

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.

5 Changes made to the manuscript are blue.

Comments from reviewer 1

General Comment

10 *This manuscript describes new nitrogen (N) and phosphorus (P) cycling processes that have been added to a new version of the UVic ESCM intermediate complexity Earth system model. The new processes are well justified, clearly described and well supported by citations to relevant literature, data syntheses, and theory. The new model functions are evaluated using global datasets of carbon, nitrogen, and phosphorus pools and/or fluxes and the level of agreement and areas for improvement in the model are described clearly and fairly. Overall, this is a well-written model description paper.*

Response

15 Thank you for the positive feedback.

General Comment

20 *There are some areas where the clarity of the manuscript could be improved, primarily related to the equations and figures. While the Methods section does include several relevant equations for N and P cycling, it omits some important processes and overall does not provide a complete picture of N and P cycling in the model. Importantly, equations and explanations are not provided for the variable tissue C:N and C:P ratios that are an important part of the stoichiometric limitation component of the model. I would advise including those equations in the main text since they are a key part of the model functionality and*

results. I would also suggest adding an appendix with the complete set of equations related to N and P cycling so readers do not need to search through other previous papers to gain a complete picture of how nutrient cycling in the model works.

Response

Thank for your comment. The equations governing C:N and C:P ratios were added in the manuscript in section 2.2, as well as a more in depth review on the limitation processes in section 2.4.

Section 2.2:

$$CN_{leaf} = \frac{C_{leaf}}{N_{leaf}}, \quad (1)$$

where C_{leaf} is the carbon content in leaves and N_{leaf} is the nitrogen content in leaves. CN_{leaf} is one of the most important nutrient limitators in the model. It controls the maximum carboxylation rate of RuBISCO. Furthermore, it control vegetation biomass. If nitrogen concentration in leaves is higher than $CN_{leafmax}$ (the maximum CN ratio parameter) terrestrial vegetation biomass is reduced.

Section 2.4:

The model assumes nutrient limitation when the concentration of nitrogen and phosphorus is higher than the maximum CN ($CN_{leafmax}$) and CP ($CP_{leafmax}$) in leaves. For grids with nutrient limitation the carbon in leaves is reduced to match the maximum CN or CP ratios in leaves. The carbon that is reduced is transferred to the litter pool. This reduction can happen for one or both nutrients until the ratio is met.

The following equations regulate the reduction of biomass based on nutrient limitation:

$$C_{leaflimitedn} = N_{leaf}CN_{leafmax}, \quad (2)$$

$$C_{leafdiffn} = C_{leaf} - C_{leaflimitedn}, \quad (3)$$

$$C_{leaflimitedp} = N_{leaf}CP_{leafmax}, \quad (4)$$

$$C_{leafdiffp} = C_{leaf} - C_{leaflimitedp}, \quad (5)$$

where $C_{leaflimitedn}$ and $C_{leaflimitedp}$ are the carbon concentration in leaves if the system is considered to be limited. $C_{leafdiffn}$ and $C_{leafdiffp}$ are the the carbon lost due to nutrient limitation and their value are
50 sum in the litterfall equation when the system is in nutrient limitation.

General Comment

*The figures are generally informative, but there were some parts of the text that described model-data comparisons and other patterns that were not directly shown in the figures. In addition, I think it would improve the readability of the paper if the figures showing N results matched the figures showing P results
55 in their content. Currently, the N figures and P figures show different comparisons in some cases which makes it less straightforward for readers to evaluate those parts of the model. For example, Figure 9 shows maps of modeled total soil P compared with measured total soil P and the difference between them. In contrast, Figure 7 shows maps of modeled N but does not show any direct comparison with measured patterns of N, even though model-data comparisons of global N patterns are discussed in the text.*

60 Response

Thank you for your comment. The figures show different comparisons in order to be compatible for comparison with other studies. It is correct that Figure 7. does not show any direct comparison. We have now added a nitrogen soil distribution map from The Global Soil Data Task.

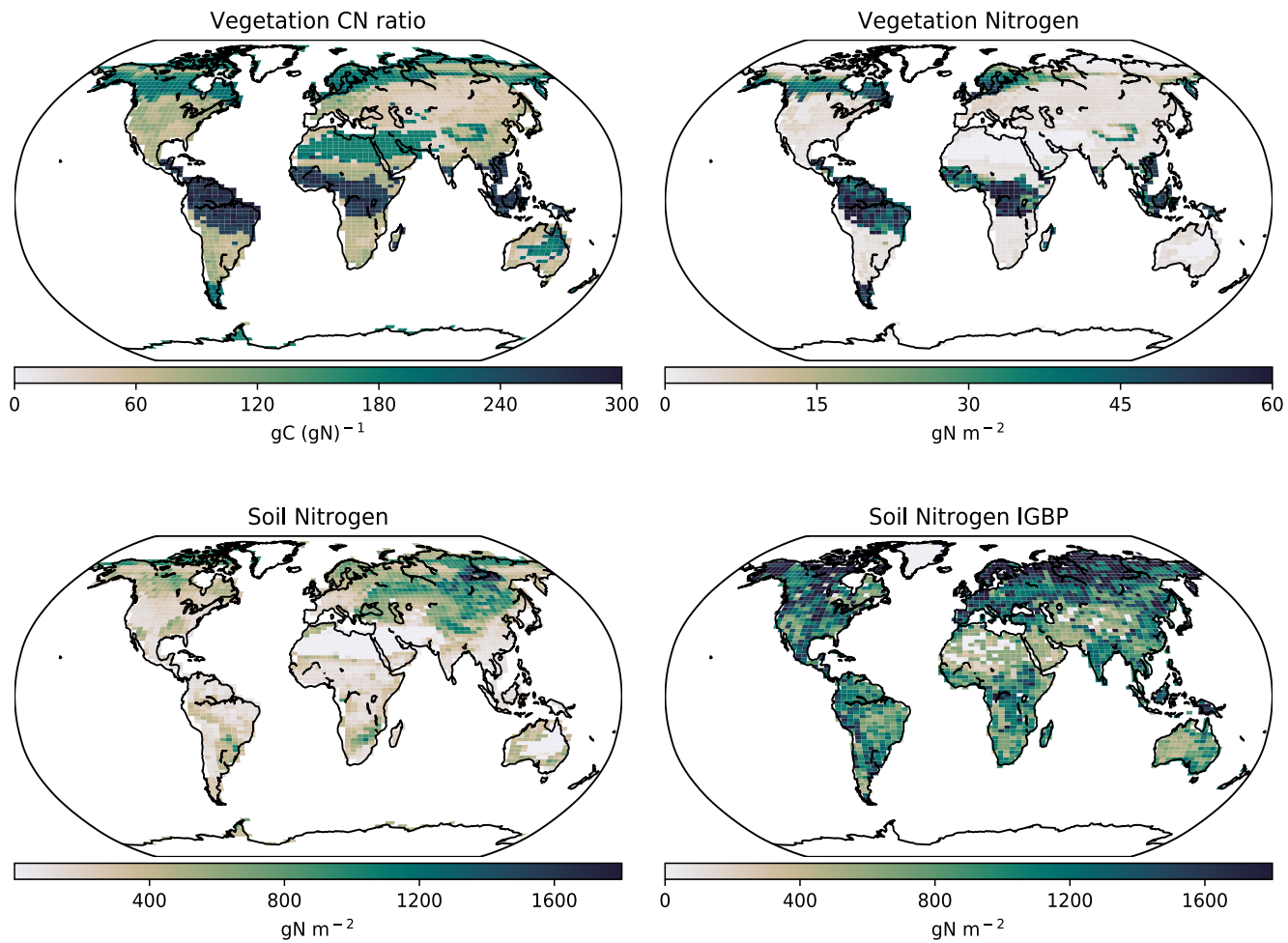


Figure 1. Modelled global soil and vegetation nitrogen in the CNP version of the UVic ESCM from 1980-1999. Lower right map corresponds to the soil nitrogen from the IGBP-DIS dataset (Global Soil Data Task Group , 2000)

Specific comments

65 **Comment 1**

Line 14: It would be helpful to include the GPP for carbon-only simulations in this comparison as well.

Response 1

The text has been changed to:

70 For the years 2010-2020 the nutrient limitation resulted in a reduction of GPP from the Carbon-Nitrogen model version of 133 Pg yr⁻¹ and the Carbon-Nitrogen-Phosphorus model version of 129 Pg yr⁻¹ simulations compared to Carbon only value of 143 Pg yr⁻¹. This implies that the model efficiently represents a nutrient limitation over the CO₂ fertilization effect.

Comment 2

75 *Line 59-61: This list was hard to follow and could use some editing.*

Response 2

Thank you for your comment. The sentence was re-written to:

80 by the geochemical interactions in terrestrial soils, Vitousek et al. (2010) defined six mechanisms by which P is driven to limitation: loss by leaching, soil barriers that physically prevents access to roots, slow release of mineral P forms, P parent material, sequestration of P in soils and pools in the ecosystem and finally, anthropogenic input of nutrients.

Comment 3

85 *Line 69-75: The introduction discusses Earth system models in general, and then the history of UVic ESCM in particular). I think it would be helpful to include a few sentences about intermediate complexity*

ESMs as a class to provide some more context about the goals of the type of model that UVic ESCM represents and how it compares to other similar models.

Response 3

Thank you for the comment. We have now included a few sentences about intermediate complexity ESMs.

90

Intermediate complexity Earth system models, have a lower spatial representation, and model structures that have been intentionally simplified in one or more ways. This simplification allows for long-term simulations that are typically not feasible in higher complexity models. This class of model is not suitable for studying processes at small spatial scales. Hence, they are used in research questions that require
95 large spatial and temporal scales (Weber , 2010). The current generation Earth system models are, or have already, developed nutrient limitation for their model structure. While CN models are more common CNP models remain rarer. However, P cycles have been suggested to be included into Earth system model for its importance in tropical regions (Wang et al. , 2010; Goll et al. , 2012). The first attempt to include nutrient limitation in the University of Victoria Earth system and climate model (UVic ESCM) was done
100 by Wania et al. (2012) but was not included in the current publically available version of the model due to the need of further improvement. Hence, here we intent to improve the current state of the previous N cycle, develop a new P cycle and couple CNP in the UVic ESCM, in order to improve the carbon feedbacks projections.

Comment 4

105 *Line 105: Variable C:N ratios for leaf and root pools are mentioned but the details (and equations) of what determines the actual C:N ratio are not provided. It would also help to explain here how the variable C:N ratios affect other parts of the model (e.g., photosynthesis or root function dependence on tissue N). It would help to provide some summary of how the relevant processes from the Gerber et al citation are calculated, ideally with equations provided in an appendix.*

110 **Response 4**

Thank you for your comment. The following equation and text were added to section 2.2.

$$CN_{leaf} = \frac{C_{leaf}}{N_{leaf}}, \quad (6)$$

where C_{leaf} is the carbon content in leaves and N_{leaf} is the nitrogen content in leaves. CN_{leaf} is one of the most important nutrient limitators in the model, as it controls the maximum carboxylation rate of
115 RuBISCO. Furthermore, it control vegetation biomass. If nitrogen concentration in leaves is higher than $CN_{leafmax}$ (the maximum CN ratio parameter) terrestrial vegetation biomass is reduced. A more detailed description of nutrient limitation can be found in section 2.4.

Comment 5

*Equations 1-2: An explanation should be provided here for what “av” means in the mineral nitrogen
120 pools. Later in the paper I found that this means “available” but that should be explained here, along with an explanation of how the available fraction is calculated. Is there an unavailable fraction?*

Response 5

Thank you for your comment. An explanation has been added to the manuscript:

125 where NH_4^{UP} and NO_3^{UP} represent the nitrogen uptake, the left term is the active uptake while the right term is the passive uptake (see table 1), the latter is the transport of N via the transpiration water stream. V_{maxn} is the maximum uptake rate for nitrogen, C_{root} is the root carbon biomass, $[NH_4(av)]$, $[NO_3(av)]$ and $[Nmin(av)]$ are the NH_4 , NO_3 and mineral nitrogen concentrations, $K_{n,1/2}$ is the half saturation constant for nitrogen and Qt is the transpiration rate. av represents the available portion of
130 NH_4 and NO_3 in soil. This fraction is calculated as the total concentration of NH_4 and NO_3 divided by sorption factors (10 and 1 respectively) following Wania et al. (2012).

Comment 6

Line 122-123: I found “depth of soil layer” and “root depth” confusing. Is the soil layer referring to each individual model layer, or to the depth of the entire soil? Is the root depth a rooting depth parameter for each PFT, or the depth at which the root fraction is being calculated?

Response 6

Thank you for your comment. Depth of soil layer is the depth of each of the eight active soil layers. The UVic ESCM version 2.10 contains 14 subsurface layers with thickness exponentially increasing with depth with a surface layer of 0.1 m, a bottom layer of 104.4m and a total layer of 250 m. Only eight are active in the water and biogeochemical cycles, the remaining layers are a granite-like heat sink.

For this equation, depth of soil layer represents the depth of each specific soil layer. Root depth is a PFT based parameter that represents the depth of the roots. In the UVic ESCM, this was first developed in a doctoral thesis written by Avis (2012).

where Z_{top} and Z_{bot} represents the top layer and bottom layer depth respectively, while D and dr are the depth of the soil layer and the root depth. The depth of soil layer represents the depth of each specific soil layer. Root depth is a PFT based parameter that represents the depth of the roots. Given the multiple soil layer set up, the root fraction modifies the value of root carbon, creating a more realistic representation of the uptake root depth reach for each PFT given the multiple soil layer set up.

Comment 7

Line 129: Provide an explanation or reference for the statement that “It takes 1 mol of NO_3 to mineralize 1 mol of organic C.”

Response 7

Thank you for your comment. We have realised that the statement only applies for very specific respiration pathways. Hence, we have deleted it from the text.

Comment 8

Line 132-133: The temperature and moisture functions are not provided or explained. Is a moisture function necessary when the anaerobic respiration is calculated only for the saturated fraction of the layer?

160 Response 8

You are correct the temperature and moisture function are not defined here. They were originally designed in TRIFFID by Cox (2001). The following equations and explanation has been added:

The temperature and moisture soil functions are taken directly from Cox (2001), and are represented
165 by the following equations:

$$f_t = q_{10}^{0.1(t_s - 25)}, \quad (7)$$

$$f_m = \begin{cases} 1 - 0.8(S - S_0) & \text{for } S > S_0, \\ 0.2 + 0.5\left(\frac{S - S_W}{S_0 - S_W}\right) & \text{for } S_W < S \leq S_0, \\ 0.2 & \text{for } S \leq S_W, \end{cases} \quad (8)$$

where in f_t , $q_{10} = 2$ and t_s is the soil temperature in °C. In f_m , S is the soil moisture, S_W is the wilting point of soil moisture, S_0 is the optimum soil moisture.

170 Comment 9

Table 1: Descriptions should include “pool” or “rate” or similar for each line since the table contains a mix of different types. Also, temperature and moisture functions are functions rather than numbers or outputs and feel out of place in this table.

Response 9

175 The table has been modified to:

Table 1. Updated nitrogen cycle module pools, rates and variables.

Variables	Units	Type	Descriptions
NH_4^{UP}	$\text{kg N m}^{-2} \text{ yr}^{-1}$	Rate	NH_4 vegetation uptake
NO_3^{UP}	$\text{kg N m}^{-2} \text{ yr}^{-1}$	Rate	NO_3 vegetation uptake
Croot	Kg C m^{-2}	Pool	Root carbon
$[NH_4(av)]$	kg N m^{-3}	Pool	Available NH_4 concentration
$[NO_3(av)]$	kg N m^{-3}	Pool	Available NO_3 concentration
Froot	-	Variable	Root fraction
$[Nmin(av)]$	kg N m^{-3}	Pool	Available mineral N concentration
R_{an}	$\text{kg C m}^{-3} \text{ s}^{-1}$	Rate	Anaerobic respiration rate
C_s	kg C m^{-3}	Pool	Density of soil carbon in each layer
A_f	-	Variable	Anaerobic saturation fraction
N_2O	$\text{kg N m}^{-2} \text{ yr}^{-1}$	Rate	Nitrous oxide flux
NO	$\text{kg N m}^{-2} \text{ yr}^{-1}$	Rate	Nitric oxide flux

Comment 10

Table 2: Is DSL the same as D in Equation 3? Make sure the notation is consistent.

Response 10

180 D_{SL} from table 2 has been changed to D .

Comment 11

Equation 8: Should the Pimm term be negative in this equation? Immobilized P would be subtracted from the inorganic P pool. Also, shouldn't equation 9 be included as a negative term in Equation 8 since

sorption reduces the inorganic P pool? These equations don't seem to be mass balanced with respect to
185 each other.

Response 11

Thank you for noticing. Yes, it is a typo. P_{imm} should be negative. Regarding equation 9, we understand why it may seem that way. The equation follows Goll et al. (2017), that at the same time follows a previous study by the same author (Goll et al. , 2012). In Goll et al. (2012) things get more clearer, the
190 sum of P_{sorb} and P_{soil} represents the inorganic P pool. Both pools are assumed to be in equilibrium in daily basis. The only loss comes from the strongly sorb pool represented in the equation by T_{sorb} . Hence, the inorganic pool is first calculated as the fraction sorbed and the fraction that was not sorbed (given a map). The sum of the two lose phosphorus in a rate defined by the multiplication of T_{sorb} by P_{sorb} . Then both get recalculated based on the fraction given by the map.

$$195 \frac{dP_{soil}}{dt} = (1 - K_s)(P_{wea} + P_{litmin} + P_{orgmin} - P_{leach} - P_{up} - \tau_{sorb}P_{sorb} - P_{imm}), \quad (9)$$

The estimation of P_{soil} based on Goll et al. (2017), is originally taken from Goll et al. (2012). Here, the sum P_{sorb} and P_{soil} constitute the inorganic P pool in soil. Hence, the loss given by the rate of strong sorption is applied to the total inorganic P pool.

Comment 12

200 Line 186: Is QD here the same as q in equation 6? Both are described as runoff.

Response 12

They are both the runoff from different perspectives. Q_D represents the runoff per soil layer, while q represents the total runoff from the soil layers.

Comment 13

205 Line 189-190: Similar to C:N, the variable C:P ratio of leaf and root tissues is mentioned here but there is no explanation or equation for what controls the value of the ratio.

Response 13

We have added the following text to section 2.3.3, to introduce and explain the equation:

$$CP_{leaf} = \frac{C_{leaf}}{P_{leaf}}, \quad (10)$$

210 where C_{leaf} is the carbon content in leafs and P_{leaf} is the phosphorus content in leafs. CP_{leaf} is one of the most important nutrient limitators in the model. The limiting effect of CP_{leaf} is when its value is lower than the maximum CP leaf ratio parameter $CP_{leafmax}$. This leads to biomass reduction. In contrast to CN_{leaf} , CP_{leaf} does not control the maximum carboxilation rate of RuBISCO. A more detailed description of nutrient limitation can be found in section 2.4.

215 Comment 14

Line 195: the vegetation P change over time

Comment 14

Thanks for noticing. You are certainly correct. It has been changed to:

220 where V_{egp} is the vegetation P change over time,

Comment 15

Line 200-204: Equation 16 needs some conceptual explanation. It's not directly clear from the equation and description what process this is representing. Are the nitrogen costs related to actual nitrogen availability?

225 **Response 15**

The following has been added to the description of equation 16:

Here, the nitrogen cost refers to the nitrogen required for protein structures involved in the metabolism of phosphorus in plants.

230 **Comment 16**

Line 232: I did not find an explanation of $CP_{leafmax}$, $CN_{leafmax}$, R_{leafP} , or R_{leafN} in the text or equations showing how the model depends on these parameters. If these parameters are important enough to be the basis for the sensitivity analysis, they should be clearly explained in the text.

Response 16

235 Than you for your comment. The following text has been added:

$CP_{leafmax}$ and $CN_{leafmax}$ are the maximum leaf CP or CN ratios respectively. If the values of CP_{leaf} and CN_{leaf} are above these thresholds the model will take the system to be nutrient limited by either P or N. R_{leafN} and R_{leafP} are parameters that represents the resorption of nitrogen and phosphorus in leaves. This partly controls the loss of nitrogen and phosphorus from vegetation to the litter pool. V_{maxP} and V_{maxN} are the P and N maximum uptake rates.

Comment 17

Line 259-261: This is not shown in any of the figures. This statement could be supported by showing a map of biomass from the different simulations and the difference from the C-only simulation

245 **Response 17**

Thank you for your comment. We do show this later in the paper. Figure 4 shows the difference in PFTs fractions. Although not directly, broadleaf trees PFTs fractions are shown to be reduced in CNP compared to C only. This reduction indicates a loss of wood biomass and supports the statement in Line 259-261.

250 The high GPP in the baseline simulation can be explained by the overestimation of the vegetation biomass especially broadleaf trees in tropical regions as stated in Mengis et al. (2020). The representation of vegetation biomass is linked to the PFTs fractions in the model.

Comment 18

255 *Line 274: Difference in tropical vegetation biomass is also not shown in any figure. This could be shown as a map or an average biomass value by latitude for different simulations.*

Response 18

Thank you for your comment. We do show this later in the paper. Figure 4 shows the difference in PFTs fractions. Although not directly, broadleaf trees PFTs fractions are showed to be reduced in CNP compared to C only. This reduction indicates a loss of wood biomass and supports the statement in Line 274.

260

While CNP (72 Pg C yr⁻¹) resulted in lower values, due to the reduction of tropical vegetation biomass. The reduction of tropical biomass mainly in broadleaf trees carbon is reflected in the fraction of the PFT shown in the model output.

Comment 19

265 *Line 294: I would reorder the figures so they appear in the order described – 6 is described before 4 and 5.*

Response 19

The figures were reordered as suggested.

Comment 20

270 *Line 304-306: This statement should be supported by a figure showing vegetation carbon as a map or latitudinal gradient.*

Response 20

Thank you for your comment. Figure 4 shows the difference in PFTs fractions. Although not directly, broadleaf trees PFTs fractions are showed to be reduced in CNP compared to C only. This reduction
275 indicates a loss of wood biomass and supports the statement in Line 304-306.

Comment 21

Line 314: Figure 7 does not show the difference in N compared to Wania et al. (2012), so this statement cannot be evaluated.

Response 21

280 You are correct, there is not direct comparison in the paper in regards with this statement. The only way is to search for Wania et al. (2012) paper. The paragraph was change to:

Globally, the CNP simulated soil nitrogen is reduced compared to the original N structure in the UVic ESCM version 2.9 presented by Wania et al. (2012). The primary differences between Wania et al. (2012)
285 N cycle and the current version are the soil layer structure and the stoichiometry response to N limitation. In the former, N could be transfer from other pools when N was outside of the ratios threshold and thereby be considered to be limiting vegetation.

Comment 22

290 *Figure 4: This figure was difficult to interpret because only the differences in PFT fractions were shown, and not the actual fractions. There also is not much explanation of how relative PFT distributions relate to N and P cycling in the model so it's not clear how relevant this is to the main model developments being described.*

Response 22

The answer to the comment is in line with comment 3, 13 and the limitation description of the model.
295 The biomass reduction effect of N and P affect PFTs in different ways based on $CN_{leafmax}$ and $CP_{leafmax}$ parameters. Understanding the effect of P and N is quite insightful as we can observed how different type of vegetation are responding to the limitation. This allows for a better representation of vegetation biomass and shows the degree of biomass reduction that our parameters and constrained are doing. From a different perspective, looking and N and P as a tool to represent the terrestrial system as closer to nature
300 as possible N and P effect to PFTs gains a lot of value.

Comment 23

*Figure 5: There is no explanation of how these correlations are calculated. Is this based on relative amount of all PFTs in each grid cell? This does not seem to be the most useful test of the model since many grid cells are dominated by one or two PFTs. Wouldn't variation in PFTs across grid cells be a
305 more useful metric to test?*

Response 23

The correlation encompasses PFTs across grid cells. We have adjusted the caption of Figure 5 to make this clear:

310 [Figure 5. PFTs fractions across grid cells in the UVic ESCM for 2008-2012, CNP correlation to Poulter et al. \(2015\) PFTs dataset.](#)

Comment 24

Lines 326-328: There is an order of magnitude range in the different estimates, so they don't seem like a very strong constraint on the model. Is there any expectation of which set of estimates might be more accurate?

315

Response 24

Yes, the difference between the lower range and higher range are relatively large. We can not state which estimate is more accurate, global nitrogen and phosphorus fluxes are currently largely uncertain. The likely answer is that the most accurate value holds the middle position among the range of models. This uncertainty becomes even larger for phosphorus.

320

Comment 25

Line 329: Is CN ratio referring to soil, vegetation, or whole ecosystem? Figure 7 is also unclear about this.

Response 25

It is referring to vegetation CN ratio. The title of the plot has been changed from "CN ratio" to "Vegetation CN ratio".

325

Comment 26

Line 336: Equation 16 included some nitrogen cost of phosphatase parameters. Does this not connect the N and P cycles in a way that could allow co-limitation? It's hard to tell without more explanation of that equation.

330

Response 26

You are technically correct. It was poorly written and has been be reworded in the text. In the model structure there are some co-limitation factors. We do not account directly for co-limitation. A description of the limitation in the model which has been added in section 2.4.

335

The model assumes nutrient limitation when the concentration of nitrogen or phosphorus is higher than the maximum CN ($CN_{leafmax}$) and CP ($CP_{leafmax}$) in leaves. For grids with nutrient limitation the carbon in leaf is reduced to match the maximum CN or CP ratios in leaves. The carbon that is removed is transferred to the litter pool. This reduction can happen for one or both nutrients until the ratio is met. The following
340 equations regulate the reduction of biomass based on nutrient limitation:

$$C_{leaflimitedn} = N_{leaf}CN_{leafmax}, \quad (11)$$

$$C_{leafdiffn} = C_{leaf} - C_{leaflimitedn}, \quad (12)$$

345 $C_{leaflimitedp} = N_{leaf}CP_{leafmax}, \quad (13)$

$$C_{leafdiffp} = C_{leaf} - C_{leaflimitedp}, \quad (14)$$

were $C_{leaflimitedn}$ and $C_{leaflimitedp}$ are the carbon concentration in leaves if the system is considered to be limited. $C_{leafdiffn}$ and $C_{leafdiffp}$ are the the carbon lost due to nutrient limitation and their value are sum
350 in the litterfall equation when the system is in nutrient limitation.

Comment 27

Line 341: The model does not include anthropogenic N inputs, so is it reasonable to compare it with estimates that do include anthropogenic inputs? Couldn't this indicate that the model overestimates natural sources since anthropogenic N inputs in reality are very high?

355 **Response 27**

We do account for atmospheric anthropogenic N deposition. We do not account for agricultural inputs in this paper. The model structure for N₂O might indeed overestimate natural sources as you mentioned but are within the range of uncertainty. For a follow-up project (part of a paper in preparation) we applied agricultural fertilization and retested the model N₂O and the resulting emission is increased from 10 to 25
360 %. The model estimation of anoxic fractions from wetlands might be mitigating the effect of agricultural fertilization. Nonetheless, the model does acceptably well.

Comment 28

*Line 372-374: Global terrestrial P should be included as a line in Table 6. Table 6 does not indicate estimates from terrestrial P models (or at least does not indicate which estimates are from models versus
365 measurement syntheses). What is the evidence that other models are underestimating P in subsoils and not that this model overestimates P in subsoils?*

Response 28

Thank you for the comment. Global terrestrial P has been included in table 6. Table 6 was change accordingly. The statement is based only on the comparison with He et al. (2021) datasets. Hence, it was
370 removed from the text.

Comment 29

*Figure 10: This figure doesn't make much sense to me. Why would the model N:P leaf ratio be perfectly linear with respect to latitude? Is the Reich and Oleksyn relationship a simple linear function with respect to latitude? If so, this seems like a very simplistic test of a complex model. Also, it is difficult to interpret
375 this figure because there was no explanation provided for what controls leaf N:P ratio in the model.*

Response 29

It is a linear function with respect to latitude. This comparison was set to use the same metric as in Wang et al. 2010. While simplistic this shows if our model correctly displays the global limitation pattern where N is abundant in tropics and scarce in high latitudes and opposite to P. It is of course not that simplistic in nature and in reality without a linear function our model nor the observations describe a perfect line.

Comment 30

Table A1: There is no reason this short table should be in a separate appendix. It's an important part of the model and should be in the main text.

Response 30

Table A1 was moved to the main text after table 2 "Table 2. Updated nitrogen cycle parameters. See appendix A.1 for values that vary for each PFT."

General comment

390 *Sisto et al. describe the modifications to the UVic ESCM intermediate complexity Earth system model done to represent new nitrogen (N) and phosphorus (P) cycling processes. The new model is evaluated based on global datasets of C, N, and P pools and fluxes. Incorporating P cycles into ecosystem models is timely and important work. The equations and processes are clearly described. Overall, this manuscript is well-organized and easy to read. However, some concerns need to be addressed or clarified.*

Response

395 Thank you for the positive feedback

General comment

400 *Insufficient description of methodology: The model is insufficiently described in 2.4 Nitrogen and phosphorus limitation. The critical aspect of nutrient effects on C cycling is the competition of plants and microbes for limited nutrients. This aspect is not described here. How do you deal with cases in which available soil P and mineralization are insufficient to satisfy the P immobilization demand? In addition, the N, P limitation on the C cycle in vegetation is described too simply; we don't have enough details. (The Parameters for sensitivity analysis are not described enough in the model structure).*

Response

405 Thank you for your comment. A more in depth description was added in section 2.4. You are correct the competition of plants and microbes for limited nutrients is indeed important for nutrient limitation. Having said that, the UVic ESCM is a global model and our vegetation is represented by a top-down approach. We do account for plant competition in the model and it is shown in the PFTs change in the text and figures. We do not account directly for microbial interaction. That by itself could be a great addition for our model but remains an avenue for future model development. Some aspect of nutrient cycling need

410 to be simplified. Hence, some interactions and dynamics are not captured within are global model struc-
ture. It is true that the parameters of the sensitivity analysis are not described sufficiently. A more detailed
description was added.

Section 2.4:

415

The model assumes nutrient limitation when the concentration of nitrogen and phosphorus is higher
than the maximum CN ($CN_{leafmax}$) and CP ($CP_{leafmax}$) in leaves. For grids with nutrient limitation the
carbon in leaf is reduced to match the maximum CN or CP ratios in leaves. The carbon that is reduced is
transferred to the litter pool. This reduction can happen for one or both nutrients until the ratio is met. The
420 following equations regulate the reduction of biomass based on nutrient limitation:

$$C_{leaflimitedn} = N_{leaf}CN_{leafmax}, \quad (15)$$

$$C_{leafdiffn} = C_{leaf} - C_{leaflimitedn}, \quad (16)$$

425 $C_{leaflimitedp} = N_{leaf}CP_{leafmax}, \quad (17)$

$$C_{leafdiffp} = C_{leaf} - C_{leaflimitedp}, \quad (18)$$

where $C_{leaflimitedn}$ and $C_{leaflimitedp}$ are the carbon concentration in leaves if the system is considered to
be limited. $C_{leafdiffn}$ and $C_{leafdiffp}$ are the the carbon lost due to nutrient limitation and their value are
430 sum in the litterfall equation when the system is in nutrient limitation.

Parameter sensitivity:

$CP_{leafmax}$ and $CN_{leafmax}$ are the maximum leaf CP or CN ratios respectively. If the values of CP_{leaf} and
435 CN_{leaf} are above these thresholds the model will take the system to be nutrient limited by either P or N.

R_{leafN} and R_{leafP} are parameters that represents the resorption of nitrogen and phosphorus in leafs. This partly controls the loss of nitrogen and phosphorus from vegetation to the litter pool. V_{maxp} and V_{maxn} are the P and N maximum uptake rates.

General comment

440 *Lack of evaluation: Even though this is an ESM model, this paper focuses more on land surface processes. The evaluation should include different scales, such as site and regional levels. We can only see the global scale values. In addition, do you capture spatial gradients in GPP or seasonal or interannual variation in GPP?*

Response

445 Thank you for your comment. While it is true that a site or regional level comparison would be beneficial for the paper, the UVic ESCM resolution is not suitable for those type of comparison. Depending on the definition of what is a region we might present results of nutrient variables. However, the lack of observations in a region large enough to be compatible for comparison is a challenge. There are examples of studies that compared nitrogen and phosphorus in sites (e.g. Goll et al. (2017); Nakhavali et al. (2021)).

450 In those studies the models have considerably higher resolution than ours and the land-surface can be de-coupled from the atmosphere, allowing prescribed local vegetation and meteorology. Hence, a site comparison is more feasible. Furthermore, the UVic ESCM is always used for global studies. Thereby, a global approach was thought to be the most reasonable. We added the following figure representing seasonal and global GPP compared to FLUXCOM GPP dataset.

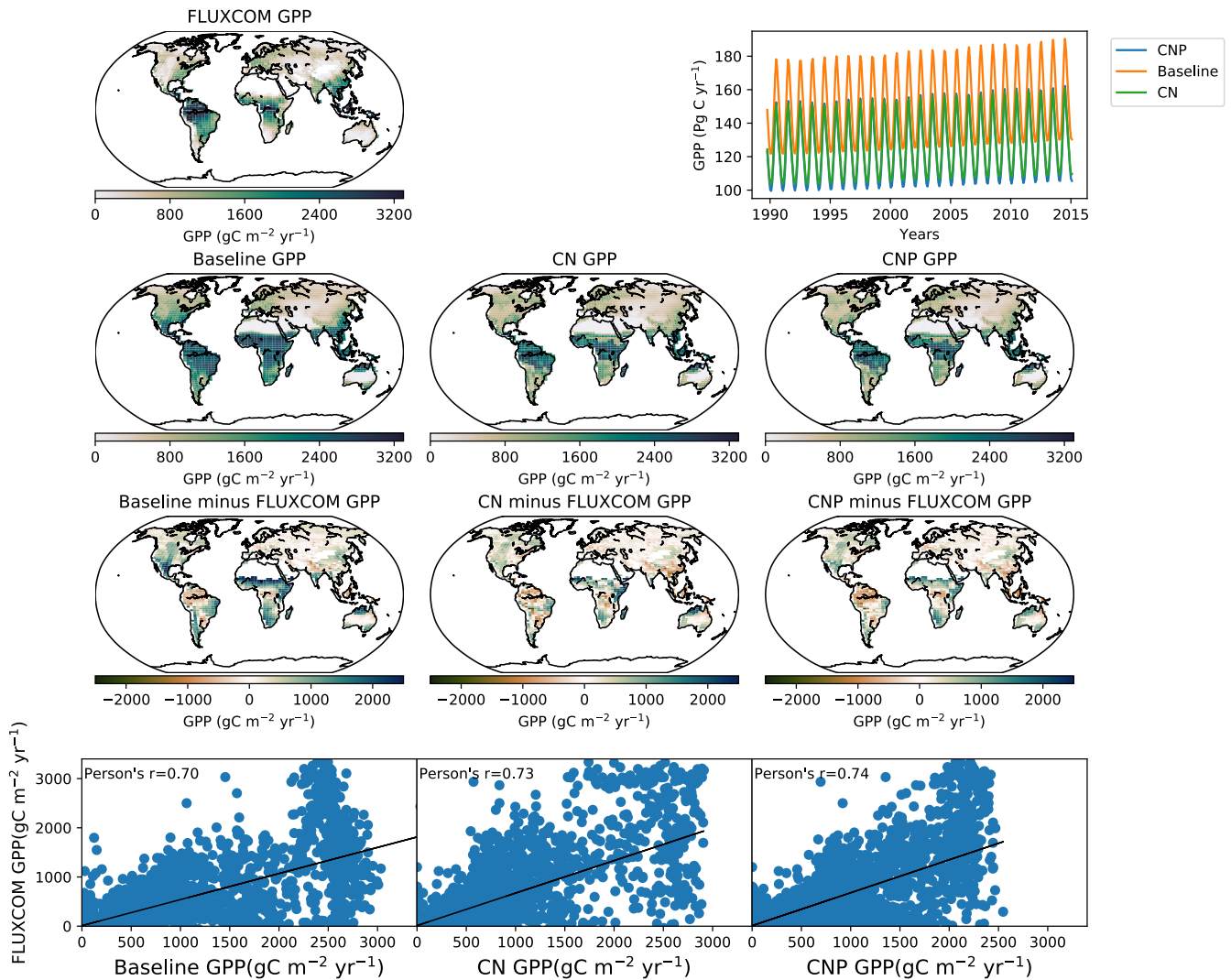


Figure 2. Upper left figure shows FLUXCOM GPP dataset from 2000-2010, upper right figure shows the seasonal GPP from 1990-2015 for Baseline, CN and CNP. The second line from left to right shows the global GPP from 2000-2010 for Baseline, CN and CNP. The third line from left to right shows the difference between Baseline, CN and CNP and FLUXCOM GPP dataset. The fourth line from left to right shows the correlation of Baseline, CN and CNP to FLUXCOM GPP dataset.

455 The GPP distribution from Baseline, CN and CNP reproduce FLUXCOM dataset values reasonably well. The seasonal pattern of GPP is also well represented within our simulations as shown in Fig. 3. The addition of nutrients improves the representation of GPP, where CNP had the highest correlation with FLUXCOM GPP dataset.

General comment

460 *I expect the author to provide an understanding of the model dynamics, such as the C, N, and P processes in vegetation and soil, and how they couple and interact.*

Response

Thank you for your comment. A more detailed description has been added in section 2.4 describing the coupling of nutrient limitation.

465 The model assumes nutrient limitation when the concentration of nitrogen or phosphorus is higher than the maximum CN ($CN_{leafmax}$) and CP ($CP_{leafmax}$) in leaves. For grids with nutrient limitation the carbon in leaf is reduced to match the maximum CN or CP ratios in leaves. The carbon that is reduced is removed to the litter pool. This reduction can happen for one or both nutrients until the ratio is met. The following
470 equations regulate the reduction of biomass based on nutrient limitation:

$$C_{leaflimitedn} = N_{leaf}CN_{leafmax}, \quad (19)$$

$$C_{leafdiffn} = C_{leaf} - C_{leaflimitedn}, \quad (20)$$

475 $C_{leaflimitedp} = N_{leaf}CP_{leafmax}, \quad (21)$

$$C_{leafdiffp} = C_{leaf} - C_{leaflimitedp}, \quad (22)$$

were $C_{leaflimitedn}$ and $C_{leaflimitedp}$ are the carbon concentration in leaves if the system is considered to be limited. $C_{leafdiffn}$ and $C_{leafdiffp}$ are the the carbon lost due to nutrient limitation and their value are sum
480 in the litterfall equation when the system is in nutrient limitation.

Specific comments

Comment 1

Line 68, 71: Some references related to the recent CNP model should include. (Fleischer et al., 2019; Thum et al., 2019; Wang et al., 2020; Yang et al., 2014)

485 Response 1

Thank you for the comment. The suggested references were included in the text.

of P into ESM structures is possible and that it improves the representation of vegetation biomass in tropical regions (Wang et al. , 2007, 2010; Goll et al. , 2012, 2017; Fleischer et al. , 2019; Thum et al. ,
490 2019; Yang et al. , 2019; Wang et al. , 2020; Nakhavali et al. , 2021).

Comment 2

Line 153: It is section 2.4, not 2.5

Response 2

Thank you noticing. The line was changed accordingly.

495

Section 2.4 presents a detailed explanation of nutrient limitation for N and P.

Comment 3

Line 155: *The names of inorganic P pools need to be more consistent. You named the labile P pool, but in figure 1, it is Dissolved Inorganic P. Different models have a different definitions of labile P and dissolved inorganic P, such as (Goll et al., 2017; Thum et al., 2019; Wang et al., 2020; Yang et al., 2014). So, giving clear definitions and keeping the name consistent is better.*

Response 3

Thank you noticing. Dissolved inorganic P was changed to labile P thorough the text accordingly.

Comment 4

Line 170: *Which part of the approach is more controllable? Explain it more precisely.*

Response 4

Thank you for your comment. An explanation has been added to the line.

Here we only apply Wang et al. (2010) approach as we found it to be more controllable and an advantage to the planned coupling of P flux from land into the ocean. Hartmann et al. (2014) requires the estimation of runoff by the model structure. Hence, while representing a dynamical P release it needs to be carefully assessed so that no extreme overestimation or underestimation are represented regionally. Wang et al. (2010) approach provides constant input without variability which in this particular case is favorable.

Comment 5

Line 221: *In equation 19, if the CN increases, the V_{cmax} will increase; how is plant productivity reduced? Line 219 describes it in the opposite direction.*

Response 5

Thank you for your comment. In equation 9 we multiply CN_{leafi} not CN_{leaf} . CN_{leafi} is described as the inverse of CN_{leaf} . Hence, line 219. CN_{leafi} was changed to $CN_{invleaf}$.

520 Comment 6

Line 224: The NP limitation needs to be clarified. Is it based on Liebig's Law of the Minimum?

Response 6

Thank you for your comment. No, the nutrient limitation has a more simplistic approach. The following was added in section 2.4.

525

The model assumes nutrient limitation when the concentration of nitrogen and phosphorus is higher than the maximum CN ($CN_{leafmax}$) and CP ($CP_{leafmax}$) in leaves. For grids with nutrient limitation the carbon in leaf is reduced to match the maximum CN or CP ratios in leaves. The carbon that is reduced is transferred to the litter pool. This reduction can happen for one or both nutrients until the ratio is met. The following equations regulate the reduction of biomass based on nutrient limitation:

530

$$C_{leaflimitedn} = N_{leaf}CN_{leafmax}, \quad (23)$$

$$C_{leafdiffn} = C_{leaf} - C_{leaflimitedn}, \quad (24)$$

$$535 \quad C_{leaflimitedp} = N_{leaf}CP_{leafmax}, \quad (25)$$

$$C_{leafdiffp} = C_{leaf} - C_{leaflimitedp}, \quad (26)$$

were $C_{leaflimitedn}$ and $C_{leaflimitedp}$ are the carbon concentration in leafs if the system is considered to be limited. $C_{leafdiffn}$ and $C_{leafdiffp}$ are the the carbon lost due to nutrient limitation and their value are sum
540 in the litterfall equation when the system is in nutrient limitation.

Comment 7

Line 248: Phosphorus dataset is Pdataset in the equation (20).

Response 7

Thank you for noticing. The lines has been changed to:

545

the soil layer depth and $P_{dataset}$ (kg P (kg soil)⁻¹) is He et al. (2021) dataset.

Comment 8

Line 273: Should the baseline have a higher NPP? If I understand it correctly.

Response 8

550 Only for CN not CNP. This was observed since the first construction of the nitrogen cycle in the UVic ESCM: Wania et al. (2012), argued that the reason behind the high NPP was the dependence of autotrophic respiration on N content in leaf, root and stem which are based on the original MOSES/TRIFFID version (Cox et al. , 1998). CNP does result in a lower value.

Comment 9

555 *Line 277: This part is hard to follow and needs to be articulate.*

Response 9

Thank you for your comment. The paragraph was rewritten to:

560 In CN and CNP, the reduction of wood CN ratios and higher leaf content than in CN and CNP which fluctuates from a minimum to a maximum value gives place to the reduction of the maintenance respiration which reduces the autotrophic respiration and consequently NPP. Furthermore, in the new CNP version while wood CN remains to be fixed the stoichiometrical reduction of wood carbon by the lack of P availability decreases wood carbon even more especially in tropical forests and other tropical ecosystems.

Comment 9

565 *Line 288: In fig 3, why CN has a larger atmospheric CO₂ pool than CNP?*

Response 9

The main driver for that response is the increase of ocean carbon uptake, given the initial reduction of land carbon sink in CNP. That offsets the effects of phosphorus. It gets a bit more complicated than this. Given our model limiting structure in this study phosphorus limitation had low impact. We recently have
570 assessed that atmospheric CO₂ is indeed lower in CNP for 2020 under a higher impact of phosphorus limitation in the system. The impact is determined by $CP_{leafmax}$, the maximum phosphorus in leaves set in the model.

Comment 10

Line 298: Could you describe your mechanisms of dynamic PFT, maybe in the supplement?

575 Response 10

The following was added to the description of TRIFFID in line 87:

The terrestrial vegetation is simulated by a top-down representation of interactive foliage and flora including dynamics (TRIFFID) representing vegetation interaction between 5 functional plant types: broadleaf trees, needleleaf trees, shrubs, C3 grasses, and C4 grasses that compete for space in the grid following the Lotka-Volterra equations (Cox , 2001). Net carbon fluxes estimated in the model updates the total areal coverage, leaf area indexes and canopy height for each PFT. For each PFT the carbon fluxes are derived from a photosynthesis-stomatal conductance model (Cox et al. , 1998).

Comment 11

585 *Line 329: Tropical areas are supposed to have enough N, so a lower NP ratio.*

Response 11

We are unsure of exactly what the reviewer was asking in this comment. Line 329: The global pattern of CN ratio is similar to Wania et al. (2012) structure with the highest located in tropical regions especially. Hence, it's not NP but CN.

590 **Comment 12**

Line 332: uncertainty (95-730). This is a huge range.

Response 12

Thank you for your comment. Yes, depending of the species the C:N ratio can vary largely. For example, Martin et al. (2014) shows a range of tropical wood C:N ratio from 92 to 1360 in different Panamanian tree species.

Comment 13

Line 354: I wondered why Fertilization inputs did not use here.

Response 13

This paper represents the core skeleton of the phosphorus cycle. The phosphorus inputs data and structure
600 were not at the point of utilization. Hence, we decided to only account for a natural P cycle. This is a
priority in future development plans. Furthermore, P fertilization forcing is not yet available in future
scenarios.

Comment 14

*Line 366: “This underestimation is likely the result of a high mineralization rate.” Do you check the soil
605 C pool? And maybe the CP ratio also gives some clues about the underestimation of organic P.*

Response 14

After writing the paper and analyzing further simulations. We’ve realised that the underestimation of P
comes simply by the lack of P fertilizers. Hence line 366 will be changes to: This underestimation is likely
the result of the lack of P fertilization on land.

General comment

The paper is mostly presenting a new model, which is appropriate for this journal. However, more “science” needs to be presented with the model in order to determine if their new tool is appropriate for further studies at its present state. For example, the authors could provide more model validation and
615 *intercomparison with available products and similar models. I appreciate the evaluation of the Keeling curve and FLUXCOM GPP, but this is only a validation of CO₂. What about energy? What about water? What about the nutrient cycles?*

Response

Thank you for your comment. The purpose of this paper was to describe a terrestrial nitrogen and phosphorus cycle adapted, developed and implemented for the UVic ESCM version 2.10. As such the main
620 changes we wanted to capture in this paper are in the terrestrial system, especially the vegetation. While the Keeling curve does only validate CO₂, the FLUXCOM GPP validates the gross primary productivity, a relevant variable to assess the representation of the metabolism in vegetation. While we appreciate your suggestions, assessing energy and water were recently extensively evaluated for the latest public version
625 of the model (UVic ESCM 2.10) by Mengis et al. (2020). The addition of terrestrial N and P cycles had only a minor effect of these variables, and thus reassessing them would add considerable length and little value to the present paper. Finally, we do validate nutrient cycles with other relevant modelling studies and available observations, and through our revisions have made this clearer. One of the challenges of representing nutrients in Earth system models is the lack of observations from where to validate.

630 **General comment**

I suggest adding tool like ILAMB (Collier et al., 2018) for evaluating model performance throughout variables related to the carbon, water and energy cycles, as well as a sensitivity analysis of current with meteorological variables.

Response

635 Thank you for your comment. A detailed evaluation was conducted recently for the UVic ESCM version 2.10 by Mengis et al. (2020). Regarding the carbon cycles, we did use ILAMB dataset for GPP (FLUX-COM). As for the nutrient cycles, ILAMB is not yet adequate to assess nitrogen or phosphorus in ESMs (Spafford and MacDougall, 2021).

The following text has been added in section 2.1:

640

Mengis et al. (2020) merged previous version of the UVic ESCM and evaluated its performance representing carbon and heat fluxes, water cycle and ocean tracers.

General comment

The authors should add comparisons with other similar studies. They do that to an extent (Poulter et al.,
645 *2015; He et al., 2021), but there are so many other similar and recent studies that should be added, such as: Wang, Y., Ciais, P., Goll, D., Huang, Y., Luo, Y., Wang, Y.P., Bloom, A.A., Broquet, G., Hartmann, J., Peng, S., Penuelas, J., Piao, S., Sardans, J., Stocker, B.D., Wang, R., Zaehle, S., Zechmeister-Boltenstern, S., 2018. GOLUM-CNP v1.0: A data-driven modeling of carbon, nitrogen and phosphorus cycles in major terrestrial biomes. Geosci. Model Dev. 11, 3903–3928. <https://doi.org/10.5194/gmd-11-3903-2018>. Goll,*
650 *D.S., Vuichard, N., Maignan, F., Jornet-Puig, A., Sardans, J., Violette, A., Peng, S., Sun, Y., Kvakic, M., Guimberteau, M., Guenet, B., Zaehle, S., Penuelas, J., Janssens, I., Ciais, P., 2017. A representation of the phosphorus cycle for ORCHIDEE (revision 4520). Geosci. Model Dev. <https://doi.org/10.5194/gmd-10-3745-2017>. Braghieri, R.K., Fisher, J.B., Allen, K., Brzostek, E., Shi, M., Yang, X., Ricciuto, D.M., Fisher, R.A., Zhu, Q., Phillips, R.P., 2022. Modeling global carbon costs of plant nitrogen and phosphorus*
655 *acquisition. J. Adv. Model. Earth Syst. e2022MS003204. <https://doi.org/10.1029/2022MS003204>*

Response

Thank for your comment. In our study we compare our results with several other studies such as: Reich and Oleksyn (2004); Mengis et al. (2020); Wania et al. (2012); Global Soil Data Task Group (2000);

Zaehle et al. (2010); Yang et al. (2009); Xu-ri and Prentice (2008); Crippa et al. (2021); Smil (2000);
660 Mackenzie (2002); Wang et al. (2010); Yang et al. (2013).

We thank you for the suggested sources. Many of these papers use experimental designs examining much smaller spatial scales and hence are difficult to compare with results from our experimental design. For example, while some equations we use are based on Goll et al. (2017), in their study a regional approach is used, which cannot be compared to our global study. There are indeed other studies that address
665 phosphorus in regional scales, but such studies cannot easily be scaled-up to the global scale, and nor can global models be scaled-down to the site-scale for comparison.

The following comparison have been added:

670 Section 3.2.1:

Our model estimates a mean biological nitrogen fixation for 2010-2020 of 119 Tg N yr⁻¹. This value is above 35 Tg N yr⁻¹ from Braghieri et al. (2022) and within the range of 52–130 Tg N yr⁻¹ presented by Barnard and Friedlingstein (2020)

675

Section 3.2.2:

The total amount of vegetation nitrogen (2.20 Pg N) was lower than the previous N cycle (2.94 Pg N, Wania et al. (2012)). These values are similar to Zaehle et al. (2010) (3.8 Pg N) and Wang et al. (2018) (3.9 PgN) but lower than Li et al. (2000) (16Pg N) and Yang et al. (2009). (18 Pg N). Our tropical (30 to 45gN m⁻²) and boreal forest vegetation nitrogen (20 to 35gN m⁻²) results are lower than from Wania et al. (2012) (30 to 40gN m⁻²), and those of Xu-ri and Prentice (2008) and Yang et al. (2009) (both studies ranged between of 150 to 400 gN m⁻²).

685 Table 6:

Variables	Value (Pg P)	References (Pg P)
Total inorganic P	20.8	35-40 Smil (2000)
Total organic P	3.5	36 Mackenzie (2002) 26.5 Wang et al. (2010) 13.7 Wang et al. (2018) 5-10 Smil (2000)
Labile P	1.4	5 Mackenzie (2002) 5.7 Wang et al. (2010) 8.6 Yang et al. (2013) 1.5 Wang et al. (2010)
Sorbed P	1.1	3.6 Yang et al. (2013) 1.7 Wang et al. (2010)
Strongly sorbed P	12	7.6 Wang et al. (2010)
Occluded	6.3	9.0 Wang et al. (2010)
Vegetation P	0.2	0.4 Wang et al. (2010) 0.5 Smil (2000) 0.5 Wang et al. (2018) 0.2 Wang et al. (2018)
P Litter	0.01	0.5 Mackenzie (2002) 0.04 Wang et al. (2010) 0.03 Wang et al. (2018)

General comment

Moreover, why is this model needed? Is it just another model on top of the CMIP simulations? A deeper discussion about how this model relates to other models and the future of climate modeling is needed.

690 Response

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

The UVic ESCM has been a critical tool in developing the cumulative emissions framework to climate mitigation (Zickfeld et al. , 2009; Matthews et al. , 2009; Matthews and Weaver , 2009; MacDougall and Knutti , 2016; Mengis et al. , 2018; Tokarska et al. , 2019) due to its low computations cost and strict enforcement of matter and energy conservation the model is capable of conducting a host of simulation beyond the limits of most other models, but with higher resolution than other EMICs (e.g. Montenegro et al. (2007); Matthews and Caldeira (2008); Keller et al. (2014); MacDougall and Knutti (2016); MacDougall (2017); Pahlow et al. (2020); Kvale et al. (2021)) . As terrestrial nutrient limitation constrains the carbon cycle in nature, the new nitrogen and phosphorus modules allows addressing research questions relating carbon budgets, carbon cycle and CH₄ feedbacks, carbon dioxide removal and permafrost carbon cycle, among other questions. Furthermore, the nitrogen and phosphorus cycles can represent environmental and climate critical processes such as the release of N₂O, agricultural impacts on terrestrial soils and coastal lines, eutrophication, anoxic events and nutrient fluxes from land to ocean.

705 **General comment**

Why do you have a schematic of the P cycle but not the N cycle? Be consistent.

Response

Thank you for the suggestion. The following figure has been added:

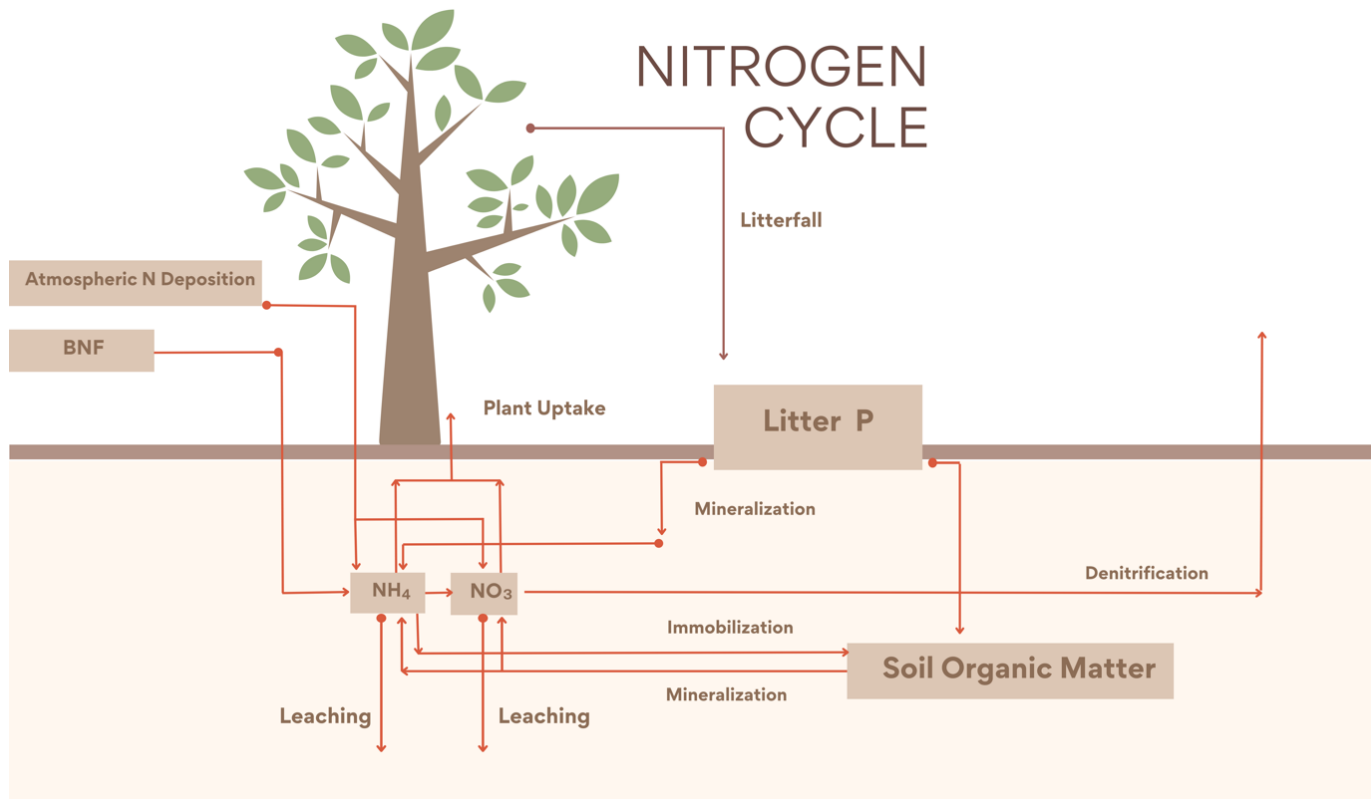


Figure 3. Diagram representing the UVic ESCM nitrogen cycle.

710 Specific comments

Comment 1

Introduction needs work in properly linking biodiversity with other aspects of the biogeochemical cycles in the Earth system and climate change. I added a few extra references, but a more thorough literature review is required. Introduction: I find the introduction a bit shallow and very model centric. I understand

715 *this is a modeling journal, but the reader would benefit from more general scientific discussions at the beginning. You may want to cite: Wieder, W.R., Cleveland, C.C., Smith, W.K., Todd-Brown, K., 2015. Future productivity and carbon storage limited by terrestrial nutrient availability. Nat. Geosci. 8, 441–444. <https://doi.org/10.1038/ngeo2413> Zaehle, S., Jones, C.D., Houlton, B., Lamarque, J.-F., Robertson, E.,*

720 *2015. Nitrogen Availability Reduces CMIP5 Projections of Twenty-First-Century Land Carbon Uptake. J. Clim. 28, 2494–2511. <https://doi.org/10.1175/JCLI-D-13-00776.1>*

Response

Thank you for the comment. The following text was added to the introduction:

725 Biodiversity plays a crucial role in biogeochemical cycles. Microbial diversity for example enables ni-
trogen pathways that only some taxa can metabolize. Plant diversity, is linked to soil health and function-
ing, and is core for the nitrogen and phosphorus cycles. Overall, biodiversity constitute an environmental
resilience factor to abrupt changes (Van Oijen et al. , 2020). However, implementing such dynamics re-
mains far beyond the capabilities for the present generation Earth systems models.

Comment 2

730 *Line 2: Earth system models (ESMs)*

Response 2

Changed

Comment 3

735 *Line 6: Nitrogen (N). It is not the first time the word nitrogen appears. Please define acronyms in first
appearance.*

Response 3

Thank you for your comment Nitrogen (N) was defined in line 4.

Comment 4

Line 30: Missing References:

740 **Response 4**

Thank you for your comment. Goll et al. (2017) and Wang et al. (2020) were added as references.

Comment 5

Line 63: This isn't true. Although ESM modeling with phosphorus is indeed limited. See: Wang, Y., Ciais, P., Goll, D., Huang, Y., Luo, Y., Wang, Y.P., Bloom, A.A., Broquet, G., Hartmann, J., Peng, S., Penuelas, J., Piao, S., Sardans, J., Stocker, B.D., Wang, R., Zaehle, S., Zechmeister-Boltenstern, S., 2018. GOLUM-CNP v1.0: A data-driven modeling of carbon, nitrogen and phosphorus cycles in major terrestrial biomes. Geosci. Model Dev. 11, 3903–3928. <https://doi.org/10.5194/gmd-11-3903-2018>. Goll, D.S., Vuichard, N., Maignan, F., Jorner-Puig, A., Sardans, J., Violette, A., Peng, S., Sun, Y., Kvakic, M., Guimberteau, M., Guenet, B., Zaehle, S., Penuelas, J., Janssens, I., Ciais, P., 2017. A representation of the phosphorus cycle for ORCHIDEE (revision 4520). Geosci. Model Dev. <https://doi.org/10.5194/gmd-10-3745-2017>. Braghiere, R.K., Fisher, J.B., Allen, K., Brzostek, E., Shi, M., Yang, X., Ricciuto, D.M., Fisher, R.A., Zhu, Q., Phillips, R.P., 2022. Modeling global carbon costs of plant nitrogen and phosphorus acquisition. J. Adv. Model. Earth Syst. e2022MS003204. <https://doi.org/10.1029/2022MS003204>

Response 5

755 Neglected was changed for rare.

Comment 6

The Baseline GPP of the model substantially overestimates FLUXCOM, why is that?

Response 6

Thank you for your comment. It is explained in the paper in section 3.1.1 line 258: The high GPP in the baseline simulation can be explained by the overestimation of the vegetation biomass especially broadleaf trees in tropical regions stated in Mengis et al. (2020).

Comment 7

Line 274: NPP of 72 PgCyr also seems a bit high. What is a good estimate of global NPP? Add values in the study.

765 Response 7

That is correct, we are very close to the upper range of the modelled NPP range Li et al. (2015): 21.5 to 69.3 Pg C yr⁻¹. The following was added in the line:

770 Similar to Wania et al. (2012), we found higher values of NPP for CN (77.4 Pg C yr⁻¹) compared to the baseline simulation (74.2 Pg C yr⁻¹). While CNP (72 Pg C yr⁻¹) resulted in lower values, due to the reduction of tropical vegetation biomass. CN and CNP results are close to the upper range (21.5 to 69.3 Pg C yr⁻¹) of simulated NPP showed in Li et al. (2015).

Comment 8

775 *Figure 7. These values could be compared to other studies as well. See: Braghieri, R.K., Fisher, J.B., Allen, K., Brzostek, E., Shi, M., Yang, X., Ricciuto, D.M., Fisher, R.A., Zhu, Q., Phillips, R.P., 2022. Modeling global carbon costs of plant nitrogen and phosphorus acquisition. J. Adv. Model. Earth Syst. e2022MS003204. <https://doi.org/10.1029/2022MS003204>*

Response 8

780 Thank you for your comment. We are not sure what would be the basis for the comparison with the reference given. Braghieri et al. (2022) is focused on the plant cost for nutrient acquisition. Thereby, the study is centered on uptake. Figure 7 shows the vegetation CN ratio, vegetation nitrogen and soil nitrogen.

Comment 9

785 *Section 4: Limitations and applications of the terrestrial nutrient modules. I would add the role of mycorrhizae into NP acquisition. Please refer to: Braghiere, R.K., Fisher, J.B., Fisher, R.A., Shi, M., Steidinger, B.S., Sulman, B.N., Soudzilovskaia, N.A., Yang, X., Liang, J., Peay, K.G., Crowther, T.W., Phillips, R.P., 2021. Mycorrhizal Distributions Impact Global Patterns of Carbon and Nutrient Cycling. Geophys. Res. Lett. 48. <https://doi.org/10.1029/2021GL094514>. Shi, M., Fisher, J.B., Brzostek, E.R., Phillips, R.P., 2016. Carbon cost of plant nitrogen acquisition: global carbon cycle impact from an improved plant nitrogen cycle in the Community Land Model. Glob. Chang. Biol. 22, 1299–1314. <https://doi.org/10.1111/gcb.13131>*

790 Response 9

Thank you for the suggestion, the following was added in Section 4:

795 5. The model does not account for uptake constrains 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). Furtermore, we do not estimate nitrogen cost for phosphorus metabolization or viseversa.

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