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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.

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The paper presents an interesting ecosystem modelling 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.

Both, the description of the model, and the description of the calibration need further explanation and justification.

## 1 model

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.

I am missing information how the nutrient immobilization flux $F^{immob}$ from appendix A is distributed to the changes of the different SOM pools $F_j^{immob}$ (eq. 5 and 6). Why is there another subscript $i$ in $F_{ji}^{immob}$?

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?

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?

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.

## 2 calibration

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?

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.

The presented way of inspecting uncertainty reduction is rather longwinded and error-prone. I would not trust the conclusions from first specifying priors by factors of one estimate (p4072,l14), then specifying a $\sigma_{prior}$, and then inferring a $\sigma_{posterior}$ 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.

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.



Because the authors did not convince me to trust the results of the model calibration, I am also reluctant to accept the applications to tropical forest sites and the conclusions on relative competiveness.

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).

## 3 Specific comments

To my opinion the introduction is quite verbose and could be shortened.

When stating the objectives p4063, (2) seem to be a means of achieving (1), rather than an objective.

Eq. 1ff: Notation of d and Delta are quite confusing. I suggest calling the deltas decomposition or mineralization flux with own symbols.

P4066L18, the “respectively” is ambiguous.

Fig. 4: Please, indicate the uncertainty of the data.

Fig. 5 For people not familiar with the measurement magnitudes it is hard to interpret the figures. I suggest plotting the relative changes to the control.

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