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Geoscientific Model Development Discussions
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Interactive comment on "CoupModel (v6.0): an ecosystem model for coupled phosphorus, nitrogen and carbon dynamics – evaluated against empirical data from a climatic and fertility gradient in Sweden" *by* Hongxing He et al.

Anonymous Referee #1

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The paper by He et al. presented the integration of the phosphorus (P) cycle into the CoupModel and evaluation of the new model, Coup-CNP, against four regions in Sweden that differ in climate and fertility. It is overall a very interesting paper, particularly with the novel setup of both NP cycles and mycorrhiza. The results are well presented, and the discussion is clear and well organized; the authors have put much effort into compiling the information of the model development both in the paper and appendix. Overall I think the paper is in good shape and contributes to advances in the modeling community.

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Discussion paper

However, the current quality of the paper needs to be improved before final acceptance. First of all, I found the quality of English writing an obstacle for me to keep focusing on the scientific content of the paper. I would recommend the authors to go for a professional editing service with the paper. I listed some obvious mistakes in the detailed comments, mostly before the results section, since I stopped to do that for the rest of the paper simply due to the heavy load of scientific information. Secondly, this study lacks a proper sensitivity analysis. The authors did a simple sensitivity test on the fungal organic uptake rates of N and P and presented the result in the appendix. As far as I see from the description in this paper, Coup-CNP is a heavily parameterized model with a huge number of parameters. It is extremely important to run a proper sensitivity analysis with multiple parameters, not only to see the effects of parameterization on model outputs but also to test the stability and robustness of the model. Thirdly, apart from a sensitivity test, I would also recommend the authors to conduct a few model experiments to see the model responses to alternative model assumptions or changing climatic/environmental conditions. For example, the authors introduced the plant growth response to P stress based on leaf C/P ratio (Eq.9), which is novel and interesting but at the same time debatable. I personally would really like to see the effect of this mechanism on the predicted GPP/NPP and biomass. Another example is the role of mycorrhiza uptake and the so-called organic uptake of N and P. I found that the authors made some very strong assumptions regarding the uptake competition (the sequence of uptake) between plant, fungal and adsorption, and it would be interesting (and fundamental) to see the effect of these strong assumptions.

Detailed comments

1. Abstract and Introduction

Line 18: make “which explicitly consider mycorrhizal interactions” a relative clause after “The extended Coup-CNP”

Line 26: what is “a steady state in P availability”? I don’t find “P availability” from the P budget

Line 40: “nutrient cycling” is not a biochemical reaction

Line 50: it is true that N inputs to the atmosphere increased due to human activity, but for terrestrial ecosystems, the important process is the N deposited from the atmosphere

Line 52: mechanisms can not be amplified, right?

Second paragraph: I think it is a brilliant idea to review the literature of the P cycle in current models, but the organization of information needs to be much improved in this paragraph. I also have some disagreements with the authors about the interpretations of some cited publications, and would like to discuss with the authors about them.

Line 56-65: I think this part is irrelevant to the overall discussion and conclusion of this study. I would recommend to remove or to shorten it.

Line 72: there are some more P-enabled ESMs, e.g. Zhu et al. 2016 Biogeosciences, Goll et al. 2017 GMD, Thum et al. 2019 GMD.

Line 75: Zaehle et al. 2014 does not support your statement here

Line 76-92: The interpretation of these studies is a bit imprecise and vague. I found it difficult to jump from one study to another one; maybe it is better to reorganize all the studies with some intrinsic links, such as common problems or findings. What I will recommend is to focus on the role and effect of plant P uptake in different model studies.

Yu et al. only included the P cycle into the ForSAFE model. I would not phrase it as "developed the model", which causes confusion

Line 99: whose interaction with soil mycorrhizal fungi?

Line 100: I don’t fully agree with the interpretation of the references here. These data-

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driven meta-analyses do not really explain "how P availability affects plant growth", and if this mechanism is influenced by mycorrhizae-plant interactions. They are more of "a proof" than "an explanation" to me

Line 109-111: please restructure the sentence

Line 132: soil organic matter is a more commonly used term than "soil organics"

Line 134: there is little evidence for organic P uptake of plants and microbes, as far as I know

2. Model structure and description of processes linked to the phosphorus pool

Please rename the title, maybe “Model structure and phosphorus process description”? Another piece of advice is linking the process description of Section 2.2 with the equation number in Section 3 and Appendix A. It is much easier for the readers to track information in this way.

Line 142: what does "flexible" model mean?

Line 145: please check the grammar

Line 147: maybe already mention the normal time step and the smaller time step here?

Line 149: "crucial"??? what and why?

Line 151: why the radiation forcing has to be “global”???

Line 153: compete for light??? Not "light interception"?

Line 161: strange sentence structure, please consider adjusting it

Line 164: “can differ” => differs, or do you mean that there are two options for time step???

Line 166: difficult to understand the sentence

Line 171: there is not a common definition of "mineral P", please distinguish it from other inorganic P forms

Line 174: “inorganic-phosphorus”, why a hyphen here?

Line 176-180: I don’t see the connection between the model definition and Hedley fractionation. Please elaborate.

Line 183: “which contains”=> “for”

Line 185: which decomposition rate is used for the combined litter pool?

Line 185-200: If I understand correctly, Coup-CNP applied a three-pool structure for soil inorganic P, which is different from most other P models. One thing that is particularly different in this study is that the role of adsorption/desorption is greatly neglected by most biochemical processes since Pisol is only relevant to transport and Pilab is relevant for other processes, such as deposition, weathering, plant/fungal uptake and etc.. It is a very interesting setup, but I think it needs to be better explained.

Particularly, the statement that “These Pi ions are normally loosely adsorbed to surfaces and can thus easily re-enter the Pilab pool through the desorption process (McGechan and Lewis, 2002).” is wrong. There is plenty of evidence for the strong adsorption of phosphate, which is also the main reason for the extremely low soluble inorganic P concentration in the soil water. The main reason that plant and microbe can take up enough P in such a low P concentration is probably the fast replenishing of soluble P in soil water, which are the consequences of desorption/diffusion and biological mobilization (mineralization). Please see Buenemann et al. 2016, SBB and Pistocchi et al. 2018, SBB, and the references therein for more information.

Line 214: what is mobile P and N? this is a very strong assumption that plants can capture nutrients from litterfall, and I wonder how sensitive are the model outputs to this assumption.

Line 221: what are the enzymatic processes? Please be specific. Btw, phosphatase is not a process

Line 222-225: well, this is another astonishing assumption, which needs to be properly tested. And the hidden hypothesis that it only occurs after inorganic P uptake when plant P demand is not fully met is also quite strong from my personal feeling. It basically means that there are no interactions (feedback/competition) between soil organic and inorganic P cycling processes, all the feedback mechanisms have to go through the plant growth&litterfall pathway. I wonder how the model will perform in an extremely P limited ecosystem.

Line 229: how is the DOM redistributed between layers? Is it described in the paper?

3. Equations describing key phosphorus processes/fluxes and their parameterization

One major trouble to me is that the use of both uppercase and lowercase P (p) in the equations. It is extremely difficult sometimes, please consider replacing one of them with another letter. Another major issue is that I could not find information on how leaf P content is calculated, which is essential to understand some results

Line 243: judging from Eq.4, I don't think "proportional" is the right word here

Line 247: how does erosion affect weathering rate? I cannot find it in the paper

Line 254: there is a potential problem that diffusion is also considered as weathering. how uncertain is it to assume diffusion and weathering has the same temperature response? This is even a bigger problem for pH response as there is no evidence that pH affects diffusion

Line 295: I am not sure if this theory is applicable to leaf CP ratio since P is not as essential as N for photosynthesis and the role of leaf P in photosynthesis is not well understood yet. As I mentioned before, it will be interesting to conduct model experiments to test this theory. Additionally, I did not find the information on how Coup-CNP calculates leaf P content.

Line 303: The mycorrhiza module??? This sentence is confusing to me

Line 314: Eq.9 seems the only place that soluble P concentration is used except leaching, how realistic is it to take this assumption directly from N, given the fact that P concentration is much lower than N?

Line 316: “wais” => was

Line 317: the piavail is another very problematic assumption, and I cannot find any theory or evidence to support it. Since the soluble P concentration is not used to calculate the plant P uptake, I could foresee that if labile P is freely taken up by the plant, the model might end up with no P limitation and the labile P might get depleted very soon. If there is no theory or literature to support this parameter, at least it should be tested in the sensitivity analysis

Line 405: where is f(Piavail) used? which equation?

4. Description of the region used for simulation and model setup

It seems the same study regions have been tested with the previous version of Coup-Model before, and it is unclear from this section if the new Coup-CNP model is recalibrated in this study. Please state it clearly in the paper how the model is parametrized and why some parameter values differ from previous studies (I assume that is the case)

Line 420: kg N ha-1 yr-1, right?

Line 423: please cite the most recent FAO standard

Line 443-446: difficult to follow the sentence

Line 449-450: The model was spun up for 10 years, and then a clear cut is simulated??? How do you determine the initial SOM content and soil stoichiometry? How big are the effects of initial SOM status?

Line 450-452: difficult to follow. Unclear to me what are the plant components and how are they treated

Line 470: “chronicle”? I am not sure that is the right word here???

Line 477: This is a very unrealistic assumption; please see Yu et al. 2020 GMD

Line 485-487: One specific question to Table 2 is that, why the decomposition and uptake rates for different latitudes are different, given that the temperature response function already accounts for the difference in temperature? If they are calibrated separately, what is the meaning for choosing a climatic gradient???

Table 3: I would recommend running a full sensitivity test with parameters in this table

Line 519-521: difficult to follow the sentence

5. Results

Line 561: confusing, please rephrase

Line 564: why the new Coup-CNP C sequestration rates are so different from previous studies of the same regions?

Line 573-575: I only see that the P leaching is very small, which may infer that it has a small effect. But the fact that P leaching accounts for 30% of P deposition does not lead to the conclusion that “a small effect on the system”. I guess the key point here is that both P deposition and P loss are very small compared to other fluxes, e.g. plant P uptake

Section 5.2: the rotation period, 10 years to 10 years after the final felling, makes it a bit difficult to understand the results in figure 4, particularly the plant growth and change in plant in panel A. For me it is very difficult to judge how much of the changes in plant and soil pools are due to the very short spin-up time (10 years)? Is it possible to run a real spin-up to ensure a more stable state of the soil pool? Also, I did not fully understand why the pool size of 10-year-old trees differ so much in N and P size, to me it seems to be the effect of model initialization and spin-up.

6. Discussion

Section 6.1: all the studies that are compared to in the section are modeling studies, which should be made very clear.

Section 6.2:

In general, the discussion is interesting and the findings are encouraging. However, I do have an understanding problem regarding the soil N/P ratio. From the description in the method part, the soil N/P ratio seems to be a parameter in the sensitivity analysis. But its value is not reported in Table 3, and it seems that it is also not a constant value from Figure 3d.

A more methodological problem is, only three parameters were tested in the sensitivity analysis, and the result for one parameter was presented. How could one conclude that this one is the most important parameter for the ecosystem? As I mentioned before, if this is the first study of the Coup-CNP, a better-designed sensitivity test should be performed. I am very convinced by the authors that soil N/P is an important indicator of Swedish forests, but I am convinced by the way it was accidentally chosen in this study.

Line 676: where does this conclusion come from? increasingly P limited with time or latitude, or another gradient?

Line 682: have you checked if the threshold is the same for pine and spruce? if not, please be specific about tree species

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