Final response to interactive discussion

Dear Referees, Dear Editor,

We would like to thank you very much for your positive comments and constructive suggestions to our manuscript "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"

In this document, we provide responses to each comment of the two referees; the referee comments are given in normal font, and our responses in italics.

One important change in the revised version is that we will additionally evaluate the sensitivities of all the newly introduced Coup-CNP parameters by a Monte Carlo sensitivity analysis method. The covariance between parameters and equifinality for the selected model outputs will be presented (compared to three key parameters sensitivity test in current version). The sensitivity analysis enables us to address the concerns raised by referee#1 about the global sensitivity and assumptions, and also the model behavior issue raised by referee#2.

Further, we will improve the introduction and the presentation of model description by merging the process description of Section 2.2 with the equation number in Section 3 and linking to equations numbers in Appendix A as suggested by referees.

Below, we state how we will address each specific comment when revising our manuscript. After revising, we will send the manuscript for language checking by professional English language proofing company. Hence, some formulations below may change.

We hope that our response together with the revision of the manuscript sufficiently addresses the referees' concerns.

Sincerely,

Hongxing He (on behalf of the author team)

Reply to Anonymous Referee #1

We thank Anonymous Referee #1 for your positive comments and constructive suggestions to our manuscript. Here are our responses to the comments. The referee comments are given in normal font and our response in italics.

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.

- *We are glad the referee appreciates the science and novelty value of this new model development. We appraise that our P cycle and mycorrhiza model has a good potential to contribute to improved understanding of and insights into current ongoing discussion of nutrients impacts on C cycle. We also pleased that the referee finds the presentation of the results and discussions are in a good shape.*

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.

- *This time we will send the manuscript for language checking by professional English language proofing company after revision is completed. The language of the manuscript had previously been edited by professional editing service, but that was not the final, submitted version.*

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.

- *We agree a more comprehensive sensitivity test including all the P parameters and few N related parameter could be a good addition. We will conduct a more thoroughly sensitivity test of all the new introduced parameters by a Monte Carlo based sensitivity analysis of all the newly introduced P parameters in the revised paper.*

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.

- *We agree. These are interesting implications of our new model. Our aim is to be able to test state-of-art theory on the importance of fungal and plant uptake with our model. In the revision, we will better backup assumptions with literature and carry out a sensitivity analysis including parameters relevant for the assumptions, e.g., C/P optimal (leaf) $p_{cp,opt}$, C/P threshold(leaf) $p_{cp,th}$ (Table 3) and fungi uptake P competition parameters, e.g. potential unit fungal mycelia uptake rate PO4, $p_{i,rate}$ (Table 3). We will further discuss how these parameters link to the modelled outcome, e.g. plant biomass, predicated GPP/NPP.*
  Please note the changing climatic/environmental model experiments were already conducted by applying the new Coup-CNP model on four regions with varying climate (annual temperature between 0.7-7.1 C) and fertility (with the soil C/N and C/P ratios between 19.8-31.5 and 425-633, respectively) along a gradient from South to North Sweden.

Detailed comments

1. Abstract and Introduction

Line18: make "which explicitly consider mycorrhizal interactions" a relative clause after "The extended Coup-CNP"

- *We will revise this accordingly.*

Line 26: what is "a steady state in P availability"? I don't find "P availability" from the P budget

- *We will revise the texts to make it clear. A steady state in P availability means the outflow of P fluxes including plant uptake, leaching losses, harvest etc is more or less balanced by the inflow of P fluxes including weathering, deposition and mineralization.*

Line 40: "nutrient cycling" is not a biochemical reaction

- *We agree that nutrient cycling is more than a biochemical reaction. We will rephrase the sentence by replacing 'biochemical reactions' with 'processes' and 'nutrient cycling' with 'decomposition of soil organic matter and nutrient uptake'.*

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

- *Agreed. We will revise to "N deposition 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.

- *Agreed. We will remove "mechanisms" in the revision. We will shorten the literature P model review to make it more focused and thoroughly recheck the cited publications to ensure they are cited correctly. If the reviewer has one or some particular in mind besides the ones mentioned below, we would appreciate to know which.*

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.

- *Agreed. We will shorten this part.*

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.

- *Thanks for the information. We will add these into the revised paper*

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

- *Agreed. Zaehle et al 2014 discussed mainly N not P.*

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

- *Thanks for the great suggestion. We will update the organization of the literature P model and compare the COUP-CNP approach with those of existing models. We will also reword the ForSAFE model statement.*

Line 99: whose interaction with soil mycorrhizal fungi?

- *Interaction between plants and mycorrhizal fungi, we will rephrase this sentence to make it clear*

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

- *Agreed. We use this evidence here to highlight the importance of fungi for P availability, thus motivate the fungi model development. We will rephrase to make it more clear*

Line 109-111: please restructure the sentence

- *We will restructure the sentence*

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

- *Agreed.*

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

- *First, we would like to point out that the organic uptake defined in Coup-CNP, differs that of direct uptake of organic P molecules, see the definition of "organic uptake" Line 220-225. The model assumes plant roots and symbiotic fungi bypass the labile P_{ilab} pool, and obtain mineralized P_i directly from the organic matter P_{olit} and P_{ohum} pools. Essentially, the organic uptake in the model is a short cut for plant uptake that bypass the microbe mineralization process.*
  However, we agree with the referee that the evidence for direct uptake of organic P molecule by plants and microbes are few, the most uptake forms remains to be P_i. There are some empirical studies (e.g. Jayachandran et al. 1992; Fox et al. 2010) show mycorrhizal fungi may be able to acquire P from organic sources that are not available directly to the plant (e.g. phytic acid and nucleic acids). Also see, Lindahl et al. (2002) and Johnson and Gehring (2007). Such potential uptake is also included in our organic uptake concepts.
  Our model application also showed that organic P uptake of plants are needed to sustain the soil C/P ratios and plant growth demand, particularly for P poor region see Table 4. The modeling study by Orwin et al 2011 conclude the same.

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.

- *We agree with the referee. We will merge part of the section 2 and 3 in the revision to make it easier to follow.*

Line 142: what does "flexible" model mean?

- *CoupModel has a number of modules that can be activated by choice of the model user. We will remove "flexible" to avoid misunderstanding*

Line 145: please check the grammar

- *We will reword the sentence.*

Line147: maybe already mention the normal time step and the smaller time step here?

- *Agreed.*

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

- *Here is a general description of the model, when calculate e.g. event like snow melting peaks which is important to have a short time resolution to water flow* *estimation to avoid numerical error and water imbalance. We will remove this to avoid confusion.*

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

- *Global Radiation is used according to accepted meaning as the sum of both diffuse and direct incoming shortwave radiation. We may reword this to "short wave incoming radiation". This is a CoupModel convention, that the global radiation includes both direct and indirect radiation.*

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

- *Yes, we will revise it in the revision.*

Line 161: strange sentence structure, please consider adjusting it

- *We will rephrase the sentence.*

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

- *In CoupModel, it is possible to choose one-time step for water & heat, but another for C, N &P. We will delete this sentence to avoid confusion since the same time step was used in this study.*

Line 166: difficult to understand the sentence

- *We will reword to make it more clear*

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

- *We realize the definition of mineral might be unclear. We therefore will re-describe this state variable and rename it to distinguish to the other inorganic forms. In the model, soil inorganic phosphorus can be divided into labile inorganic P (Pi, phosphate ions, e.g., H3PO4, H2PO4-, HPO42-, PO43-) and* ***soil solid mineral****, (Pm). Soluble (Pisol) are a part of labile (Pilab) that are dissolved and not adsorbed.* ***Soil solid mineral Pm*** *is a lumped state variable containing primary and secondary solid P compounds.*

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

- *We will remove the hyphen in the revision*

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

- *The defined inorganic phosphorus forms in the model was partly adopted from the Hedley fraction, e.g. by using different extraction method for soluble and labile P. We will delete the Hedley fraction sentence in the revised paper to avoid confusion.*

Line 183: "which contains"=> "for"

- *Agreed.*

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

- *The litter decomposition rate coefficient that integrated the readily and more resistant litter was used. The same was used as in Svensson et al. 2008 and He et al. 2018 calibration paper and all CoupModel publications before 2000, when only one litter state variable existed. The litter decomposition coefficient was obtained through the litter bag incubation studies at those or nearby Swedish forests, more details see Table 5, Svensson et al. 2008. We will add this information in the revised paper.*

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.

- *We thank the referee for pointing out the potential importance of strong adsorption of phosphate in regulating the Pi availability. However, we have already assumed a strong and instantaneous response by the split between labile and soluble form of mineral P. Our current model does not allow formation of secondary solid minerals. We understand that Coup-CNP use fewer inorganic pools compared to some of the other models (e.g. Wang et al. 2010). Such models have separated primary, secondary mineral P (i.e. sorbed and strongly sorbed P pool in Wang et al paper), also occluded P, thus further introduce fluxes exchange between these pools. Theoretically these might represent a more physical realistic picture of the inorganic P dynamics, and we are aware this could lead to some over-simplifications (see our response to the weathering calculation)*
- *However, in reality very few data exist for the state variable size and the flux rates at the ecosystem level and hence in Coup-CNP, ' soil solid mineral Pm is a lumped pool containing primary and secondary mineral P (and occluded P) (line 173*-174*),'and one net weathering flux were used. In addition, we would also like to point out that the fast replenishing of soluble P was the case in current model structure since we assume the instant equilibrium of the total soluble P and the labile P in the soil water.*

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.

- *The plant mobile N and P are state variables designed to mimic the nutrient reallocation or retranslocation process, a process by which nutrients (here N and P) were mobilized from senescing structure to developing tissues e.g. before litterfall, thus acting as an important mechanism to reduce dependence on nutrient uptake and increase nutrient recycle. Aerts, (1996) showed mean nutrient resorption efficiency for perennial plant was c.a. 50% for N and slightly higher, 52% for P. A more recent global synthesis of most measured data for woody plants showed similar results that mean N resorption efficiency of 48.4% and mean P resorption efficiency was 53.3% (Yan et al. 2018). Nieminen and Helmisaari (1996) show a 67-88% mobile nutrient N and P decreasing during needle senescence for Scots pine in Finland. These evidence serves the rational of including the nutrient resorption process in the model. In Coup-CNP, P was a similar concept to that of N, this process simulates the resorption of nutrients before litterfall (N and P were assumed to be stored internally in the mobile state variable and these can then be used next year to develop new shoots/leaves). We will describe this more in detail and add this into sensitivity test to evaluate how sensitive the model outs to this.*

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

- *For example, phosphatase released by the root exudates. We will reword and make it more specific*

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.

- *We thank the referee for the comments on P uptake. We realize the description may be unclear thus we will revise this to make it clearer. The soil organic and inorganic P cycling processes are highly interacted in current model structure, besides through the plant growth & litterfall pathway, other the feedback mechanisms include , e.g. the decomposition of soil organic pools resulting in phosphate formation (L815-835 equ A2), the soil microbes regulates the mobilization or immobilization of the inorganic P from organic P (L815-835, equ A2); The growth of microbes and the plant organic uptake also compete for inorganic P; the availability and adsorption also regulates the competition for plant P uptake (L265-277, equ5).*

  In our model, the P uptake is driven by the demand of the plants but regulated by the availability in the soil. The uptake priority is the mineral P_i then the "organic P". For the mineral P_i, the availability is regulated by the availability fraction parameter, p_{iavail}. The availability of organic uptake is similarly regulated by a coefficient called *nutrient shortcut uptake rate (equ A4, Table 2, called fungi organic uptake coefficient in current version). The organic uptake concept was initially suggested by Beier & Eckersten,1998; Gärdenäs et al. 2003, (which was refined in) Svensson et al. 2008. We do not account for any organic uptake providing that the demand was fully met by the mineral P_i uptake. However, we can reduce the efficiency of uptake by reducing the parameter value for p_{iavail}. Our results also indicate the nutrient shortcut uptake coefficient needs to be higher in the P limited ecosystem. These are also why the nutrient shortcut uptake rates are selected as sensitivity analysis. The sensitivity results shown in Fig C1 and table B1, B2, B3 shows the organic uptake clearly impact the plant growth, the P leaching, thus shows that the inorganic uptake is strongly linked to the organic uptake. But there are no direct interaction from organic uptake mechanism to the mineral uptake in Coup-CNP.*

The north 61 ^{o}N region was a P limited forest ecosystem, the model showed a reasonable agreement with tree biomass although slightly underestimation of growth (probably caused by the overestimation of the leaf C/P, C/N ratios), but one of the noticeable results are the need of higher organic uptake to fulfill the plant demand when P was highly limited (e.g. Table 4).

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

- *The redistribution is done following that of water flow, as the DOM is assumed to have full mobility with water. The formation of DOM is from litter and humus, but DOM can also be fixation back to humus, see equations (A.7), parameterization following Svensson et al., 2008. We will add description and link to the equation (A.7) into the revision.*

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

- *We plan to revise the symbols of the equations where could lead to possible confusion and double check consistency,. wW will also add explanation texts in the main texts to explain the rule of the symbol to make it easier to follow. In general, capital P represents a pool or amount (state variables), small p represents a parameter.*
- *The leaf P content (mostly using C/P ratio as its indicator in the manuscript), leaf C/P ratio were calculated as the ratio between the leaf C (g C m-2) and leaf P (g P m-2), where leaf C and P were calculated separately, and updated for each time step. For each time step, the model calculate the leaf C include the C influx: photosynthesis allocation to leaf, C outflux: leaf respiration, leaf litterfall, etc. For leaf P, the model calculates the P influx: total P uptake allocate to leaf, P outflux: P litterfall, P to internal mobile P, etc. The model updates the leaf C:P ratio at each time step and used to estimate the photosynthesis for next time step. Note that the uptake of P is driven first by the demand of P and second by the availability of P. The demand of P*

is driven by the C but the availability of P is independent of C. We will add and/or clarify the description in the revised paper.

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

- *We mean here the flux is proportional to the pH response where the response itself is another equation. We will reword this in the revision.*

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

- *The erosion cause transport of particles that contains P. However, we only simulate a loss of particles (See appendix A12, A13, A14). We will improve the references to appendix by adding equations number in the revised paper to make it clearer.*

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

- *First, the weathering in Coup-CNP is independent of the mobile part of P in our model structure. The diffusion/desorption flux was implicitly included into the weathering flux (L196-198). Thus, how the diffusion/desorption flux response to temperature and pH is not explicit considered. However, again our aim was to build a simple yet realistic P net weathering flux. We compared to the current net weathering flux to a more detailed and rigorous geochemical model PROFILE, but not a dynamical model; that is more widely used for weathering estimates and current P flux estimates were rather similar (L635-645). We will revise the weathering part in our model conceptual presentation and add remarks in the equations description to make it clear about our assumptions.*

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 CoupCNP calculates leaf P content.

- *We will include the leaf CP ratio parameters in the Monte Carlo sensitivity analysis to evaluate the C/P ratio impact on the growth of the plants, See response above for the leaf P content calculation.*

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

- *We remove the sentence "P fungi processes analog to N processes (He et al., 2018) are found in appendix A" as the fungi P processes are described in main text. Hopes that answer the reviewer question as we are not completely sure we got it right.*

Line314: 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?

- *We will revise the wording here to clarify, it should be analogue instead of the same. Equ 9 is a response function that account the decreasing of plant C allocation to fungi when inorganic P are high. Mathematically it ranges from 1 to a threshold value. What we mean here is an analogue equation formula is used to describe P as for N but with different coefficients and drivers. The P dependency is defined by the soluble P concentration and the reduction parameter, p_{avai} in equ (9).*
  A number of studies show higher fungal production under more P-limiting conditions, e.g. under future eCO_2 (Bahr et al., 2015; Ekblad et al., 1995; Nylund and Wallander, 1992), Increasing soil P_i concentrations is also shown to reduce the plant carbon allocation to fungi (Bahr et al., 2015; Gower and Vitousek, 1989). This is the rationale behind this reduction function.
- *In addition, we will include this parameter for the sensitivity analysis*

Line 316: "wais" => was

- *Agreed.*

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

- *We will include this parameter in the sensitivity analysis. The conceptual meaning of p_{iavail} is that only a fraction defined by this parameter that could be available for plant uptake at the time step of calculation. Please also see our response above on the plant uptake concepts and rationale.*

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

- *see Equ (A8), we will add this in the revised paper*

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

It seems the same study regions have been tested with the previous version of CoupModel 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)

- *These regions were previous tested and used in a number of CoupModel studies (Svensson et al 2008; He et al 2018), In Svensson et al. 2008 study parameters were subjectively calibrated with the Coup-CN only model to the regional biomass data. He et al. 2018 employed a formal Bayesian calibration to the four regions with Coup-CN but with newly developed fungi model. Thus most C-N related parameters were previously calibrated, the newly introduced P parameters mostly were derived from literature if not then a subjective calibration were made to fit the observed data, in the revision we will make a Monte Carlo sensitivity analysis for all the newly introduced P parameters Few parameters values were different from previous studies, mainly due to an updated simulation design (L438-446). e.g. The rotational period in previous* *studies were 100 years for all the region, in current setup, a different rotational period was designed. We revised this since we consider this was closer to the real forest management practice in Sweden.*

- *In addition, by adding the P cycle also some bugs were revealed, for example how interception of light by trees affect light interception by understorey, resulting into adjustments of parameter settings.*

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

- *Agreed.*

Line 423: please cite the most recent FAO standard

- *We will revise to the more recent FAO 2006 guideline*

Line 443-446: difficult to follow the sentence

- *We will rephrase the description text to make it clear and easy to follow*

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?

- *Concerning spin up (L 432-434), the first 10 years after 1st clear-cut were used as spin-up period for the plants. From year 10 simulation results were saved until 10 years after 2st clear-cut. This was done to ensure to cover the potential nutrient leaching during the regeneration phase as in Gärdenäs et al. (2003).*
- *The initial SOM content and soil stoichiometry was reported in the Table 1 and also tested in the sensitivity analysis of varying soil N/P ratio (Fig 5 and Fig. C1). The effects of the initial SOM status were determined by the inventory data. The effects of the initial SOM status were important for the results (Fig. 5), as seen in our sensitivity test results. We reported these effects in section 5.3 and discussed thoroughly in section 6.2. However, we will also further discuss the additional effects (if any) of the new sensitivity analysis in the revised paper. Here we will add the additional soil N and P data of the Swedish Forestry Agency. Please also see the response to the initial and boundary condition comment of the topical editor, also see response concerning spin up below.*

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

- *We will reword the text to make it clearer.*

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

- *Agreed. We will reword in the revision.*

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

- *Thanks for pointing out this unclear formulation. We assumed for the total soil organic matter (SOM) that 5% decomposed with litter decomposition rate and 95% according to humus decomposition rate. Total amount of SOM decreased exponential* *with soil depth. The litter were assumed to be distributed down to 0,5 m but the humus down to 1 m depth. This follows Svensson et al 2008 and He et al. 2018.*

- *We agree this is quite important as also shown in our results of soil N/P ratio sensitivity (Fig. 5), however due to the measured data only covers the O horizon, and no further data available in the soil N/P ratio in the deeper layer. However, we have conducted a survey in the literature value of N/P ratio data in Swedish forests and used the range to test the sensitivity, as the results presented in Fig 5. We will further discuss this with the new sensitivity analysis. Note we also get hold of some additional soil N and P content at the organic layer where leaf N and P content were measured and we will add that in the revision.*

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

- *We agree the referee with the full sensitivity test and a full Monte Carlo based sensitivity test will be added. We are glad the referee pointed out the different decomposition coefficient, which we had discussed these interesting different coefficients phenomena in different latitudes in a series of previous papers, e.g. Svensson et al., 2008 (Table 8, Fig 5 and Fig 6), later a formal Bayesian calibration was also conducted to constrain these decomposition parameters to the measured growth rate in He et al 2018 (e.g. Fig. 7), both results show a different coefficients were needed to obtain the measured growth data.*
  Mechanically, the different decomposition rate of humus was reflecting implicitly other drivers such as different microbial functional groups and/or the quality of the soil organic matter. Microbial responses to temperature increase in northern Sweden are known to be faster where growing season is shorter. In the previous study the impact of P and other elements on SOM quality was not explicitly taken into account. This is another reason why temperature response may differ. (Table 1), Note this issue has been discussed in great detail in Svensson et al. 2008, we will add those references in the revised manuscript.

Line 519-521: difficult to follow the sentence

- *We will revise these sentences to make it easier to follow*

4. Results

Line 561: confusing, please rephrase

- *We will revise and make it more clear*

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

- *We thank referee's comment on soil sequestration rate but we respectively disagree with the interpretation of these C sequestration rates. First, Previous model results* *did not consider P, and we had discussed our newly introduced P cycle has clear impacts on C sequestration rates (Fig 5b). Second, we also had different set up with previous settings (see response above), thus a different C sequestration rate could be expected, given the soil sequestration rate were a net result of a number of C fluxes. However, the general trends were the same where an increasing soil C sequestration rate moving towards the south and we consider these results were rather in according with previous results not differs.*

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

- *We agree and the intention was to compare to the outflow flux to the internal flow, we will revise this in the revised paper.*

Section 5.2: the rotation period,10 years to10 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.

- *The rotational period set up were intended to capture the high leaching period after the final felling, for the rationale and detail results see Gärdenäs et al 2003. The initial conditions for the plants were generally known and were set according the data for the plant seedlings thus were unlikely to influence our results. The soil pools (e.g. litter, humus) were previous calibrated and used for these four regions before, e.g. the soil pools were evaluated to reach a steady state also a steady soil C/N ratio over 100-year period in Svensson et al 2008. Therefore, we believe our results were not likely be much influenced by the spin-up of the plant and soil, however, we will report the initial and boundary conditions more in detail in our revision.*
  Besides, the use of a spin up to find a stable equilibrium for the soil initial values are avoided by purpose. The main reason is that we have no evidence for a long-term equilibrium of the organic pools in Swedish Forest Soils. The Swedish forest is strongly dependent of the recent history and we have substantial ongoing transitions in the organic pools. We would also like to note that our soil state variables and our simulated soil C/N ratio and N/P ratio was shown to the in a steady state (Fig 3), previous studies by Svensson et al. 2008 show a that the soil C/N ratio may be a possible indicator of the state of forest soils also when they are in a long term transition process
- *The difference of plant pool of N and P could be a number of reasons, 1) a higher nutrient availability after clear felling 2) the clear felling kept 5% of trees thus a higher biomass and thus N and P were expected.*

5. Discussion

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

- *We will revise the title of the section to make clear that are modelling studies compared.*

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.

- *We thank the referee for found the discussion interesting and convincing. We will revise the section with N/P ratio confusion and made it clearer, the initial soil N and P data were reported at Table 1. The varying soil N/P ratio were varied from 10 to 25 (L476-480)*
  The results for the other parameters of the sensitivity analysis will be added in the revision, please also see response above.

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

- *With decreasing latitude, we will revise this to make it more clear*

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

- *We will check the data again from previous publications of Swedish forests and be specific about the tree species in the revision.*

Reply to Anonymous Referee #2

We thank Anonymous Referee #2 for your positive comments and constructive suggestions to our manuscript. Beloware our responses to the comments; The referee comments in normal font and our response in italics.

The paper of He et al., brings us a model that couples P into an existing CN model. It is an interesting study with special focus on mycorrhizal fungi, which is important in P dynamics, but has yet to be adequately represented in current literature. My major concern though, is that the model is heavily parameterised with great details and many parameters, but the model performance is systematically biased. Figure 2 and Table 4 evaluated modelled tree biomass, leaf C:N, leaf C:P, leaf N:P and P leaching against measurements. First of all, for a model that covers many aspects of C, N, P dynamics, variables evaluated here are not adequate to show the model performance. Secondly, the model systematically overestimate leaf C:P (all sites) and leaf C:N (3 out of 4 sites), and underestimate P leaching (all sites). I am not convinced that the model does a good job in capturing the system. Additional work and data are needed to understand the model dynamics and thoroughly assess the model performance.

- *We are glad the referee appreciates the value of this new model development. The referee raised about concerns about the model performance. Our aim was to demonstrate model behavior and the implication of the newly added P in the model structure. We tested the model, to identify the implications of integrating the P cycle using published parameter settings for C, N and mycorrhiza. The intention was not to make a site specific detailed model calibration.*
  Moreover, possibilities toevaluateg the performance in more details are limited due to lack of P data currently available representing the four regions in a consistent way. The forest biomass was a direct result from the regional survey, thus represent the regional characteristic however; the leaf C/P ratio data are data from some few representative sites within the region and the measured P concentrations in the streams also include other source of P from the whole watershed. When evaluating the model performance, these should be bear in mind. So, the results are mostly to demonstrate current understanding of P cycle and interactions with N and C.
- *However, to elucidate in more detail we suggest to add a Monte Carlo based sensitivity analysis of all the newly introduced P parameters to systematically evaluate the parameter sensitivity. This will demonstrate also co-variance between parameters. Yet, we estimate possibilities to reduce the current parameter uncertainty by the few data available in the current paper are limited but have some hopes as during summer, we got hold of some additional data of P and N content in the organic layer at the sites where N and P content of leaves where measured.*
  We hope the sensitivity analysis further elucidate the model behavior and demonstrate the model ability of capturing the system response to the four regions.
  However, already the current test demonstrate that the model can capture the main features of the system behavior. First, the model P budgets have been detailed discussed in section 6.1 and compared to different previous modeling results and empirical data available, since we aims to present a first modelled P budget

complement to C and N budget, the modelled P budgets including the net weathering fluxes, the plant P uptake, harvest remove of P, soil C sequestration, soil C/N ratio, soil C/P ratio, etc were all thoroughly discussed (section 6.1). The conclusion from those comparison suggest the Coup-CNP model captured a reasonably P budget. Concerning the underestimation of P leaching, please bear in mind that the model simulated P leaching from the mineral soil. The measured stream P concentration contains also sources of P from the whole watershed but our model contains only the upstream.

In addition, I feel it is quite difficult to follow the model description. Sometimes there are logical issues related to terminology and the separation among system compartments (please see detailed comments below). Sometimes it is due to lack of critical information in P cycling in the main text, for example, P dynamics in vegetation (allocation, resorption etc.), through mineralization etc. It might be better to put part of the information in the appendix into the main text, or at least have some overall description of these processes in the main text and point to the appendix for detailed information. The goal is to give the reader a complete picture of P cycling the model tracks.

- *We will thoroughly revise the model descriptions (sections 2.2 and 3) and add key information of the vegetation representation and mineralization in the main text. We will reorganize the model description so that the linkage of P processes to C and N processes as well as link between equations to concepts, in main text and appendix are easy for a reader to follow*

The novel part of this model, from my perspective, is related to symbiotic mycorrhizal fungi. I did not find any observations to initialise, evaluate model performance or constrain model parameters related to this part. It is also not clear what is the advantage of incorporating detailed symbiotic mycorrhizal fungi, how it affects system dynamics, what are the novel model behaviours due to this part? I feel these questions are worth answering to persuade the reader that the model is advantageous and worth the great details..

- *We thank the referee for the commenting on the importance of the fungi module. This paper follows a series of publications of the importance of considering the various uptake pathways for nutrients, Näsholm et al. (1998, 2009) experimentally show boreal forests take up organic nitrogen. For modelling, previously Beier and Eckersten (1998) had shown the need of organic N uptake to sustain the forest and soil N status in Swedish forests. These was also later shown in the calibration study by He et al. (2018) that the include of mycorrhizal fungi will make an important impact on N dynamics including the plant N uptake and N leaching (Fig. 3, He et al. (2018)), distribution of soil organic and mineral N (Fig. 4, He et al. (2018)), plant GPP, NEE, soil C soil C sequestration, etc (Fig. 5, He et al. (2018)). All these previous researches show the importance of the inclusion of mycorrhizal, thus motivates our current work, our current model description paper is to present the new P model and how the new model compared to the previous models were discussed, shown by our model, at least we can conclude that considering the P cycle and mycorrhiza fungi explicitly has aclear impact on the forest growth, and soil C sequestration (e.g. Fig 5). Our previous model estimates of the soil C sequestration in Swedish forests thus might be biased due to ignore the impact of P. When compare to* *our more detailed fungi model the litterfall is higher, and also the plant P uptake was higher than the previous model estimates (Table 4).*

- *The previous model calibration study (He et al 2018) also compare the non-organic uptake approach to, the explicit and implicit approach of representing fungi. The data clearly reject the non-organic uptake- approach and show the importance of organic N uptake (Fig 2, He et al 2018). Those rejections are based on the fact that conventional assumptions of mineral N uptake to plant roots cannot satisfy the demands from the trees. Our new model provides one complimentary description of regional uptake patterns of nutrient consistent with available biomass data. However, a detailed evaluation and compare to the other non-explicit approaches is out of the scope of current work, with the overall aim to present the new P model. Instead, we strongly recommend including such efforts in new research.*

Detailed comments:

BeforeLine65-70, CMIP6 model results are openly available now. One model (probably the only one) that has land P component is from CSIRO, Australia. The name of the earth system model is ACCESS and land component is CABLECNP.

- *Thanks for the information, we will update CMIP6 in the revised paper*

Lines70-75, whether CNP models from Goll et al., 2012; Wang et al., 2010; Yang et al., 2014 are simplified are context dependent. As far as I know, these models incorporated key processes in C, N, P, water and energy dynamics and take into account coupling and interactions across spatial-temporal scales. They are not necessarily simpler than the model presented here.

- *Agreed. "simple" here refer to the P uptake pathways, i.e. none of the global models explicitly consider the fungi. We will revise the texts to make it clear.*

Line 75-80. Models in Medlyn et al 2016 are not earth system models per se. They are process-based vegetation models. ESMs have coupled land, atmosphere, ocean etc. Some models might be used as the land component of some ESMs. Some models may not be directly coupled.

- *Agreed, we will clarify this in the revision.*

Line 80-85. Low eco2 response do not imply "In other words, the vegetation is rather inflexible to increase P uptake". There are many factors come into play. Without CNP, the models have difficulties in capturing nutrient limitation on CO2 response. In nutrient limited locations, nutrient limitation is likely to reduce eco2 responses. And it is not only about the uptake capability. It is also related to nutrient availability.

- *We agree there are several explanations for the low eCO2 response, but here we would like to highlight the potential role of P in regulating the NPP in the models. In other words, the vegetation is rather inflexible to increase P uptake is refer to "The P cycle is assumed to be relatively closed". We believe the missing the linkage between plants-mycorrhiza fungi reduce the uptake flexibility. We will reformulate this paragraph to make this clear.*

Line 140-150, "The main model structure is a one-dimensional, vertical layered soil profile including plants." This sentence is confusing. How vertical soil profile could include plants ?

- *We will rephrase this sentence in the revised paper.*

Line 150-155, the concept of "big leaf" model assumes canopy carbon fluxes have the same relative responses to the environment as any single unshaded leaf in the upper canopy. You have two layers, trees and understory. Normally when people talk about "big leaf" model, it does not simulate light competition between up- vs. understory plants.

- *Multi-big leaves model concept was used. We will reword this.*

Line 170-171, the naming convention is quite confusing. By common definition, inorganic P is part of soil mineral P.

- *Agreed. We will revise the naming in our revision thoroughly. We will revise the definition as "Soil mineral phosphorus can be divided into labile inorganic P (Pi, phosphate ions, e.g., H3PO4, H2PO4-, HPO42-, PO43-) and **soil solid mineral**, (Pm)". We will clarify that we define soil mineral P as solid P containing primary and secondary mineral P (and occluded P) see L173-174.*

Line 180. The description of different P pools is rather confusing. If "soil mineral P is the total soil P without organic Po and labile P", how could you estimate it with total P content and bulk density. When we measure bulk density, we do not exclude the contribution from the organic matter.

- *We thank the referee to comments, the soil solid mineral P is a conceptual pool, the total P content and dry bulk density gives the total P in the soil layer, then subtract organic P and labile P (including soluble Pisol, that is phosphates)gives the soil solid mineral P, a lumped pool of solid primary and secondary mineral P like. We will revise the naming in our revision to clarify that we mean solid P with mineral P.*

Line 180-185. What do you mean by "fresh plant residues"? If plant residue that stays above soil, but it is not fresh (e.g., it is from the last year), do you exclude it from litter?

- *No, conceptually the pool can include litter from this year and before. We will revise the definition of litter and make it clearer, thanks for point this out.*

Line 180-185, "In CoupModel, soil litter could be further divided into two litter pools: one which contains readily decomposing materials (e.g., plant leaves and fine roots) and another for decomposition-resistant litter (e.g., stems and coarse roots)". If you do not represent these in your model, please skip these texts to reduce confusion.

- *Agreed. We will remove this to avoid confusion.*

Line 190-195. Do you take into account the hysteresis in P adsorption/desorption?

- *No, we assume instant equilibrium between the labile and soluble P. The adsorption/desorption of soluble P within one-time step (daily) was assumed excluding eventual hysteresis of the soluble P and labile P in the soil water. Similar set up as other models, e.g. Wang et al 2010.*

Line 170-205, you talked about litter pool, how do you treat soil organic matter/P pool? Do you only have humus pool? If so, non-symbiotic soil microbes are classified as litter in your model?

- *The soil organic matter is conceptually divided into litter and humus pools, non-symbiotic soil microbes are implicitly classified as litter in current model, this was made to follow Svensson et al. (2008) and He et al. (2018). We will add this information of the soil organic matter pools in the revised paper.*

Lines 210-215, "During certain seasons, plants can also capture mobile P (as well as mobile N) to prepare for rapid growth in the spring". What do you mean here? You mean plants take up more P in other seasons other than Spring, store it and use it in Spring? How does it occur? What do you mean by mobile P(N)?

- *We realize this description is unclear for both referees asked about the mobile pools and thus we will carefully reword this in the revise paper to make it clear.*
  The plant mobile N and P are state variables designed to mimic the nutrient reallocation or retranslocation process, a process by which nutrients (here N and P) were mobilized from senescing structure to developing tissues e.g. before litterfall, thus acting as an important mechanism to reduce dependence on nutrient uptake and increase nutrient recycle. Aerts, (1996) showed mean nutrient resorption efficiency for perennial plant was c.a. 50% for N and slightly higher, 52% for P. A more recent global synthesis of most measured data for woody plants showed similar results that mean N resorption efficiency of 48.4% and mean P resorption efficiency was 53.3% (Yan et al. 2018). Nieminen and Helmisaari (1996) show a 67-88% mobile nutrient N and P decreasing during needle senescence for Scots pine in Finland. These evidence serves the rational of including the nutrient resorption process in the model. In Coup-CNP, P was a similar concept to that of N, this process simulates the resorption of nutrients before litterfall (N and P were assumed to be stored internally in the mobile state variable and these can then be used next year to develop new shoots/leaves). We will describe this more in detail and add this into sensitivity test to evaluate how sensitive the model outs to this.

Lines 220-225, I don't understand what do you mean by "In Coup-CNP, biochemical mineralization is defined as organic uptake". Biochemical mineralization and organic uptake are different processes.

- *We agree with the referee that theoretically these processes are different and will rephrase it in the revision.*

Line 316, "wais" to "was"

- *Agreed.*

Line 535 – 540 and Figure 2. From Figure 2, the model systematically over-estimate Leaf C/P and leaf C/N ratio (except one site). Is it because an over-estimation of the leaf biomass? If there are coherent bias for all or most sites, it is not a neglectable issue. Figure 4. Why do you plot plant growth in C flux but change in plant for P flux, please be coherent and consistent. Table 6, systematically underestimation of P leaching

- *We thank for the comments regarding the model performance. There is generally a lack of P data currently, the model configuration was designed and benchmarked to a regional representation, where the forest biomass was a direct result from the regional survey, thus represent the regional characteristic however; the leaf C/P ratio data are data from some few representative sites within the region (two sites for each region, max three sites for the southmost region) and the measured P concentrations in the streams also include other P source from the whole watershed thus discrepancy could also be attribute to these. When evaluating the model performance, these should be bear in mind. We will revise the descriptions to make this clear.*
- *However, to further investigate the model performance, we had employed the Monte Carlo based sensitivity analysis   to further analyze the model with possibly investigate how the model behavior vary with varying parameters. The plant growth in Fig. 4 represents the net ecosystem production, which is an additional flux from the atmosphere compared to N and P. Change in plant is the difference between the pool at start of simulation (when forest was 10 years old) and at end of simulation in the next generation forest at age 10 years. We will describe this more clearly in the revision, please also see response above for model performance.*

Reply to editor comments

We thank editor for your positive comments and constructive suggestions to our manuscript. Here are our responses to the comments; The editor comments are in normal font and our response in italics.

Thanks for preparing a revised version of the manuscript addressing my previous comments. I will accept now the manuscript for publication in the discussion forum and formally start the peer review process. However, your answer to my question on the type of dynamic update, with your respective answer about coupled partial differential equations, suggests that your presentation of equations in the text is not adequate, and that you would have to rewrite many of the equations to make explicit the use of partial differential equations. You also would have to state more explicitly the boundary conditions and the initial conditions since these are factors that strongly influence the solution of the system of equations.
I accept the current version for the review process, but keep this comment in mind when preparing a revised version addressing reviewers' comments.

- *Thanks for the comments. We will rewrite our equations to differential forms accordingly. We will also make the initial conditions and boundary conditions more explicit in the text. The water and heat boundary and initial conditions were kept the same with the previous publications, i.e. Svensson et al 2008 and He et al 2018. We will explicitly descript the initial conditions of P in addition to current Section 4.2 and also Table 1.*

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