

Overall, I am still very enthusiastic about the N-COM model and its competitiveness framework between plants and different microbial groups. Unfortunately, there still some issues with the Bayesian calibration of the N-COM model:

1. In lines 350-351 of the revised manuscript, you state you could “ensure thorough convergence” of your three MCMCs. To prove convergence you are referring to the trace plots in figure A1, but they rather prove that a lot of your parameters have not converged and the chains are generally not well mixed. In my first review I also recommended to use the Gelman convergence criterion. With your additional two chains you can now calculate the Gelman convergence criterion for every parameter.
2. Contrary to the statement in the main manuscript, you acknowledged in the reply to the reviewers that some parameters do not show convergence: “We noticed that the some of the parameters were not well constrained, which was reflected in their relatively large posterior uncertainty and small uncertainty reduction. We argued that the non-convergence resulted from data paucity rather than a short MCMC chain.” Here, you are confusing convergence of MCMCs with how well a parameter is constrained by the data. Paucity of data should not hamper convergence. If a parameter is not well constrained by data, the posterior should follow the prior. However, the MCMC algorithm should be efficient enough to be able to sample parameter as prescribed by the prior.
3. As a way forward, I could recommend to either try adaptive MCMC algorithms or to just use the best parameter set obtained with the current setting. For the second option, you would acknowledge that you did not manage to retrieve a proper posterior sample. Rather you would state that the calibration was explorative to study competitiveness effects between plants and microbes and how the model fares for a specific site.

When reading through the manuscript and the appendix again, I stumbled over equation 9 and equation A12. It was not entirely clear to me how equation 9 was solved:

- $\frac{d[PO_x]}{dt}$ appears on both sides of equation 9 of the manuscript. On the left hand side directly as $\frac{d[PO_x]}{dt}$ and on the right hand side somewhat hidden in the term F_p^{surf} .

According to equation 28 F_p^{surf} is defined as the potential rate times a rate-limiting term:

$$F_p^{surf} = F_p^{surf,pot} \cdot RateLimitingTerm$$

$F_p^{surf,pot}$, however is defined as (equation A12):

$$F_p^{surf,pot} = \frac{VMAX_p^{surf} \cdot KM_p^{surf}}{(KM_p^{surf} + [PO_x])^2} \cdot \frac{d[PO_x]}{dt}$$

Collecting terms, we have something like that (omitting other sources and sinks, F_{others})

$$\frac{d[PO_x]}{dt} = F_{others} + \frac{VMAX_p^{surf} \cdot KM_p^{surf}}{(KM_p^{surf} + [PO_x])^2} \cdot \frac{d[PO_x]}{dt} \cdot RateLimitingTerm$$

How is this equation solved?

Minor issues

In line 286-287, you state that “Mineral surface “effective enzyme” abundance (E_p^{surf}) is approximated by $VMAX_p^{surf} - [SP]$.” I would appreciate if you could motivate this equation with one or two sentences.