

We would like to thank Dr. Zaehle for his constructive comments and suggestions. We respond in detail to Dr. Zaehle's comments below (our responses are given immediately below Dr. Zaehle's comments).

• Isn't the model spin-up procedure inconsistent with using data from a chronosequence? A model that simulates P dynamics explicitly can be run from bareground to the age of the soil in the chronosequence, and be expected to reproduce the observed P availabilities - rather than prescribing the observed concentrations of all pools. This would be a much stronger model test than prescribing the observed C:P ratios and turnover constants and evaluating the effect of these parameters on the simulated NPP - seems like a missed opportunity. Maybe a simulation over that time-period is infeasible for computational reasons, but then I would expect a discussion of the limitation of this test.

Response: We agree that a more strenuous test for the model would be running from parent material and simulating the long-term (4.1 million years) soil and ecosystem development along the chronosequence. However, it is not feasible to do that because of the immense amount of computational time required. In addition, realistic driver data is not available for the long geological time period.

The following statement has been added regarding the limitation of this test (Page 14, lines 401-406).

“One limitation for the simulations along the Hawaii chronosequence is that we prescribed the soil P pools using site measurements rather than running the model from parent material to simulate soil and ecosystem development. A simulation over the time period of 4 millions of years is infeasible for CLM as it requires unrealistic computation time. Realistic driver data is also not available for this long time period.”

• The authors calibrate the models for the N-limited and P-limited site, but only present data for the P-limited site in Figure 2. I think it would be important to show that CLM-CN captures the N-limited site and that using the P cycle does not degrade the simulation for that site.

Response: Results for the N-limited site have been added as Fig. 2a. It shows that CLM-CN simulated NPP is consistent with observations, as well as leaf C, root C, and litter C. CLM-CN overestimated C in wood and coarse root and underestimated soil organic C. CLM-CNP produced the same results as CLM-CN as P is in adequate supply at this young N limited site. The following statement has been added (Page 13, lines 370-373):

“Fig. 2a shows that at the N-limited site, CLM-CN simulated NPP is consistent with observations, as well as leaf C, root C, and litter C. CLM-CN overestimated C in wood and coarse root and underestimated soil organic C. CLM-CNP produced the same results as

CLM-CN as P is in adequate supply at this young N limited site.”

• **What strikes me with the analysis of the Hawaii chronosequence is that the authors prescribe the end-result somewhat by adjusting soil and vegetation parameters for the chronosequence study. Particularly, the lower foliar C:P at the P-limited site leads to increased P demand (and therefore potentially also P limitation), relative to the N-rich site. It would be important to separate the vegetation C:P effects from the soil P effects, which I think should govern the P limitation response. Most convincing, and easily done, would be a little parameter perturbation experiment (e.g. perturbing one parameter at a time, each of the adjusted parameters by _10%, or replacing it with respective value of the N limited site), that would demonstrate, to which extent the lower P availability rather than the larger P demand at the site leads to the good fit with the observed data, in terms of both, the NPP and the fertiliser site response.**

Response: We have run the simulation at the P-limited site using the leaf C:P ratio of the N-limited site and the simulated NPP is only slightly higher (about 3%). Considering that NPP was reduced by about half in CLM-CNP simulation, compared to CN simulation (Fig. 2b), we believe P limitation at this site is mainly caused by soil P availability rather than the larger P demand induced by the lower leaf C:P ratio.

• **In Figure 4a, next to the available P, it would be good to report the average temperatures, precipitation and N fixation for these sites, especially because of the BR-SA3 site ‘outlier’ and the issue with within canopy litter decomposition, alluded to in P144454 L19ff. It’s less convenient to have to go to the Tables and search for this information.**

Response: Since Table 2 has provided average temperature and precipitation, it would be repetitive to have it reported again in Figure 4. We do agree with the reviewer it is less convenient to have to search this information in Table 2, which has different sorting from Figure 4. In the revised manuscript, we have revised Table 2 to have the same sorting as Figure 4 so that it will be easier to get the site information needed.

• **In Figure 4b): If disturbance was really the cause for the higher N than P limitation at this site, why would the CN model, which also does not know about the disturbance simulate a larger NPP than the two adjacent sites roughly consistent with the data? Surely there must be something other than P availability that drives the larger NPP at this site.**

Response: CN model simulated a larger NPP at this site than the two adjacent sites, mainly because of higher incident solar radiation in the driver data for this site. We believe the simulated strong P limitation at this site is not realistic because of the lack of consideration of shift in nutrient limitation induced by disturbances. The statement has been revised (Page 14, lines 415-419):

“The notable exception is the Tapajos tropical evergreen forest BR-Sa3 site. CN model simulates a larger NPP at this site than the two adjacent sites, mainly because of higher incident solar radiation in the driver data. The introduction of P limitation at this site leads to worse rather than better model performance, suggesting that the simulated strong P limitation at this site is probably not realistic.”

• A paper on C-N-P dynamics, demonstrating that a P cycle representation is important to simulate tropical C dynamics, is a strange place to advocate the “N limitation is a surrogate for P limitation” hypothesis (P14455 L8ff). I also don’t follow the argument, particularly because, as the authors demonstrate in the previous and subsequent Sections, P and N limitations do have different characteristics. The fact that P and N limitation co-exist in tropical ecosystems at the present-day (in the model) does not imply that N limitation is a good surrogate for P limitation in the future, because the response of soil N and P to elevated CO₂ and increased N or P demand will be quite different (N losses might be reduced, N inputs increased, where as the P capital is constant; the residence time of the pools between N and P are different, etc.). I would simply drop this statement, but at least, the authors should clarify and discuss that there is a difference between limitation at present-day and responses to future climate changes, and that an apparent similarity between the extent of N limitation in the absence of P and P limitation in the presence of N today does not imply necessarily that the analogy holds for the future.

Response: Thanks for the suggestion. The statement has been dropped and the following statement has been added (Pages 15-16, lines 449-453):

“The present-day similarity between the extent of N limitation in CLM-CN and P limitation in CLM-CNP, however, does not imply that N limitation is a good surrogate for P limitation in the future. As discussed previously, N cycle and P cycle have different characteristics and their response to changes in atmospheric CO₂ and climate will also be different.”

• In the list of differences why the P cycle is different to the N cycle I have been missing a discussion of the different plant physiological importance of N and P, the fact that the C:N ratios are smaller than the C:P ratios (hence lesser plant demand in absolute terms), and the flexibility of the ratios appears to be larger.

Response: we have added the following statement as suggested (Page 4, lines 111-117).

“The physiology of P limitation and N limitation is also different. Nitrogen is a major constituent of photosynthesis enzymes (mainly Rubisco) and light capturing proteins of thylakoid (Evans, 1989). Phosphorus is required for various processes such as the production of ATP from ADP and the regeneration of RUBP (Geiger and Servaites,

1994;Campbell and Sage, 2006). Plant growth requires larger quantities of N than P so C: N ratios are smaller than C: P ratios in terrestrial ecosystems (McGroddy et al., 2004)."

Minor comments:

Repeatedly, the authors present the current prevailing hypotheses as facts. I recommend to be more careful in wording at the following places:

P14440 L 6: We believe that P is the most limiting nutrient in the Amazon, but as the authors demonstrate later, the evidence is quite limited. Please rephrase to "currently believed/hypothesised to be the most limiting nutrient."

Response: The statement has been revised as suggested (Page 1, line 12).

P14441 L15: Do we really know that the major uncertainty in C modelling in the tropics stems from the lack of P availability, and not different sensitivities to drought stress, temperature extremes etc.? If so, please provide an appropriate reference. I don't think that the difference between Thornton 2009 and Zaehle 2010 can be explained simply by P, because neither of the model does consider P, and if the "P is a surrogate for N" hypothesis were true, it would apply to both models, such that it would not explain the model difference.

Response: We acknowledge that there are many uncertainties in C modeling in tropical regions, including different model sensitivities to drought stress, increased temperature, CO₂ fertilization, disturbances, and nutrient availability. Another important problem for tropical C cycle modeling is the lack of consideration of phosphorus (P) and P limitation. We have deleted the statement "one of the major uncertainties....". We have added the following statement (Page 2, lines 50-55).

"Most of the uncertainties arose from differences in model sensitivities to drought stress, increased temperature, CO₂ fertilization, disturbances, and nutrient availability. Another important problem for the current generation of nutrient-enabled models, such as CLM-CN (Thornton et al., 2009;Thornton et al., 2007) and O-CN (Zaehle et al., 2010), is the lack of consideration of P cycle and P limitation."

P14441 L18: Same issue as in the abstract. Replace "is" by "is hypothesized", "there are good reason to assume" or such alike

Response: The statement has been revised as "Phosphorus has been considered as the most limiting nutrient" (Page 2, line 56).

P14442 L4ff: Neither Cleveland not Wang actually provide a proof that - globally - P availability constrains N fixation - especially, as plants possess the capacity to access

P if needed. I would suggest to remove this sentence, as the next sentence is much better evidence and sufficient to make the point.

Response: This sentence has been removed as suggested.

P14456 L16f: “Therefore this feedback would be of great importance”. This is pure speculation, and should be labelled as such. There is yet very limited evidence that this process would actually have a strong effect under elevated CO2.

Response: The statement has been revised as “this enzymatic feedback pathway could potentially be important for P availability...” (Page 17, lines 489-491).

Further comments:

P14442 L29: I would add that also the links between the P cycle and the C:N cycles aren't all that self-evident.

Response: The statement has been added (Page 4, lines 99-100).

P14443 L6: I would add that the long-time scales involved makes P for models that operate typically on time-scales of centuries or less, in which the P cycle does not equilibrate make the P cycle problem very much an initial value problem, which requiresthe use of good information to start the model. There happens to be recent paper by the first author that would be a good quote here.

Response: The statement has been added as suggested (Page 4, lines 107-111).

“The long-time scales involved make the P cycle problem very much an initial value problem for models that operate typically on time-scales of centuries or less, in which the P cycle does not equilibrate. Yang et al. (2013) provided global maps of various P pools that can be used for the initialization of these models.”

P14443L12: This is wrong. Both CASA-CNP and JSBACH-CNP can be run at 30min time-scales, e.g. Zhang et al. 2011

Response: The statement has been deleted.

Editorial issues:

P14440 L24: Why not C-N-P? I assume this is a typo? Justify otherwise why C-N is not important

Response: It has been revised now (Page 2, line 32).

P14442 L18: a word is missing: nitrogen availability, turnover, inputs ?

Response: It should be soil nitrogen availability. Text has been revised accordingly (Page 3, line 87).

P14442 L23: New paragraph begins with “despite”.

Response: It has been revised as suggested (Page 4, line 93).

P14445 L12: “Resolution of P limitation” seems strange to me. “Representation of P limitation”

Response: It has been revised as suggested (Page 6, line 178).

P14447 L5ff: I wonder whether it would make sense to separate some of these differential equation from the main text, and provide them as an Appendix, because they arenecessary for the specialist, but not maybe not required to get the general idea of the model - would help readability, but I’m split-minded myself about this.

Response: We would prefer to keep the equations in the text, as the equations are integral to the understanding of the model results.

P14452 14453: I would prefer to see Sections 3.1.1 and 3.2.1 as part of the Methods section. These texts answer some of the questions I had when reading the Methods.

Response: We have moved Sections 3.1.1 and 3.2.1 into the Methods section as 2.4.1 and 2.4.2.

P14454 L1: soil properties

Response: It has been revised (Page 13, line 365).

P14455 L6: “On the other hand”, requires “on the one hand”

Response: It has been deleted.

The reference list is incomplete wrt to the citations in the text, e.g. Zaehle et al. 2010 is missing.

Response: Zaehle et al. 2010 has been added in the reference list.

Table 2: It would be good to have a similar sorting than Figure 4. Check the temperaturevalue for BR-Sa3, the reported value is unlikely to be correct. Add unitsfor all parameters.

Response: Table 2 is now having the same sorting as Figure 4. The temperature for BR-Sa3 has been corrected. The unit has been added for all variables.

Table 3: There are 4 entries for soil C:P. Either redundant, or some labels are missing.

Response: They should be SOM1 C:P to SOM4 C:P. The table has been revised.

Figure 1: please clarify the relationship between solution and labile P

Solution P is assumed to be in equilibrium with labile P pool. The relationship between solution P and labile P can be described using the Langmuir equation in CLM-CNP. This has been discussed in detail in section 2.1 (Page 5-6, lines 150-153). We have added the following sentence in the figure caption "Solution P is assumed to be in equilibrium with labile P pool."

Figure 2 4 report fluxes in "a-1", whereas elsewhere "yr-1" is use - please be consistent.

Response: The fluxes are in "a-1" throughout the text.

Figure 7: please use the symbols used in the equations/Table 1 as Y-axis labels

Response: Since not all of the variables in Fig. 7 exist in Table 1 or equations (such as NPP, TOTVEGC, TOTSOMC), we prefer not use symbols as Y-axis. Instead we have added the symbols that exist in Table 1 in the figure caption so the readers can easily find the information.

Reference :

Campbell, C. D., and Sage, R. F.: Interactions between the effects of atmospheric CO₂ content and P nutrition on photosynthesis in white lupin (*Lupinus albus* L.), *Plant, cell & environment*, 29, 844-853, 2006.

Evans, J. R.: Photosynthesis and nitrogen relationships in leaves of C3 plants, *Oecologia*, 78, 9-19, 1989.

Geiger, D. R., and Servaites, J. C.: Diurnal regulation of photosynthetic carbon metabolism in C3 plants, *Annual review of plant biology*, 45, 235-256, 1994.

McGroddy, M. E., Daufresne, T., and Hedin, L. O.: Scaling of C: N: P stoichiometry in forests worldwide: implications of terrestrial Redfield-type ratios, *Ecology*, 85, 2390-2401, 2004.

Thornton, P., Doney, S., Lindsay, K., Moore, J., Mahowald, N., Randerson, J., Fung, I., Lamarque, J., Feddema, J., and Lee, Y.: Carbon-nitrogen

- interactions regulate climate-carbon cycle feedbacks: results from an atmosphere-ocean general circulation model, 2009.**
- Thornton, P. E., Lamarque, J.-F., Rosenbloom, N. A., and Mahowald, N. M.: Influence of carbon-nitrogen cycle coupling on land model response to CO₂ fertilization and climate variability, *Global Biogeochemical Cycles*, 21, GB4018, 10.1029/2006gb002868, 2007.**
- Yang, X., Post, W., Thornton, P., and Jain, A.: The distribution of soil phosphorus for global biogeochemical modeling, *Biogeosciences*, 10, 2525-2537, 2013.**
- Zaehle, S., Friend, A., Friedlingstein, P., Dentener, F., Peylin, P., and Schulz, M.: Carbon and nitrogen cycle dynamics in the O-CN land surface model: 2. Role of the nitrogen cycle in the historical terrestrial carbon balance, *Global Biogeochemical Cycles*, 24, 2010.**