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Interactive comment on "Multiple soil nutrient competition between plants, microbes, and mineral surfaces: model development, parameterization, and example applications in several tropical forests" *by* Q. Zhu et al.

Q. Zhu et al.

qzhu@lbl.gov

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Response Letter

Title: Multiple soil nutrient competition between plants, microbes, and mineral surfaces: Model development, parameterization, and example applications in several tropical forests

General Response: We would like to thank the two anonymous referees and T. Wutzler for their constructive comments. Special concerns came from the two anonymous

reviewers about the "constant enzyme abundance assumption". In this revision, we modified our model so that plants are able to dynamically adjust their nutrient carrier enzyme abundance according to their fine root biomass. Sensitivity analysis, model calibration, and evaluations were completely re-done. Since the fertilization experiments we examined were short-term (24 or 48 hours), plants were not able to adjust their competitiveness and we therefore did not see large difference between the new and original models. However, allowing the plants to adjust their competitiveness did affect plant nutrient uptake over longer time periods (e.g., seasonal). The model modifications suggested by the reviewers give the model great potential to better represent nutrient competition among various nutrient consumers.

The response letter is organized by (1) reviewers' major comments in blue; (2) authors' response in black. Minor reviewer comments (e.g., typo) are not listed here. We have carefully checked the entire paper and incorporated those specific minor comments.

1. The paper presents an interesting ecosystem modeling study dealing with multiple elements and competition. This issue is timely and interesting to a broad community. I read the paper initially with great interest. The ECA formulation of resource uptake probably has large potential to construct adequate models in competition settings. However, by progressing to the results section, and frequently going back to the methods to understand the results, I became disappointed about the model performance and also doubting about the claims of the paper.

Response: Thanks for the general positive comments. We have modified our model based on your comments. Please see the following responses.

2. In the supplement, the authors write that "soil CNP stoichiometry is flexible and depends on the predicted immobilization rates". This assumption need to be defended very well. If I understand correctly, there are no stoichiometric constraints on SOM decomposition and only the product stoichiometry is adjusted due to the currently available nutrient uptake flux. However, decomposition is done by microbial biomass with

rather strict homeostatic constraints. From a model designed to study competition for nutrients, I would expect to deal with stoichiometric constraints and resulting changes in other processes such as decomposition with inhibition or overflow respiration. Maybe it was not well explained, as stoichiometry factors are referred to as subsets of the parameters on page 4071.

Response: The original N-COM version assumed that soil CNP stoichiometry is flexible within upper and lower bounds. Therefore, the model has some stoichiometry constraints on SOM decomposition.

For the revised N-COM model, we adopted the fixed CNP stoichiometry. We agree that fixed SOM stoichiometry could better deal with the soil CNP imbalance during decomposition if the nutrient immobilization could not satisfy the nutrient demand.

3. I am missing information how the nutrient immobilization flux F immob from appendix A is distributed to the changes of the different SOM pools F immob j (eq. 5 and 6). Why is there another subscript i in F immob ji ? From eq. A6-A8 I first got confused that immobilization fluxes do not depend on the inorganic pool. The amount of substrate, surprising to me, is presented as part of the relative competitiveness (eq. 13ff). Can this be presented better? Further, did I understand correctly that NH4 and NO3 are not in direct competition for satisfying the N demands?

Response: Sorry for the confusion. Fimmobij means the immobilization flux occurred when carbon flowed from upstream (ith pool) to down stream (jth pool).

The potential immobilization flux (Equation A6 – A9) is calculated as the total nutrient demand during SOM decomposition. The soil has to immobilize those nutrients to satisfy the soil CNP stoichiometry. However, the soil may not be able to get those nutrients due to the competition stress from other nutrient consumers. Therefore, the immobilization rates are not dependent on the inorganic pool directly, but are constrained by the inorganic pool size indirectly.

Equation 13 – 22 are improved in terms of formulation of competition. Further, we added Equation 23 – 28, 32 – 33 to facilitate the explanation of our competition equations.

We assumed that soil microbes have no preference in nutrient uptake (NH4+ versus NO3-). The microbes will take up both NH4+ and NO3- according to their availability in the soil (Equation A7-A8).

4. The assumption of the enzyme baseline seems rather strict. On the other hand, with fitting all the Km coefficients, the concentrations become rather arbitrary because they could cancel with the Km. What would be the consequences on the results by doubling one of the enzyme concentrations?

Response: We also realized that “constant enzyme abundance assumption” might be too strict. In the new model, for example, the plant nutrient carrier enzyme abundance is updated dynamically based on fine root biomass. That means that during the growing season, plant will produce more enzymes. At the same time, if microbial enzyme abundance does not change, plants will become more aggressive and take up more nutrients than predicted by the original N-COM model. Doubling the plant enzyme abundance will enhance plant competitiveness, but not exactly double, because of competition with other consumers.

5. The competition between microbes, plants, and mineral surfaces is probably very different in rhizosphere, litter layer and bulk soil, with depth, and also at smaller scales down to aggregates. The microbial properties (all the KMs) are probably very heterogeneous in space too. I am missing some critical discussion on this heterogeneity.

Response: To address this concern, we added text in the Methods describing that the model did not discriminate bulk soil, rhizosphere soil, or the litter layer. In order to be consistent with CLM4.5 model structure, the competition model is designed for the whole soil column (mixed environment of rhizosphere and bulk soil). We agree that heterogeneity is one of the important factors that controls competition. We had some discussion about soil heterogeneity in section 3.4. In the revised manuscript we added more detailed discussion in section 3.1.

6. I am missing the specification of the likelihood or cost function. Especially with several data streams there are several crucial choices to make. How was convergence of the limiting distribution checked?

Response: Apologies for the missing explanation. We have added an equation to describe the cost function in the revised manuscript (Eqn. 29).

The convergence of the model parameters was checked visually. We plotted out the evolution of model parameters during MCMC calibration in the Figure A1. Most of the model parameter converged, and have a Gaussian distribution (Figure A2).

7. Fig 2 is too small and the binning of the histogram is done in a way that does not allow many conclusions. All that I get is the impression that the MC calibration did not successfully converge to the limiting distribution and that the presented sample is far from assumed Gaussian.

Response: Figure 2 has been updated: (1) add more bins for posterior parameters and (2) add prior distribution for comparison. We have added new figures to show that most of model parameters were converged to a Gaussian distribution (Figure A1 and A2).

8. The presented way of inspecting uncertainty reduction is rather longwinded and errorprone. I would not trust the conclusions from first specifying priors by factors of one estimate (p4072,l14), then specifying a σprior, and then inferring a σposterior from fitting a normal distribution to the posterior samples presented in Fig.2. I suggest plotting the prior distribution of the range of relevant posterior together with a reasonable histogram and/or density line of the posterior.

Response: Thanks for the suggestion. In Figure 2, we plotted the prior and posterior distribution together for the purpose of comparison. The estimation of prior parameter

uncertainty was also improved. We first assumed that the model parameters could vary within [10% 500%] of their prior values. We then fit them to lognormal distribution, and infer theta_prior from the distribution's variances.

9. Since, the parameters are restricted to positive values and are constrained by 10% to 500%, it will be more reasonable to use a log-normal distribution as prior and fit to the posterior, or alternative do the calibration on log-transformed parameters. To me the resulting prior and posterior sigma would be more meaningful.

Response: We re-did the model calibration by assuming the prior model parameters were lognormally distributed between 10% and 500% of their prior values. We also updated the parameter uncertainty reduction based on the new prior and posterior parameters variance.

10. The authors claim (p 4084), that with more temporally resolved observations the model could be constrained better. From Fig. 4, however, I get the impression that the model structure was not able to already fit the given observations (although the observation uncertainties necessary for evaluation are not presented).

Response: The model structure has been improved with: (1) fixed SOM CNP stoichiometry and (2) flexible abundance of plants' nutrient carrier enzyme. The calibration was also improved by adopting a lognormal distribution for prior parameters. We showed that our posterior model could reasonably reproduce the observed tropical ecosystem C/N/P dynamics, which imply the efficacy of our model calibration and the accuracy of our model structure.

Interactive comment on Biogeosciences Discuss., 12, 4057, 2015.