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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

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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; (2) authors’ response. Minor reviewer comments (e.g., typo) are not listed here. We have carefully checked the entire paper and incorporated those specific minor comments.

1. SOM modelling as it is done in most ecosystem models with first-order decay kinetics has been criticised for being too simplistic with a strict artificial division of available N between microbes, plant uptake, de/nitrification and other losses. Zhu et al. address this issue by introducing a soil nutrient competition model N-COM. At a first glance the model seems very interesting and comprehensive, but the way it has been tested and calibrated in the manuscript leaves many questions unanswered.

Response: Thanks for your positive comments. According to your comments, we improved our manuscript accordingly. Please see our responses below and revised manuscript for more details.

2. In the abstract the authors state that their results imply a certain competitiveness order for NH_4 , NO_3 , and PO_x . I would argue that this order comes mainly from calibrating the model to data than as an independent modelling result.

Response: You are correct that the reported competitiveness is a modeling result. The important distinction for our results and paper is that competitiveness is defined so that

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it can be calibrated based on observational data, rather than being imposed as a fixed value or order (as is done by most large scale models). We believe this feature is one of the promising aspects of the N-COM model. For this study, the calibrated “competitiveness” is for tropical ecosystem only. The relative “competitiveness” will change due to changes in e.g., plant root architecture, mycorrhizal fungi association, decomposing microbial community, soil pH, and soil order. N-COM facilitates representing the impacts of these factors on nutrient consumers’ dynamic competitiveness. We added more discussion about this issue in section 3.1.

3. It has been shown (FACE MIP project; Medlyn et al. 2015 and series of articles) that ecosystem models differ in their representation of many different processes with contrasting responses to perturbations to the system. So to say that nutrient competition is critical for the ESMs is a little too strong, as I think many other process brought to light in Medlyn et al. (2015) will have larger impacts.

Response: Model-data inter-comparison at FACE sites (FACE MIP) is a valuable project resulting in several interesting publications (Walker 2014; Zaehle et al., 2014; Medlyn et al., 2015). These papers showed that no tested ecosystem model was able to reproduce observed carbon and nitrogen dynamics at ORNL and DUKE FACE sites. Their results implied that new theory and modeling methods were needed, especially for modeling how ecosystems respond to elevated CO₂ conditions.

We acknowledge that many other mechanisms besides nutrient competition are important for ESM performance. Therefore, we modified our introduction, by saying that “nutrient competition is critical for ESMs to simulate ecosystem responses to nutrient perturbation”.

4. The model at a first glance looks interesting (Eqn 13-21, A6-14 and B8), but the decision to keep the enzyme abundance of all consumers at a constant value (4068 L14) makes me disappointed. So by not activating a flexible [E] I think this study is missing a lot, which the manuscript is mentioning when stating that robust competition

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representation for climate-scale models will require representation of dynamic changes in plant allocation (4063 L3).

Response: To address this concern, we modified the assumption of “constant enzyme abundance assumption”. In the revised manuscript, we assumed that plant nutrient carrier enzyme abundance is proportional to fine root biomass. During wintertime, fine root biomass declines because the production rate is lower than mortality. In contrast, during the growing season, a large portion of NPP is allocated to fine root production in tropical forests, enhancing plant nutrient competitiveness. In this way, plants are able to adjust their nutrient competitiveness through the imbalance of fine root carbon allocation and mortality. Enabling the changes of enzyme abundance gives the model potential to better represent soil nutrient dynamics and carbon-nutrient interactions.

We modified our model and re-did the model calibration and evaluation again. We found that although the new model was theoretically more robust, the posterior model was similar to previous versions in terms of simulating nutrient competition in the short-term (24 or 48 hours) fertilization experiments. Plant competitiveness changes could not be so rapid. Therefore the advantage of “dynamic enzyme abundance assumption” was not realized for these short simulations. However, as mentioned above, these changes do result in dynamic relative competitiveness over seasonal to annual time frames

5. VMAX is mentioned throughout the paper, but isn't the constant to calibrate k in eqn B8, if flexible $[E]$ is to be used? Will the calibrated values of VMAX and K_M be valid under flexible $[E]$?

Response: If $[E]$ is flexible, then VMAX must be treated as the product of two independent variables. One is the reaction rate for one unit enzyme (k), the other is the enzyme abundance $[E]$. In the revised N-COM model, $[E]$ is scaled by fine root biomass. Therefore, we only need to calibrate the enzyme base reaction rate (k).

6. The model was spun up for 100 years for the Puerto Rico simulation (4074 L19). I

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would like some more explanations on the spin up procedure (SOM build up? N/P up-take? etc) and the need for it as this is the only place it is mentioned in the manuscript.

Response: Sorry for the confusion. We took soil organic matter stock (carbon) from a CLM4.5 restart file (which is in steady state). Other state variables were obtained from literature for tropical forest sites, but not particularly for Puerto Rico or Hawaii tropical forests (e.g., free phosphate was from Panama site (Wright et al., 2011), organic nitrogen and phosphorus were inferred based on SOM CNP stoichiometry (Parton et al., 1988), adsorbed phosphorus and parent material phosphorus were from Yang et al., 2014).

In order to eliminate the impact of our imposed initial conditions on the fertilization experiments, we ran the model from the initialization state for 100-years and then applied the fertilizer into the soil. In the revised manuscript, we summarized this model spin-up procedure.

7. The change of coarse woody debris turnover time by 50% (4075 L18) is significant and its consequences to the C, N and P cycles/stocks should be covered.

Response: That's a good idea. For the revised version of N-COM, the posterior model had longer woody debris turnover time but shorter litter turnover time. The whole soil organic carbon decay was not significantly changed, however. In the revised manuscript, we added more discussion about calibrated model parameters (compared with their prior) and their impact on the system C, N, and P cycles (particularly for those parameters closely associated with nutrient dynamics).

8. Figure 5 shows the result of the Hawaiian chronosequence experiment. It looks like modelled microbes is taking up close to nothing at all three sites and mineral all available P. Could this figure become clearer or is it that mineral is so competitively strong that they get 100% in the model?

Response: First of all, the minerals are very competitive, and they can quickly establish

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equilibrium between adsorbed and free phosphate. Therefore, the large adsorption flux would limit microbial P immobilization.

Secondly, the response of microbial P immobilization to P fertilization is also limited by underestimation of microbial P demand. For our CENTURY type decomposition model, stoichiometric differences between soil organic matter and microbes are not considered. The observed difference between microbial and soil C/P ratios can be as large as 6-fold (Mooshammer et al., 2014; Xu et al., 2013). Were that the case in the observations we applied, the potential soil P demand calculated based on microbial C/P ratio should be 6 times higher than that based on soil organic matter C/P ratio. We have added some discussion on this issue in section 3.4.

9. For NH₄ at the Puerto Rico site, plant and microbe is opposite strong at taking up NH₄. Could this be adjusted if [E] became flexible?

Response: You are right, if [E] becomes flexible, the tree may adjust its competitiveness when the environmental conditions are favorable. Since the fertilization experiment was conducted during growing season we expect to see that plants have higher competitiveness than during the non-growing season.

10. I also miss how the model would affect plant growth, heterotrophic respiration, N fluxes compared to a normal first-order decay kinetics model. Have this test been done? If so could a section describe the differences in their behaviour? Or even add it to Figure 5 for comparison.

Response: At this point, we have used the model as a diagnostic tool. Many variables were taken from CLM4.5 simulations, as described in Methods. Our ongoing work is to integrate the competition scheme in CLM4.5, so that we can run the full model to predict how nutrient competition affects plant growth, soil respiration, etc.

The aims of this study are to (1) point out the importance of nutrient competition, (2) propose a feasible model structure for competition modeling, and (3) test the concepts

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against fertilization experiments. More detailed analyses at the whole ecosystem level are ongoing and will be reported in future publications.

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