

Biogeosciences Discuss., author comment AC1
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Reply on RC1

Lin Yu et al.

Author comment on "Improved representation of phosphorus exchange on soil mineral surfaces reduces estimates of phosphorus limitation in temperate forest ecosystems" by Lin Yu et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-114-AC1>, 2022

Reply to "Comment on bg-2022-114 RC1"

RC1: In this study, Lin et al presented a double surface Langmuir adsorption isotherm in the QUINCY model and compared it to the traditional/simplified (single surface) Langmuir isotherm, that is mostly used in current TBMs. After model calibration, the authors argue the double isotherm shows a better representation of the inorganic P cycling. The improved P (ad)sorption model also suggests the current assumed P limitation in temperate forests was likely overestimated. Overall, the paper is well written, and the results are also sound. Given our current understanding of P limitation is still very limited, thus before the P models can be applied to make sound predictions, the model structure needs to be evaluated and discussed. Study like this paper thus contributes to improving the process description of P exchange in forest ecosystems and advances in C-P coupling in TBMs. I would thus recommend publishing this work at Biogeosciences. However, I have the following comments for the authors to consider during their revisions. First, the authors did a literature survey to highlight uncertainties of the current parameterization of Langmuir isotherm in some TBMs. The parameter error in some previous TBMs was also noticed by myself when I develop a recent ecosystem CNP model.

Authors: we thank the referee for acknowledging our work.

RC1: Thus I think it is important to highlight this for the community and a very good motivation for the current study. However, one would ask if this is just a parameterization issue or if it is a model structure issue (as the authors argued here)? Empirical data that fit different isotherm functions, including traditional Langmuir isotherm, generally show various but reasonably well-fit results (e.g. Brenner et al 2019, Lin et al. 2020, and much more). Thus, I would like to discuss this with the authors. First, which part of the improved model fits the measured data that could be attributed to the model structure, which part is from improved parameters? I have concerns about how the model comparison is made and how much conclusions can be drawn from such a comparison? In Line 138, the authors state separate calibrations were made for each site and each depth, what's the influence of those separate calibrations for the comparison?

Authors: we would argue the different simulation results, especially the simulated soil inorganic P pools (Fig. 3), are solely caused by the model

structure rather than parameter values. We did not quantify the effects of model structure versus parameterizations, but in one of the sensitivities test we did, it is very clear that the simulated pattern of labile P to exchangeable P ratios (Fig. S7) is rather consistent within the same model structure regardless of the parameterizations. As for the calibration mentioned in Line 138, we only calibrated against the SOM stoichiometry to minimize the impact from organic cycling processes and did not calibrate the double Langmuir parameters (which are based on soil texture, pH, and exchangeable Al/Fe of each study site), thus we believe the different results we showed are caused by model structure rather than parameterizations.

RC1: Second, as the authors argue the advantage of using double surface Langmuir, i.e. its higher buffer capacity. Then I would suggest a better separation of the influence of P release from other releases and uptakes? i.e. the feedback is of need. From the results, the main improvement is the ratio between Plab and exchangeable Pi (section 3.2). The P uptake across models seems rather similar (Fig. S2), i.e. for the P limited site LUE, the uptake PO₄ for siLang, dbLang and 4pool model (Fig. 2Sf). The siLang shows a higher uptake in autumn but at an annual scale, the overall rates seem rather similar. The different approaches show a rather large influence on the C partitioning, (LAI, aboveground C, Fig. S2 bc). This is rather strange, what causes such large feedback on the aboveground plant properties, given the total P uptake seems rather similar? I also do not find evidence to support the statements of Line 203, i.e. the differing plant P uptake.

Authors: the plant P uptake in QUINCY is controlled by multiple factors, including soluble P concentration, root biomass, and plant P demand. Moreover, the root biomass is also heavily influenced by productivity and the plant nutrition status. We totally agree that it is not straight forward to directly draw any conclusions from the plant P uptake comparison, as the referee pointed out in Fig. S2, the rather similar annual P uptake could be either due to a combination of high root biomass and low soluble P (siLang, VES), or low root biomass and high soluble P (control, LUE). In general, high root biomass lead to higher uptake in non-growing season but meanwhile probably lead to lower uptake in growing season as the high root biomass indicates strong nutrient deficiency, i.e. very low soluble P concentration in growing season. We will clarify it more in the revision.

As for Line 203, we should refer to Fig. 4 rather than Figs. S2-5. We thank the referee for pointing this out and will change it in the revision.

RC1: Third, the model performance of foliar P, Fig.4b shows a convergence of different models when P availability becomes smaller. In other words, in more P-limited conditions, the difference between the models becomes smaller, although all of them largely underestimated the measured P concentration. How come such large differences in the Prich sites? Is this due to the calibration being mainly focused on the soil and thus less on the vegetation?

Authors: The main reason for the small difference of foliar P at low P sites by different model variants, as we have already touched upon a bit in the discussion (Sect. 4.4), is that the main mechanism to alleviate the simulated P stress in the low P site should be the organic P cycling rather than the inorganic P cycling. With this said, the double Langmuir isotherm we presented here would help to release some of the unrealistic P stress in conventional TBM, but without an advanced organic P cycling scheme implemented, it is still not possible to

reproduce the observed pattern in foliar P along this P gradient.

RC1: Some more specifics to consider:

Introduction

Line 36, missing references after "boreal forests are generally considered N limited"

Authors: *thanks, we will include it in the revision*

Line 49-50, the argument is that organic P recycling is the major flux, while the geochemical P flux is small.

Authors: *thanks, we will revise it*

Line 50-55. In literature, several isotherms, or model functions, including double Langmuir, have been suggested to describe the phosphorus adsorption-desorption processes (i.e., McGechan and Lewis, 2002). I would also suggest not to use "a novel model concept, Line 54" as the authors propose in its current form. It has been in the literature for some time. I think the novelty is the implementation of the TBM models and evaluation of the implications? Besides, I am also lacking the field and experimental evidence to support the additional supplement of P from the adsorbed P pools. So, what is the, i.e. P isotopic data suggest, and do they support your hypothesis here? What are the mechanisms behind that? I would suggest adding those to the motivate current model development work.

Authors: *thanks. Yes, the isotherm itself is not novel but the implementation in TBM is. We will clarify it in the revision. The main experimental evidence to support this isotherm is the P isotopic studies, and our scheme is also based on their conceptual model (Line 266-270).*

RC1: Methods

Line 70, equ 1, the Langmuir isotherm, do the interaction with water considered? As the concentration also dependent on the water content at each time step?

Authors: *yes, the concentration is water dependent.*

Line 116 do you have leaf P/N concentration data over years? Or just sampled for one year?

Authors: *we have only multiple year data for one site, not for all the sites*

Line 138 what is calibrated and what criteria were used for the calibration? Be specific here.

Authors: *See replies above. we will clarify it in the revision.*

RC1: Results

Line 203 given the total P uptake by different approaches?

Authors: yes

Line 246 the pool sizes differs also after the simulation, SOM top soil, the fluxes and the pools sizes. As also your sensitivity results show the importance of SOM pools for dbLang, Line 219, which indicates the potential feedback due to the biological mineralization. Also in your Line 243 on the plant and soil changes So the different approaches show impacts on the fluxes and pool sizes. Why not show a complete P budget for each site with different fluxes simulated by various approaches? Also show the different pool sizes before the simulation and after the simulation, i.e. the pool size changes. This will give an overall picture of the ecosystems.

Authors: yes, we do see an effect on SOM size and fluxes in topsoil at LUE site, but the difference for other sites are not so strong. Fig. 7 shows the temporal change (10 years) of main P pools of different models after different fertilization experiments, we believe it is a more intuitive display than a P budget for a certain period. In this figure, each point in the x axis represents a P budget at a specific time, with the size change of all P pools equal to zero (C fertilization) or the added P (P and CP fertilization). The change of the P pool also infers the changes in P fluxes, e.g. plant P pool change indicates plant P uptake change, labile P pool change indicates P adsorption change and etc.

Some references mentioned:

McGechan and Lewis, 2002 presented an excellent review of the principles, equations, and models for the sorption of phosphorus. *Biosystems Engineering* 82 (1), 1-24.

Brenner, Julia, et al. 2019, Phosphorus sorption on tropical soils with relevance to Earth system model needs, *Soil Research*, 57, 17-27.

Lin Yang, et al 2020, Anoxic conditions maintained high phosphorus sorption in humid tropical forest soils, *Biogeosciences*, 17, 89-101.

Authors: thanks, we will include them in the revision