what the reviewer is trying to communicate here. For completeness, we copy our sentence form L262-L265 here:

“For Following plant N-uptake from DayCent-SOM, our plant C-N interface framework describes N effects on plant physiology from photosynthesis, plant autotrophic respiration, and plant phenology plus a dynamic C/N ratio (Fig. 3). Following such model development philosophy, we more realistically represent the physiological processes of C-N cycling with unique features among current LSMs in C-N coupling”.

We may should use “**try to more realistically represent...**” to make our idea clearer. But realistically representing the process is every process-based model's objective, and every model will have its unique features. How can the reviewer make a conclusion based on these sentences that authors did not “**really read any other modelling description papers**”. When reading this paper, it is clear so many papers have been cited and reviewed in this paper.

We fully agree that some of our statements need to be revised/improved in our paper.

So we modified the text in line 293-295. However, this reviewer's statement is unacceptable and insulting. The first author is a graduate student and is in a learning curve. We welcome any critical comments/suggestion but not personal criticism.

(16)L323, eqn 4 and 5 – So all soil mineral N is available for plant growth. No influence of the amount of roots a PFT has or anything else? Is this realistic?

Reply:

The reviewer here distorted the meaning of the text, then provided the criticism. The text states: " N_{avail} is the amount of soil mineral nitrogen that was available at the end of the previous day", it is not "all soil mineral N is available for plant growth". "the amount that was available" does not mean "all soil mineral N is available". Additionally, the plant N demand is determined by PFT specific characteristics and potential growth of individual plant parts.

(17)L366-372, eqn 7 and 8 – Here the N limitation concept is introduced with eqn 8. $f(N)$ is the fundament of the N cycle limitation. First, you mention that V_{max} is related to leaf N concentration, but then you define $f(N)$ as the ratio between today's growth and N availability for growth. The current leaf N concentration is nowhere to be seen. By doing it this way, which I don't think makes any sense at all, you go against your own statement on line 82 where you state that your approach "prevent unrealistic instantaneous down-regulation of potential photosynthesis rates" but that is exactly what you end up doing when using only today's status as the determinant of N limitation ($f(N)$).

Reply:

First, we need to clarify that "Instantaneous down-regulation" is a specific terminology in N-limitation modeling and used in many literatures (Reich et al., 2006; Ghimire et al., 2016), which means that the photosynthesis rates (which is not the same as plant growth) would be limited as soon as the N (either in leaf or soil) is not sufficient. Four out of 5 models listed in Table 1 of DB2020, all but CLM4.5, take this approach.

Another approach (our model, CLM4.5 in which FUN is employed) takes into account that plants have resistance and self-adjustment by adjusting C:N ratios so that the N-limit effect in certain conditions does not linearly (does not instantaneously) respond to available N. For instance, the N-limit will produce effects only when available N passes some critical value (see our equation 8). This aspect is what we want to emphasize in our model development. Our model and CLM4.5 also have some differences in dealing with this issue. In the revised text lines 424-430, we explain this concept more clearly. But the reviewer's criticism is based on his/her understanding that the "instantaneous down-regulation" means "the current N limits current photosynthesis", which is actually done by every model, including our model. Therefore, the reviewer's criticism here makes no point. In this comment, the reviewer indicates "I don't think makes any sense at all". It is unclear what "make sense at all" means here refers to "instantaneous" or our approach?

References:

- Ghimire, B., Riley, W. J., Koven, C. D., Mu, M. and Randerson, J. T.: Representing leaf and root physiological traits in CLM improves global carbon and nitrogen cycling predictions, *J. Adv. Model. Earth Syst.*, 8(2), 598–613, doi:10.1002/2015MS000538, 2016.
- Reich, P. B., Hobbie, S. E., Lee, T., Ellsworth, D. S., West, J. B., Tilman, D., Knops, J. M. H., Naeem, S. and Trost, J.: Nitrogen limitation constrains sustainability of ecosystem response to CO₂, *Nature*, 440(7086), 922–925, doi:10.1038/nature04486, 2006.

(18)L410 – How is ylm affected by temperature? A reference to where this is described is needed or the equation.

Reply:

on line 466 we have added the reference (Cox 2001).

We also cited the text and equations in Cox (2001) here for an better explanation.

“Leaf mortality rates, γ_{lm} , for the tree-types are assumed to be a function of temperature, increasing from a minimum value of γ_0 , as the leaf temperature drops below a threshold value, T_{off} :

$$\gamma_{lm} = \begin{cases} \gamma_0 & T > T_{off} \\ \gamma_0(1 + 9(T_{off} - T)) & T \leq T_{off} \end{cases}$$

Where $T_{off} = 0^\circ \text{C}$ for broadleaf trees and $T_{off} = -30^\circ \text{C}$ for needleleaf trees (Woodward(1987)). The minimum leaf turnover rate $\gamma_0 = 0.25$.”

(19)Equation 12-14 – p that affects LAI (eqn 12) is adjusted by $f(N)$ (eqn 14). So, today's N limitation will dictate the full LAI. What happens if a single day has a very low $f(N)$? Is LAI also dropped to a very low value despite the N leaf concentration of already existing leaves could be very high? Does a lowering of LAI result in litter or does the LAI come back the next day if $f(N)$ is higher? This instantaneous down-regulation is also against what is written on line 82.

Reply:

This hypothesized issue will never happen in any N models. Can you image any N model produces a day-by-day fluctuation of N availability, which changes with rain or no rain, clear day or cloudy day, etc.? Moreover, in SSiB5/TRIFFID, nitrogen and carbon are cumulative. The TRIFFID passes vegetation information to SSiB5 every 10 days, which means we used the average of 10-day accumulations in coupling.

Furthermore, the $f(N)$ only affects the ΔGPP , i.e., the change of GPP due to the current photosynthesis process, not the total vegetation GPP. For LAI, the change should be even less because the $f(N)$'s effect is through the parameter of phenology. As such the reviewer's hypothesized scenario will never happen.

(20)Figure 5k – How can you have a higher GPP with SSiB5 compared to SSiB4 for the first years?

Reply:

Thanks for the very careful checking. There are two possible reasons: (1). In the offline runs for these stations, the initial conditions were obtained from our 2-D runs selected from the nearest grid point with the same year, month, and day as the station data. Because the 2-D run and offline station run have different meteorological forcing, the first-year off-line simulation may have some transient behavior although the 2-D run had reached the equilibrium condition before. From the results shown in figure 5 the 1st year and the following years results are generally consistent for all these stations but one the reviewer pointed out; (2) As indicated in the text, our N-limit approach does not directly affect GPP, but rather GPP is regulated through process interactions. Therefore, in the first year, the N-limit effect may not be in full play.

(21)Figure 8 and 9 – In figure 8f we see the impact of the N cycle on GPP. When looking at Northern America N limitation seems to be strongest in crop and high-latitude grass areas. For Eurasia, crops and grassland again seem to be affected the strongest by N limitation. Surprisingly the boreal forest, which we assume has a very strong N limitation, doesn't experience any effect on GPP when adding an N limitation. Why is this? Is the flexible C:N ratio too large that the N demand is always met? Instead, the tropical forests are N limited, which are assumed to not be N limited but P limited in reality. Why do you get these contrasting results compared to what one would like to get when introducing an N cycle to a model to represent N limitation on photosynthesis in areas we expect to be N limited (high latitudes and not around the equator)? Are you happy with these results?

Reply:

Please see our response for special comment #26.

(22)L475-477 – GPP is too high in SSiB4 so any reduction to GPP would result in an improved seasonal cycle simulation. Is there a way that you could increase the N limitation to bring GPP even closer to the observations or lower the GPP in any other way?

Reply:

Please see our response in [General comments #4](#). What we want to emphasize is that understanding processes is our goal not simply get a better result.

(23)L495 – LAI is more than twice the observations, so any decrease would reduce the bias. Is there any way to get the LAI closer to the observations? Now it is way off even with N limitation.

Reply: In our response in [General comments #4](#), we had shown that this is a very common shortcoming in current dynamic vegetation modeling. We try to overcome this shortcoming by introducing the N-limit. But it is not sufficient. More researches are needed.

(24)L546-547 – Can you make this statement from the results you have presented (NIPSN, NINPP, NIGPP)? And why do these experiments when you already in lines 382-384 state that they don't make sense?

Reply:

Please see our response in [General comments #2](#).

(25)L547-549 – Any limitation on GPP and LAI would improve the model. The important thing with introducing N limitation is to get it in the right locations. Otherwise, your model is missing something. Just reducing GPP and LAI as a global average can be done by just lowering your standard PFT Vmax value. Could probably get similar results just by optimising it.

Reply:

Please see our response in [General comments #4](#).

(26)L574-577 – Here you express that you capture the global N limitation pattern as described in Du et al. (2020), but I would disagree. Why don't you get any N limitation at all in the boreal forest regions? The "strong" latitudinal pattern of N limitation on GPP is due to grass N limitation and that the tropics are N limited for some reason. And what is meant by more comprehensive information? A better

analysis than exists in Du et al. (2020)?

Reply:

We disagree with the reviewer's statement that "Why don't you get any N limitation at all in the boreal forest regions"? In figure 13 a and b, we show the impact on GPP and in figure 13c and d, we show the impact on LAI. The LAI figure (13c,d) is close to a figure in DB2020 (attached below below), which really demonstrates the N-limit effect on NPP in boreal forests because the effect on LAI and effect on NPP should be similar. Figure 13c is similar to CLM (Figure 3a,b, DB2020). For GPP, our model also captures the nitrogen limited region reasonably in North American boreal regions, but not Siberia. We are still investigating this and have indicated this shortcoming in the revised version in line 520, 540, 542-543.

As to the "what is meant by more comprehensive information", in fact, the sentence next to the "comprehensive information" explains what this mean. Since current writing may cause confusion, we have revised the text as following:

"Our simulations show a strong latitudinal pattern of N limitation with a relatively close agreement with Du et al. (2020)'s results but also provide further information. For instance, N limitation on photosynthesis, which is shown in the difference between Exp. SSiB4 and Exp. NIPSN, results in a dominant decrease in tropical Africa and boreal regions (Figs. 13a, c), but N impacts on phenology and respiration dominate the decline in GPP in tropical forests (Figs. 13b, d). Moreover, there is a transition to N limitation at higher elevations in some lower-latitude regions (i.e., the Tibetan Plateau) (Du et al., 2020). This pattern is also captured in this study (Figs.13a, c)."

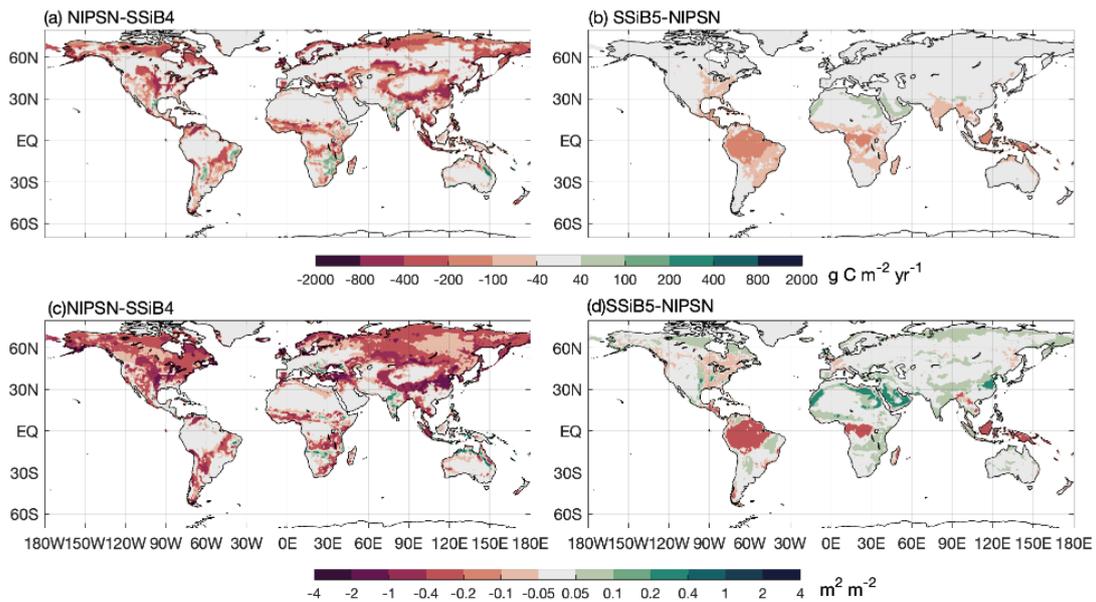


Figure 13. The 1982–2007 average gross primary production difference (a) NIPSN-SSiB4, (b) SSiB5-NIPSN, and leaf area index difference (c) NIPSN-SSiB4, (d) SSiB5-NIPSN

Note: NIPSN is N limitation on photosynthesis (V_{max}) only.

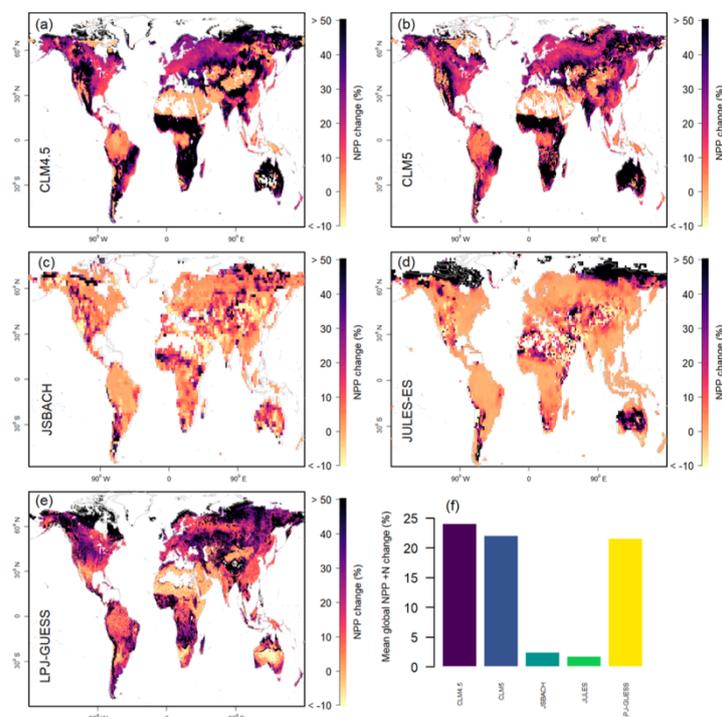


Figure 3. Model estimates of 1996–2005 mean net primary productivity (NPP) response to +N. (a–e) Model estimates, shown as the anomaly compared to the model control scenario. Values above 50% are given the 50% colour. (f) Globally integrated values. Global percent change in mean NPP.

Figure 3 are from DB2020

(27)L577-579 – Why are only grass areas affected by N limitations on photosynthesis (Figure 13a) and only tropical areas affected by Ra and phenology N limitations (Figure 13b)? This needs to be explained! Also, how does N resorption vary between regions? This could be an indication of the N limitation of a system as well as mentioned in line 592.

Reply:

For the first question, please see response to special comment #26. For the 2nd question, our model did not calculate the N resorption that was caused by plant self-adjustment during N limitation. Please see our response to Special comment #28.

(28)L592-593 – Why is eqn 7 and 8, where the instantaneous down-regulation factor ($f(N)$) is used to downregulate V_{max} , better than using well-established relationships between leaf nutrient concentration and V_{max} (Walker et al. 2014, Ellsworth et al. 2022)? How does your approach have a unique advantage compared to these?

Reply:

We have clarified the instantaneous down-regulation issue in our response in Special Comment 17. Our coupling strategy is based on SSiB/TRIFFID/DayCent-SOM framework. We do not employ the method in Walker et al. 2014 and Ellsworth et al. 2022, because our model has different structure from these models and their approaches require some information that the SSiB/TRIFFID/DayCent-SOM is unable to provide reliably.

Meanwhile, it is important to point out that the performance of the "well-established relationships between leaf nutrient concentration and V_{max} (Walker et al. 2014, Ellsworth et al. 2022) " also varies with different plant functional types over different continents. For instance, in Ellsworth et al. (2022), only tropical and sub-tropical trees are assessed, and no grass and shrub have been addressed.

The modeling of C/N coupling still has a long way to go. Experiments with different approaches are necessary.

(29)L601 – I haven't seen any improvements to boreal forests when it comes to GPP.

Reply:

Please see our response in [Special Comments #26](#).

(30)L602 – Where is it shown that SSiB5 has a more realistic C:N ratio dynamics compared to SSiB4 (assuming that it is SSiB4 that you are comparing to here)?

Haven't seen any figure or result of this. I assume that SSiB4 has a fixed C:N ratio.

Reply:

The SSiB5 and SSiB4 are different in the way plant C/N ratios are determined.

(1). SSiB5 used the dynamic C/N ratio as discussed in section 4.2 and the SSiB4 uses the fixed C:N ratios.

(2). The ranges to calculate the C:N ratio are listed in Table 3. The information in Table 3 and methodology to calculate the C:N ratio were validated in Parton et al. (1993) and Parton et al. (2007) using observational data and DayCent. Therefore, we believe this approach is more reliable.

(3). The SSiB5/TRIFFID/DayCent-SOM performance in 2-D simulation and site validation in this study demonstrate this approach is able to produce reasonable results. In the revised paper, on line 386-388, we clearly indicate the different treatments between SSiB5 and SSiB4 in C/N ratio dynamics.

(31)L606-607 – Don't agree that SSiB5 can predict a global pattern of N limitation on GPP, when the strongest limitation is in the tropical region and boreal forest has none.

Reply:

We have modified the sentence and deleted "global pattern". In the revised text, we more clearly indicate where the improvements are made in line 634-638.

(32)L615 – N limitation on Ra and phenology is effective in the tropics.

Reply:

We agree with the reviewer's comments and have revised the sentence in line 678-679.

(33)L? – There is no mention of BNF or N deposition in the manuscript. I'm assuming that BNF is done in DayCent and also N deposition is handled there. This needs to be mentioned as also which dataset is used as N deposition input.

Reply:

Since there is no N deposition observation data, we used the global average ($10 \text{ kg ha}^{-1} \text{ yr}^{-1}$, Drewniak et.al,2017) as default value for N deposition.