Response to reviewers

We appreciate the time and effort that the editors have dedicated to providing your valuable feedback on our manuscript. The reviews are copied verbatim and are italicized. Author responses are in regular font. Changes made to the manuscript are blue.

Comments from reviewer 1

Comment

Lines 14-15: This sentence needs rewording. I suggest "... nutrient limitation resulted in a reduction of GPP from the Carbon-only value of 143 PgC/yr to 133 PgC/yr in the CN version and 129 PgC/yr in the CNP version."

Response

Thank you for your comment. As suggested line 14-15 was changed to:

For the years 2001-2015 the nutrient limitation resulted in a reduction of GPP from the Carbon-only value of 143 PgC yr^{-1} to 130 PgC yr^{-1} in the CN version and 127 PgC yr^{-1} in the CNP version.

Comment

Line 123: This should read "if leaf C:N ratio is higher than CNleafmax..." Nitrogen limitation occurs when nitrogen concentration is lower, not higher, because C:N ratio divides by nitrogen.

Response

Thank you for your comment. Line 123 has been changed to:

If leaf C:N ratio is higher than $CN_{leafmax}$ (the maximum CN ratio parameter) terrestrial vegetation biomass is reduced.

Comment

Figure 1: The litter box should be labeled "Litter N", not "Litter P."

Response

Thank you for your comment. Figure 1 litter box has been relabeled to Litter N:

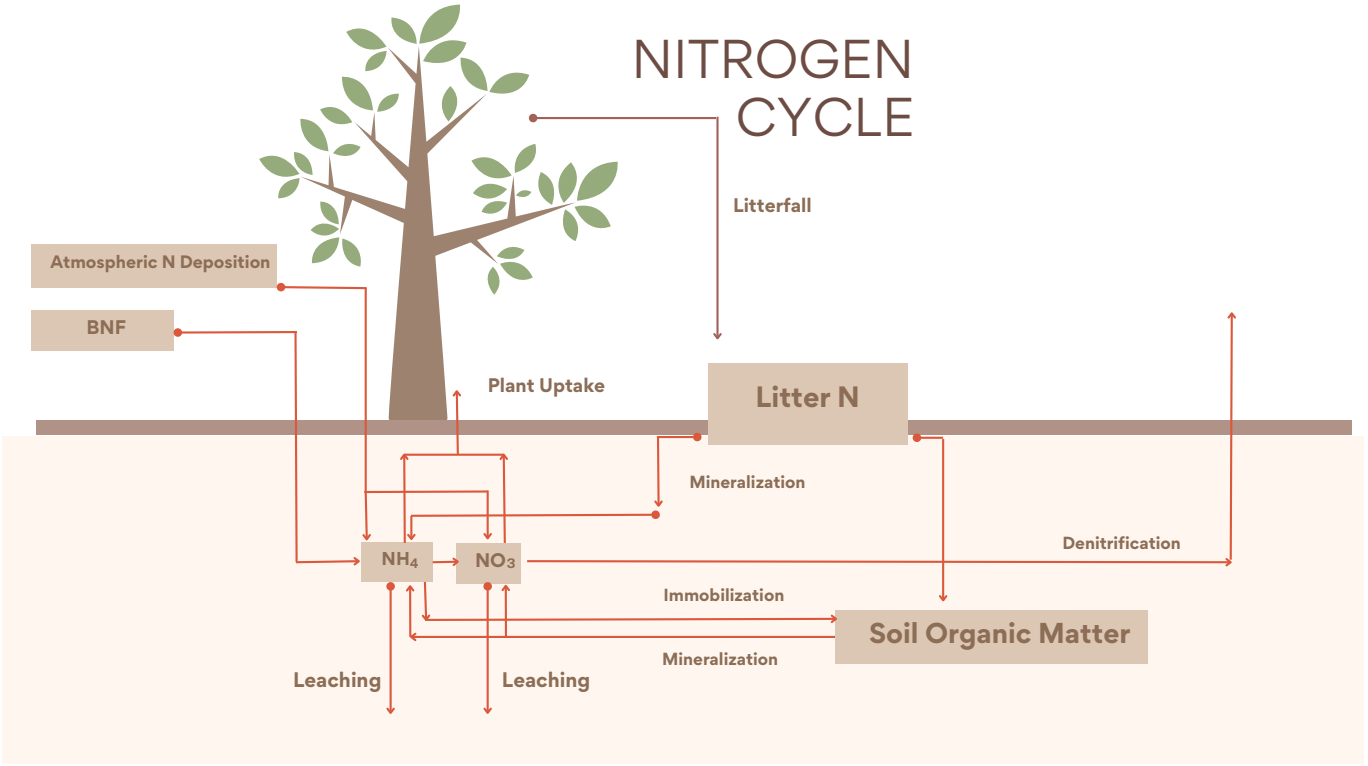

Figure 1. Diagram representing the UVic ESCM nitrogen cycle.

Comment

Line 226: P limitation of leaf biomass should occur when C:P ratio is higher than the maximum (implying that there is not enough P or too much C), not lower than the maximum.

Response

Thank you for your comment. You are correct. Line 226 has been changed to:

The limiting effect of CP_{leaf} is when its value is higher than the maximum CP_{leaf} ratio parameter $CP_{leafmax}$.

Comment

Equation 20: Is λUPtase in the numerator supposed to be λPtase? I did not see any explanation of λUPtase in the description. Also, is this equation a constant value? All the terms appear to be constants rather than variables but it wasn't clear from the text if Ptase is expected to vary or if it is a constant in the model.

Response

Thank you for your comment. That is a typo, λ_{UPtase} is λ_{Ptase}. In equation 20 λ_{UPtase} has been changed to:

λ_{Ptase}

Futhermore, the following line has been added:

P_{tase} is a constant value.

Comment

Line 259: This is incorrect. The model assumes nutrient limitation when the C:N ratio or C:P ratio (not N or P concentration) is higher than the maximum ratio. A concentration can't be compared to a ratio.

Response

Thank you for your comment. Line 259 has been changed to:

The model assumes nutrient limitation when the estimated CN and CP leaf ratio is higher than the maximum CN ($CN_{leafmax}$) and CP ($CP_{leafmax}$) ratio in leafs.

Comment

Line 306: Figure 3 in the revised manuscript shows a value of about 130 for the baseline simulation, which is different from the text and also very different from the figure in the previous version of the manuscript which did have a value of about 143 as the text states. Was there a mistake in this figure? Or did the value change in the revision? Either the figure or the text needs to be corrected so they are consistent with each other.

Response

Thank you for noticing. The incorrect file was uploaded in the revised version. The file has been changed to the original, and we will be extra careful to upload the correct file during the submission process:

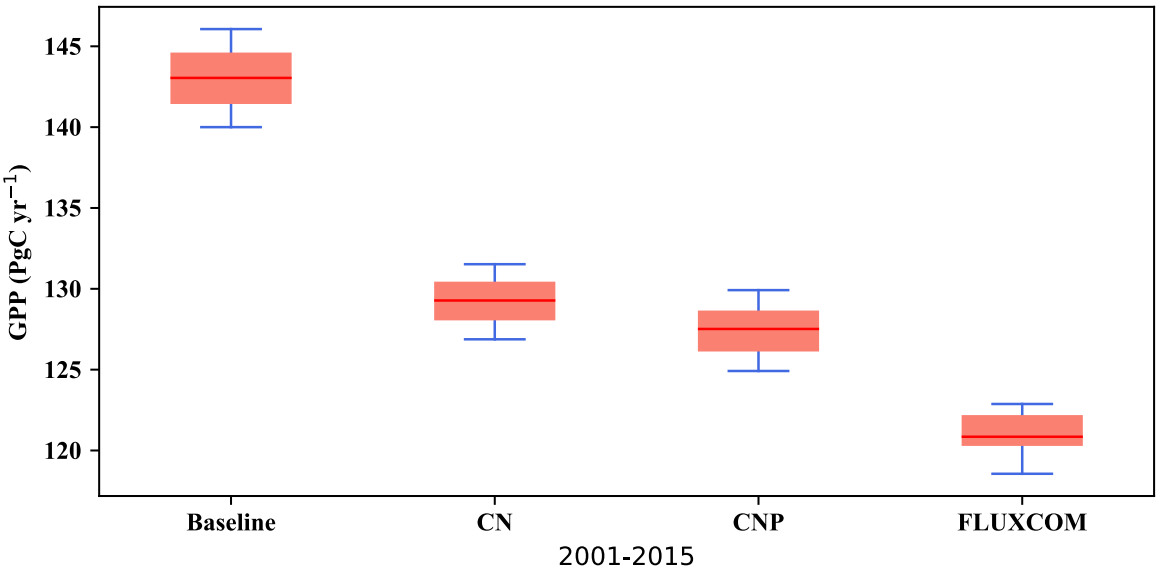

Figure 2. Modelled yearly Gross Primary Productivity (GPP) from 2001 to 2015 versus FLUXCOM GPP dataset (Jung et al. , 2019).

Comment

Line 311: Are these differences in correlations statistically significant? The differences seem very small (0.7, 0.73, 0.74).

Response

Thank you for your comment. However the UVic ESCM does not have internal variability. Any simulation with the same initial conditions, forcings, and compiler, will have identical outcomes to machine precision. This means than any change in the comparison to data is due to changes in the model structure not due to internal stochastic processes. That is, 'statistical significance' is mathematically undefined for our model structure. The UVic ESCM is purely deterministic model.

Comment

Figure 4: It's Pearson's r, not Person's r. Also, the panels in this figure should be labeled with letters (a,b,c...) that can be referred to in the caption.

Response

Thank you for your comment. Figure 4. Person's r has been changed to Pearson's r. New labels have been added to Figure 4 as:

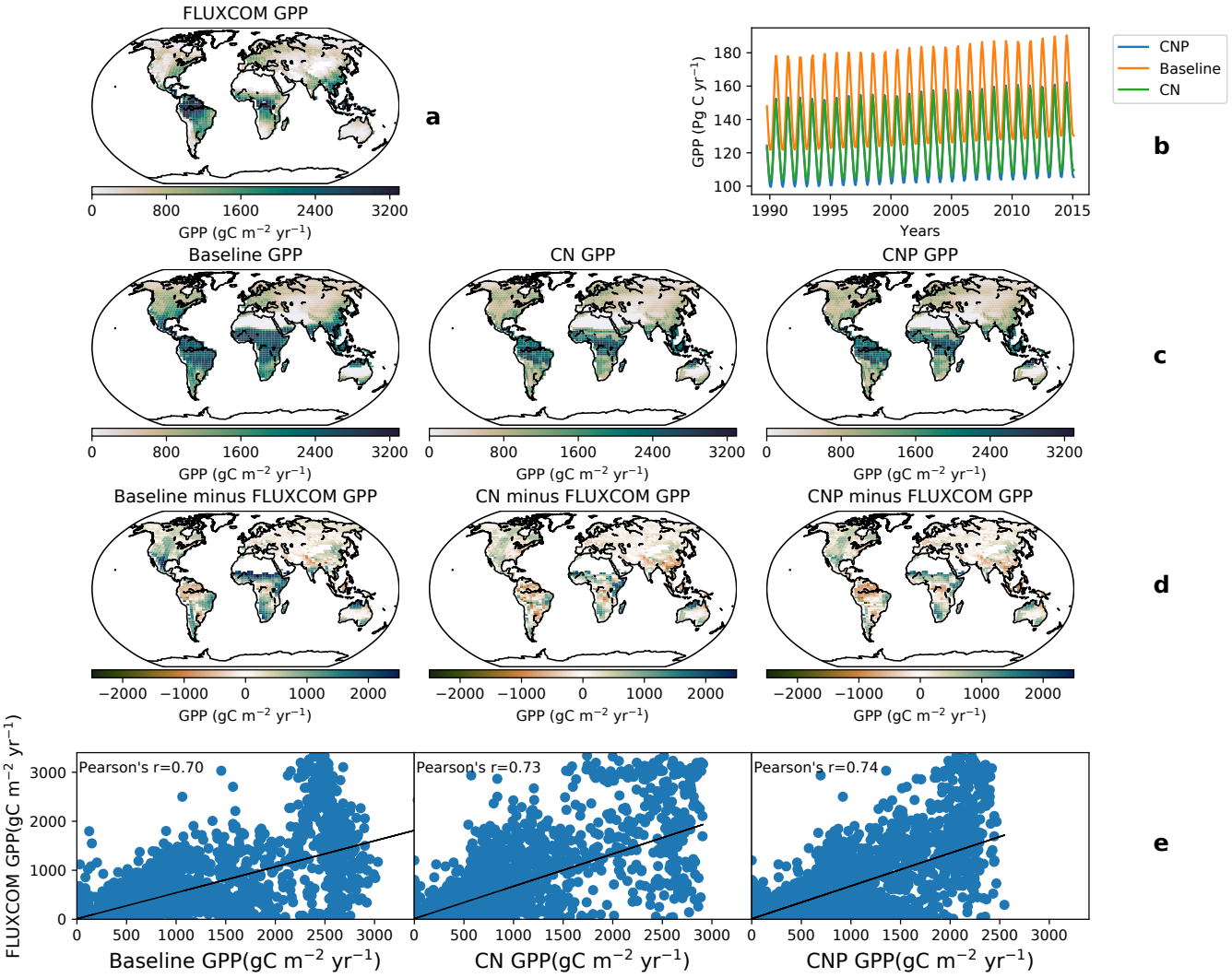

Figure 3. a. FLUXCOM GPP dataset from 2000-2010, b. Seasonal GPP from 1990-2015 for Baseline, CN and CNP. c. Second line shows the global GPP from 2000-2010 for Baseline, CN and CNP. d. The third line shows the difference between Baseline, CN and CNP and FLUXCOM GPP dataset. e. Shows the correlation of Baseline, CN and CNP to FLUXCOM GPP dataset.

Comment

Line 355: It's not clear what "this shift" is referring to. The previous sentence referred to a shift in broadleaf trees, not needleleaf trees.

Response

Thank you for your comment. Line 355 has been changed to:

Needleleaf trees were reduced in North America and Europe.

Comment

Line 360-365: The response to reviewers argues that it is not necessary to show a figure of PFT biomass because PFT fractions are shown, but I still think it would be helpful to show a map of biomass so this detailed description of spatial patterns could be matched with a visualization.

Comment

Thank you for your comment. CNP biomass has been added to figure 6:

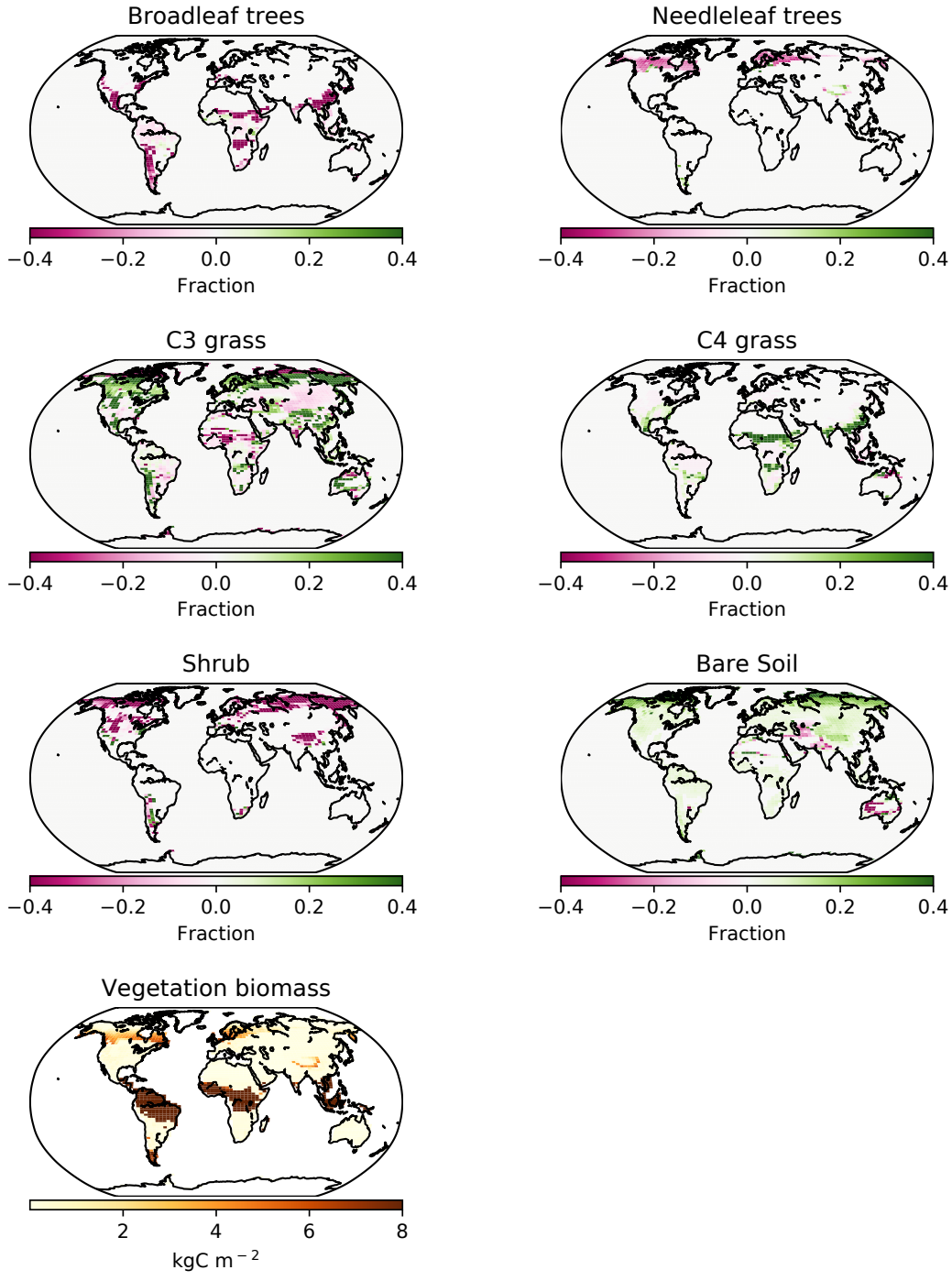

Figure 4. PFTs fractions in the UVic ESCM for 1980-2010, CNP minus baseline. Bottom last plot shows CNP global biomass distribution.

Comment

Line 369: I would add a reference to Figure 9, which shows these patterns. Also, units of nitrogen stock should be kg N, not kg C.

Response

Thank you for your comment. A reference to Figure 9 has been added. kg C has been changed to kg N.

Comment

Line 374: This should also be units of N, not C

Response

Thank you for your comment. C has been changed to N.

Comment

Line 387-390: Reference Figure 9, which shows these patterns.

Response

Thank you for your comment. A reference to Figure 9 has been added.

Comment

Line 397-408: This paragraph should reference Figure 10, which shows the N2O pattern. I also found it odd that the paragraph refers to several observational datasets that are not shown in the figure but does not describe the EDGAR dataset that is shown in the figure. It would help readability if the text and the figure were more consistent with each other.

Comment

Thank you for your comment. A reference to Figure 10 has been added. Furthermore, the following line has been added to the paragraph:

Figure 10 shows CN and CNP N_2O fluxes from 1990 to 2018. Compared to EDGAR version 6 dataset Crippa et al. (2021) our model simulates N_2O fluxes relatively well, agreeing mostly in last 10 years of the values. However, we observed an overestimation from 1990 to 2010.

Comment

Line 424-425: The response to reviews says that this sentence was changed to say that the underestimate was due to lack of P fertilization, but the text still says that the underestimate is due to high mineralization rate.

Response

Thank you for your comment. You are correct. The line has been changed to:

This underestimation is likely the result of the lack of P fertilization on land.

Comments from reviewer 3

Comment

I'd like to thank the authors for addressing the comments and suggestions provided in the first review. Overall, your response seems reasonable and appropriate to an extent. It is good to see that you have acknowledged the reviewer's suggestion while explaining the main focus and purpose of your paper. However, we recommend that you explicitly state the same in the introduction as well.

Response

Thank you for your comment. The following text has been added to the introduction:

We aim to describe a terrestrial nitrogen and phosphorus cycle adapted, developed and implemented for the UVic ESCM version 2.10. The main dynamics captured in this study are in the terrestrial system, especially vegetation. Furthermore, we intent to improve the current state of the previous N cycle implement in the UVic ESCM, develop a new P cycle and couple carbon nitrogen and phosphorus, in order to improve the carbon cycle feedbacks projections.

Comment

They also provide justification for not including energy and water assessments as they were already evaluated in a recent study and adding them would not add much value to the present paper. However, it is not accurate to say "The addition of terrestrial N and P cycles had only a minor effect of these variables", as broadly discussed in Arora et al., 2020) and (Braghiere et al., 2022). I suggest briefly expanding this discussion in introduction too.

Response

Thank you for your comment. The following text has been added to the last paragraph of the introduction

The addition of nutrient limitation has been observed to mainly effect the capacity of vegetation to uptake carbon (Wang et al. , 2010; Goll et al. , 2017; Wang et al. , 2020). Therefore, the accumulation of carbon in the atmosphere is enhanced, leading to increases of temperature in simulations. This temperature changes are likely to have some impact to variables sensitive to atmospheric temperature changes. Furthermore, the decrease of vegetation biomass affects variables affected by the distribution and composition of plant functional types, as changes in terrestrial albedo.

Comment

The authors also mention the challenge of validating nutrient cycles in Earth system models due to the lack of observations. However, it is important to note that the author could have provided more specific details on how they validated nutrient cycles with other modeling studies and available observations to strengthen their argument. For example, in Fig 9 the authors show the IGBP global soil nitrogen right next to their map, but without any statistical evaluation. I suggest adding in the manuscript a paragraph calling for an ILAMB like tool (and reference (Collier et al., 2018)) specifically for nutrient cycles, and share about your own experience of how difficult it was to find validations tools for nutrients.

Response

Thank you for your comment. The following lines has been added to section 2.5:

One of the challenges of modelling nutrients in terrestrial systems is the lack of observations and validation datasets. Furthermore, the existing range of values for N and P variables are highly uncertain. This large range in values makes it difficult to accurately tune models. Although, improvements are in sight, with new artificial intelligence derived global datasets beginning to become available (He et al. 2021). Model validation has been advancing quickly in the last decade (Spafford and MacDougall , 2021) with tools such as the International Land Model Benchmarking (Collier et al. , 2018) that significantly improves terrestrial model validation. However, there are limited variables available to compare to nutrient model development. The increase of the addition of nutrient structures in ESMs (Arora et al. , 2020) suggest the need of terrestrial nutrient validation tools to improve model accuracy in the developmental phase.

Moreover, a terrestrial nutrient model intercomparison project would unify global efforts to improve the representation of nitrogen and phosphorus in ESMs.

Comment

We also appreciate your attention to detail and agree that Figure 3 should say litter N.

Response

Thank you for noticing. Litter P has been changed to Litter N.

Comment

Furthermore, while your biodiversity comment adds a little, we suggest that you continue to work on properly linking biodiversity with other aspects of the biogeochemical cycles in the Earth system and climate change. We recommend conducting a more thorough literature review in the introduction. Additionally, citing (Wieder et al., 2015) and (Zaehle et al., 2015) would help provide more general scientific discussions at the beginning.

Response

Thank you for your comment. The following lines has been added in the introduction:

The fluxes and availability of N and P in soils depends on the interactions between soil mineral matrix, plants and microbes (Cotrufo et al. , 2013). For example, N input from atmospheric N_2 fixation is mediated by a specialized group of microorganisms. Furthermore, the recycling on N from plants-soil-microbes determines the availability of N for plant uptake. Overall, the land biota dynamics impacts the productivity, ecosystem resiliense and stability (Yang et al. , 2018). High diversity has been linked to enhanced vegetation productivity (Wagg et al. , 2014). The diversity in terrestrial ecosystem is determined by biological, environmental and physico-chemical processes. Anthropogenic activities and land use change emission can influence soil diversity, impacting the availability and cycling of N and P (Chen et al. , 2019). For

example, N and P fertilization, have been shown to affect soil microbial biomass and composition (Ryan et al. , 2009).

Comment

We also recommend defining acronyms in their first appearance and keeping their use consistent throughout the manuscript. We noticed that the manuscript is still inconsistent with some places using nitrogen/phosphorus, while others use N/P.

Response

Thank you for your comment. We have checked that acronyms are defined at their first appearance. However, if there first appearance happened to be located in the abstract, we deem necessary a second redefinition in the main text to account for readers who skip the abstract. We also checked for inconsistent use of nitrogen/phosphorus instead of N and P.

Comment

Moreover, in Figure 9, we suggest that you add statistical metrics in comparison to the IGBP product and other models. At the very least, describe it in the text, as shown in Braghiere et al. (2022) Fig S13..

Response

Thank you for your comment. We do no think there is a need for a statistical test for the IGBP product. We are not testing the statistical significance difference between our model outputs and the dataset in this case. The reason being that they are already clearly different in the maps, our model is below the concentration of the IGBP while our global distribution is closer to the IGBP. Hence, we only used range comparison. Regaring Fig S13. Average global distribution of leaf (a) N:P ratio, (b) C:N ratio, and (c) C:P ratio from 1994 to 2005 in ELM-FUN3.0. we do not understand how is a statistical description shown in here.

Comment

On line 79, we suggest that you name and reference a few important ESMs that include N (CLM, JULES, etc.) and those that include P (ELM, CABLE). In Braghiere et al. (2022), the authors have this information right in the introduction, which you can use as a reference.

Comment

Thank you for your comment. We have added the reference for the suggested models in line 76.

Comment

Line 793. I suggest adding "The model does not account for uptake constrains on terrestrial vegetation, such as the Fixation and Uptake of Nutrients (FUN) model (Allen et al., 2020; Brzostek et al., 2015; Fisher et al., 2010). This includes spatial representations of mycorrhizal associations and the carbon cost of nitrogen and phosphorus uptake from soil Shi et al. , 2016; Braghiere et al. , 2021, 2022). Furtermore, we do not estimate nitrogen cost for phosphorus metabolization or viseversa.

Response

Thank you for your comment. The following has been added to the discussion:

The model does not account for root uptake constrains of N and P on terrestrial vegetation. This includes spatial representations of mycorrhizal associations and the carbon cost of nitrogen and phosphorus uptake from soil (Shi et al. , 2016; Braghiere et al. , 2021, 2022).

Comment

Finally, what are the next steps for the improvement of this model. Write a paragraph talking about future research.

Response

245 Thank you for your comment. We do mention a list of plan improvements to the model. We clarify our intend in line 480, it now states:

In general, these are the following model limitation that are plan to be improved in future model development projects:

References

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