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Interactive comment on "The role of phosphorus dynamics in tropical forests – a modeling study using CLM-CNP" *by* X. Yang et al.

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The manuscript by Yang et al. describes a new version of the CLM-CN model, which includes a representation of the phosphorus cycle. The authors evaluate the model against observations obtained from a chronosequence, and fertiliser study in Hawaii and a P-availability transect in the Amazon Rainforest. The model simulations are - in principle and given the paucity of available observational C-N-P studies - suitable tests of the adequate representation of phosphorus dynamics at the time-scale of years to decades in the CNP version of the model. The manuscript is well written, clearly structured and falls well into the scope of Biogeosciences.

That said, there a a number of points that deserve clarification, more detailed analysis, or discussion (in no particular order):

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- 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 bare-ground to the age of the soil in the chronosequence, and be expected to reproduce the observe 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.

- 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.

- 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 $\pm$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.

- 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

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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.

- 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 that 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.

- 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 subsequence 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.

- 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.

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."

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.

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

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.

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 CO_2.

Further comments:

P14442 L29: I would add that also the links between the P cycle and the C:N cycles

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aren't all that self-evident.

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 requires the 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.

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

Editorial issues:

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

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

P14442 L23: New paragraph begins with "despite".

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

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 are necessary 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.

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.

P14454 L1: soil properties

P14455 L6: "On the other hand", requires "on the one hand"

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

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is missing.

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

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

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

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

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

References:

Zhang Q, Wang YP, Pitman AJ, Dai YJ (2011) Limitations of nitrogen and phosphorous on the terrestrial carbon uptake in the 20th century. Geophysical Research Letters, 38, n/a–n/a.

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