Biogeosciences Discuss., 10, C5860–C5863, 2013
www.biogeosciences-discuss.net/10/C5860/2013/

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

Anonymous Referee #2

Received and published: 15 October 2013

The manuscript of Yang et al consists of a describtion of the implementation of the phosphorus (P) cycle into the CLM-CN model and a evaluation of the model using observational (1) data from two fertilizer experiments in Hawaii and and (2) five sites with differing P availability from the Amazon region. The test case are well suited for evaluation of a pP model, given the scarce data. The introduction of nutrient cycles into global carbon cycle models helps to improve the reliability of these models. Therefore the manuscript is certainly in the scope of Biogeosciences.

The overall presentation of results is well structured and clear. Nonetheless, there are a number of issues which need to be addressed.

Major comments:

1. Why didn't the authors use the information on soil age at the chronosequence sites for a spin up which is consistent with the age of the soils? Such a test would be a much better test for the calibration of the model than the presented test using equilibrium simulations.

2. The model is heavily calibrated and the parameter values differ strongly from site to site. From the current analysis it is unclear which processes and thus which parameters are responsible for the occurrence of phosphorus limitation. A parameter perturbation experiment would be very beneficial to gain a better understanding of the models dynamics.

3. As the author correctly state the P and N cycles are close linked, thus it would be very interesting to the see (1) the results from the N limited Hawaii sites in Fig. 2 and (2) the simulated NPP for the CN simulation in Fig.7a.

4. It is not clear to me why the model performance greatly improved by the introduction of the P cycle based on the results shown in Fig4b. Statistical measures would be beneficial in this case.

5. The author state that the P cycle can be substituted with the N cycle (P14455, L8). This view seems problematic to me, as the mechanisms underlying N and P limitation differ strongly. A substitution might not work when the temporal evolution of the NPP response to an increase in CO2 is simulated.

6. The model description and the rationale behind the calibration strategy needs clarification: - Missing is the description of P uptake and what assumption are used. - The scaling parameter of the biochemical mineralization affects the CP ratio of the soil. How is it parametrized? The factor controls, together with the parameters for "Soil C:P", the actual simulated CP ratio of SOM. There is data on soil CP ratio. Can you use it to evaluate the parametrization? - The C:P ratios of SOM is much higher at the Amazon site compared to the Hawaii sites. What is the reasoning for changing the the C:P ratios rather than k_BC? - The parametrization of the leaching flux is not

described. Together with the rate constant for conversion of secondary to occluded P, these rate constant control the amount of P in steady state. A description and perhaps a sensitivity analysis (see above) would improve the understanding of the model.

Minor comments:

P 14440, L6 : It's not a given fact the P is the most limiting nutrient, please rephrase.

P 14440, L 26: It is not clear to me which results presented in this study justify this conclusion.

P 14441-14442: Most of the cited studies are missing in the "references" section, like Zaehle, Clark, Foley, Melillo, ...

P 14441, L 16: Change "Zahle" to "Zaehle"

P 14441, L18: It's not a given fact the P is the most limiting nutrient, please rephrase.

P 144443, L 6: There is evidence that it is a Mo effect rather than a N effect (Baron et al., 2009). It should be mentioned that the N effect rather uncertain and other theories exist.

P14443, L 9: The parametrization of the Langmuir equation in CASA-CNP and in JSBACH are based on measurements using the Hedley fractionation (see Wang et al., 2010). Please explain the difference to your strategy. In particular, why using a soluble, labile and sorbed pools compared to a labile and a sorbed pool is an advantage.

P14443, L 12: A higher temporal resolution can be an improvement but it is not per se. Please explain

P14444, L 11: The sentence is misleading as the diagram summarize P in plant compartments in a single pool. It doesn't show the compartments ,leaves", "fine roots" ,etc.

P14444, L13: In this study representation of mineral P different from Wang et al, 2010 is

used: three pools (soluble, labile and secondary) which interchange P with each other compared to two pools in Wang et al., 2010 (labile and sorbed). Please explain your rationale to have these dynamics. I am a bit hesitate to accept that a more complex representation of inorganic P is an improvement when the process understanding is poor and data to parametrize such a model is scarce.

P 14452, L 7: I guess, the parameter was tuned in both simulations, CN and CNP. (If not, the comparison of the C stocks would be rather unfair.) Please clarify this in the text.

P 14466, Table 3: "Soil C:P" is four times in the table

P 14472, Figure 6: The presentation of the feedbacks is incomplete. There is an arrow missing from "Phosphatase activity" to "available P". Phosphatase activity was shown to be depend on P availability in the field. It is not clear to me why "P demand" and "P supply" are grouped together. When the grouping is removed all signs of feedbacks could be given: There is a positive feedback from "P supply" to NPP based on the assumption P is the most limiting nutrient in tropical forests. P demand has a negative feedback to "available P" based on the assumption vegetation satisfies its demand by uptake.

P 14456, L 25: It is not clear how the simulation were performed. Was the desorption rate / biochemical mineralization rate enhanced during the spinup or were the rates enhanced beginning with the CO2 increase?

Interactive comment on Biogeosciences Discuss., 10, 14439, 2013.