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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 #2

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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,

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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.

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.

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.

Detailed comments: Before Line 65-70, 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 CABLE-CNP.

Lines 70-75, 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.

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.

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.

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 ?

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.

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

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.

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?

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.

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

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?

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)?

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.

Line 316, “wais” to “was”

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

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