Dear Reviewer,

Thank you for the constructive comments. We copied your comments below in blue font; our responses are in ordinary black font. We have labeled our responses for ease in cross-referencing. Responses to your comments are labeled "RC2". Cross-references to our responses to Reviewer 1 are labeled with "RC1" and cross-references to our responses to a community reviewer are labeled with "CC1".

Shuyue Li et al., conducted a modeling study to investigate how plant allocation in response to nutrient fertilization. They used nutrient enabled ED2 model with various different parameterizations on biomass allocation under control and fertilized conditions over tropical dry forest. Data from an fertilization experiment at Costa Rica forest was used for model comparison and validation. The paper is well organized and presentation is smooth. Below I have a few suggestions and comments.

(Response RC2-1): Thank you for your generally positive review and many great suggestions and comments. Please see our responses to each of them below.

1. introduction, first paragraph needs to be improved, Nutrient availability could affect plant activity in many different ways. The most relevant (to this paper) way is through mediation C/N/P allocation and biomass construction. However, the first paragraph try to explain how nutrient availability could affect plant response to CO2 enrichment, which is not much relevant here.

(Response RC2-2): In the first paragraph of the manuscript, we wanted to provide the overall context for this study: more process-level understanding of nutrient limitation is essential for reliable prediction on the primary production of terrestrial ecosystem under future environmental change. Yet we think that this is a fair critique. We will plan to revise this first paragraph to be briefer on the general context and to better introduce the idea that C/N/P stoichiometry can be critical for biomass construction and allocation.

2. introduction, paragraph 2 and 3 provide a nice summary of many fertilization experiments for tropical trees. However, each fertilization experiment was discussed individually. I would suggest adding some discussion about why and how experiment results differ from one another to improve the coherence of the summary.

(Response RC2-3): In Paragraph 2 and 3, we mentioned existing nutrient fertilization experiments and discussed them individually mainly because of little effect was found on stand level but large variability appeared across studies. But we also agree that adding a few more sentences of synthesis would improve the coherence of the discussion. We would be happy to follow the reviewer's suggestion in a revision.

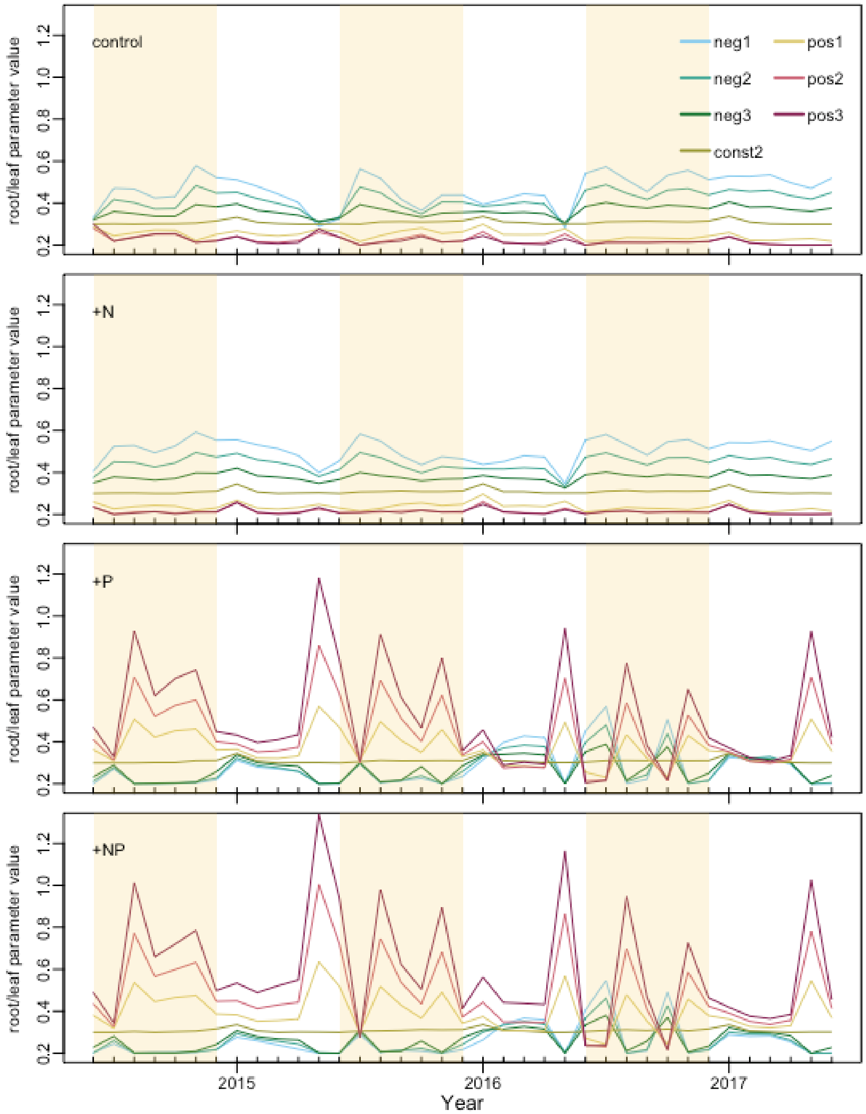
3. introduction, paragraph 5 and 6 highlight the need to investigate and improve the allocation scheme under long-term fertilization for current generation CNP models. In this case, a survey of allocation schemes used by current generation models are necessary, for example some models assume constant allocation, some assume multiple

resource coordination, some are based on carbon cost …Besides the seven CNP models mentioned in this section, two more recent global CNP models are:FUN-CNP: Braghiere, R.K., Fisher, J.B., Allen, K., Brzostek, E., Shi, M., Yang, X., Ricciuto, D.M., Fisher, R.A., Zhu, Q. and Phillips, R.P., 2022. Modeling global carbon costs of plant nitrogen and phosphorus acquisition. Journal of Advances in modeling earth systems, 14(8), 2022MS003204. ELM-CNP: Zhu, Q., Riley, W.J., Tang, J., Collier, N., Hoffman, F.M., Yang, X. and Bisht, G., 2019. Representing nitrogen, phosphorus, and carbon interactions in the E3SM land model: Development and global benchmarking. Journal of Advances in Modeling Earth Systems, 11(7), 2238-2258.

(Response RC2-4): We agree with this suggestion. We can provide further discussion on the allocation schemes of current generation CNP models in Section 4.4. We can also add an overview of various allocation schemes in Introduction as suggested. We appreciate the reviewer for bringing up these other CNP models. ELM-CNP (Zhu et al. 2019) applies a resources-dependent allocation scheme developed by Friedlingstein et al. (1999), analogous to our "neg" parameterizations; Braghiere et al. (2022) integrated the most recent version FUN3.0 with ELM, modulating plant nutrient uptake from multiple pathways by optimizing carbon cost, but did not illustrate how this strategy might affect new-growth allocation; moreover, neither of them discussed the effects of nutrient availability on relative allocation to leaves, wood and fine roots. We are happy to include the discussion on additional CNP models in a revision.

4. section 2.3, r2l is a function of soil P concentration (psol), I wonder mathematically will this equation lead to huge variability of r2l parameters especially at the time when fertilizers were applied. Maybe showing a figure of r2l during the 3 years of fertilization experiment will help to clarify this.

(Response RC2-5): The variability of the r2l parameter depends on treatment and parameterization. Without P fertilization, there is some seasonal variability in r2l, but it is relatively small. The variability is largest under P fertilization with the "pos" parameterizations, where r2l ranges mostly from 0.4 to 1.0. Despite this variability, the "pos" parameterization consistently leads to larger r2l under P fertilization than the "const" or "neg" parameterizations. We can include such a figure (see below) and some discussion in a revision.



5. section 2.4.1. Vegetation and soil are both initialized with in situ observations, rather than being determined by long-term spinup. Such approach often time will result in an dis-equilibrate vegetation and soil processes. Therefore, after initialization the vegetation and soil states will quickly changes towards quasi-equilibrate conditions, which could be largely different from the initialized conditions. I wonder if the re-equilibration also occur in ED2, how long does it re-equilibrate, and how that affect fertilization results?

(Response RC2-6): This question is difficult to answer because our study system is a 30-year secondary forest and the actual forest is not equilibrated yet. Because the actual forest is still unequilibrated, we did not intend to spin up the model to equilibrium. The two year spinup was sufficient to initialize soil water (see also in Response RC1-10). Despite the short spinup, the model has been shown to simulate aboveground biomass reasonably well (Medvigy et al. 2019) and no obvious transients in leaf area index have been observed (Xu et al. 2016). The plant-available soil nutrient pools (Figure 2 in the original manuscript) also do not exhibit much of a transient. The slowly varying soil pools were initialized from observations and were not expected to change much over the course of this study because they are slowly varying.

6. section 2.4.1. It was mentioned that fine root production was evaluated with linear regression, however, it also mentioned linear regression was not appropriate because there existed only three years of data. Here needs more clarification.

(Response RC2-7): Thank you for pointing this out. Reviewer 1 came up with a similar concern. Please see our detailed responses in Response RC1-9. In short, we reanalyzed our simulation results by first averaging over years, and then performing t-test for leaf, wood, and fine root productivity by treatment, to determine whether there were significant differences between simulations and observations. By doing so we no longer need linear regression to evaluate model performance on fine root production. We will use this fuller version of analysis in revision.

7. Figure 2, most of the simulated variability of NO3, NH4 already exist in control run (solid blue lines), it doesn't look like there were sudden increase of NH4 or NO3 right after the N fertilization. Also, it will be helpful, if the fertilization date could be marked on the x-axis.

(Response RC2-8): We agree with this comment, as mentioned in original manuscript (Line 235). This is likely because leaching is an important pathway for N loss, especially during wet seasons when fertilizers were applied. We are happy to make modifications on Figure 2 to display approximate fertilization time.

8. Section 4.1. It's still not clear to me which parametrization is the best. It was stated that "only one of the 13 parameterizations that we tested was able to simultaneously simulate leaf, wood and fine root (missing word) production consistent with the observations". Here, the screensful parameterization needs to be highlight. Also, in Figure 4, it doesn't look like any parameterization was significantly superior to others.

(Response RC2-9): It was not our intention to identify a single "best" parameter set. Rather, we wanted to find what, if any, range of parameter values would lead to simulations that are consistent with the observations. We approached the analysis being open to the possibilities that many, none, or one parameter set would be consistent with observations. In our original analysis, we now think that our analysis was weakened by the fact that we considered only seven parameter sets. To better address the problem that you mentioned as well as comments from other reviewers, we carried out more simulations on more densely sampled parameter space and changed analysis method to compare the differences between simulations and observations. Therefore, Figure 4 will not be in our revised manuscript. More details are presented in Response RC1-9.

Having now carried out these additional simulations, we think it is an interesting result that the only parameterizations that were consistent with observed leaf, wood, and fine root productivity all had a positive relationship between relative allocation to fine roots and soil P concentration. Such a parameterization had not been applied in previous models. We do not yet have the data to know if these "pos" parameterizations will be better than "neg" or "const" parameterizations on timescales longer than three years. Thus, we think that this result motivates further field and modeling work (see also in Response CC1-12).

References

Braghiere, R.K., Fisher, J.B., Allen, K., Brzostek, E., Shi, M., Yang, X., Ricciuto, D.M., Fisher, R.A., Zhu, Q. and Phillips, R.P., 2022. Modeling global carbon costs of plant nitrogen and phosphorus acquisition. Journal of Advances in modeling earth systems, 14(8), 2022MS003204.

Friedlingstein, P., Joel, G., Field, C. B. and Fung, I. Y., 1999. Toward an allocation scheme for global terrestrial carbon models, Global Change Biol., 5, 755-770.

Medvigy, D., Wang, G., Zhu, Q., Riley, W.J., Trierweiler, A.M., Waring, B.G., Xu, X. and Powers, J.S., 2019. Observed variation in soil properties can drive large variation in modelled forest functioning and composition during tropical forest secondary succession. *New Phytologist*, *223*(4), pp.1820-1833.

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