

Geosci. Model Dev. Discuss., referee comment RC1  
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## Comment on gmd-2022-191

Anonymous Referee #1

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Referee comment on "Modelling the terrestrial nitrogen and phosphorus cycle in the UVic ESCM" by Makcim L. De Sisto et al., Geosci. Model Dev. Discuss.,  
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This manuscript describes new nitrogen (N) and phosphorus (P) cycling processes that have been added to a new version of the UVic ESCM intermediate complexity Earth system model. The new processes are well justified, clearly described and well supported by citations to relevant literature, data syntheses, and theory. The new model functions are evaluated using global datasets of carbon, nitrogen, and phosphorus pools and/or fluxes and the level of agreement and areas for improvement in the model are described clearly and fairly. Overall, this is a well-written model description paper.

There are some areas where the clarity of the manuscript could be improved, primarily related to the equations and figures.

While the Methods section does include several relevant equations for N and P cycling, it omits some important processes and overall does not provide a complete picture of N and P cycling in the model. Importantly, equations and explanations are not provided for the variable tissue C:N and C:P ratios that are an important part of the stoichiometric limitation component of the model. I would advise including those equations in the main text since they are a key part of the model functionality and results. I would also suggest adding an appendix with the complete set of equations related to N and P cycling so readers do not need to search through other previous papers to gain a complete picture of how nutrient cycling in the model works.

The figures are generally informative, but there were some parts of the text that described model-data comparisons and other patterns that were not directly shown in the figures. In addition, I think it would improve the readability of the paper if the figures showing N results matched the figures showing P results in their content. Currently, the N figures and P figures show different comparisons in some cases which makes it less straightforward for readers to evaluate those parts of the model. For example, Figure 9 shows maps of modeled total soil P compared with measured total soil P and the difference between them. In contrast, Figure 7 shows maps of modeled N but does not show any direct

comparison with measured patterns of N, even though model-data comparisons of global N patterns are discussed in the text.

Finally, the paper describes the P cycle in a way that takes fertilization (which is not included in these simulations) into account, but there is no discussion of the role of anthropogenic N fertilization and whether it could introduce bias in model-data comparisons of the N cycle.

Specific comments:

Line 14: It would be helpful to include the GPP for carbon-only simulations in this comparison as well.

Line 59-61: This list was hard to follow and could use some editing.

Line 69-75: The introduction discusses Earth system models in general, and then the history of UVic ESCM in particular). I think it would be helpful to include a few sentences about intermediate complexity ESMs as a class to provide some more context about the goals of the type of model that UVic ESCM represents and how it compares to other similar models.

Line 105: Variable C:N ratios for leaf and root pools are mentioned but the details (and equations) of what determines the actual C:N ratio are not provided. It would also help to explain here how the variable C:N ratios affect other parts of the model (e.g., photosynthesis or root function dependence on tissue N). It would help to provide some summary of how the relevant processes from the Gerber et al citation are calculated, ideally with equations provided in an appendix.

Equations 1-2: An explanation should be provided here for what "av" means in the mineral nitrogen pools. Later in the paper I found that this means "available" but that should be explained here, along with an explanation of how the available fraction is calculated. Is there an unavailable fraction?

Line 122-123: I found "depth of soil layer" and "root depth" confusing. Is the soil layer referring to each individual model layer, or to the depth of the entire soil? Is the root depth a rooting depth parameter for each PFT, or the depth at which the root fraction is being calculated?

Line 129: Provide an explanation or reference for the statement that "It takes 1 mol of  $\text{NO}_3$  to mineralize 1 mol of organic C."

Line 132-133: The temperature and moisture functions are not provided or explained. Is a moisture function necessary when the anaerobic respiration is calculated only for the saturated fraction of the layer?

Table 1: Descriptions should include "pool" or "rate" or similar for each line since the table contains a mix of different types. Also, temperature and moisture functions are functions rather than numbers or outputs and feel out of place in this table.

Table 2: Is  $D_{SL}$  the same as D in Equation 3? Make sure the notation is consistent.

Equation 8: Should the  $P_{imm}$  term be negative in this equation? Immobilized P would be subtracted from the inorganic P pool. Also, shouldn't equation 9 by included as a negative term in Equation 8 since sorption reduces the inorganic P pool? These equations don't seem to be mass balanced with respect to each other.

Line 186: Is  $Q_D$  here the same as  $q$  in equation 6? Both are described as runoff.

Line 189-190: Similar to C:N, the variable C:P ratio of leaf and root tissues is mentioned here but there is no explanation or equation for what controls the value of the ratio.

Line 195: the vegetation **P** change over time

Line 200-204: Equation 16 needs some conceptual explanation. It's not directly clear from the equation and description what process this is representing. Are the nitrogen costs related to actual nitrogen availability?

Line 222-224: Equations and a more detailed explanation should be included for stoichiometric limitation, since this is a critical part of how the model works and is key to understanding the results.

Line 232: I did not find an explanation of  $CP_{leafmax}$ ,  $CN_{leafmax}$ ,  $R_{leafp}$ , or  $R_{leafN}$  in the text or equations showing how the model depends on these parameters. If these parameters are important enough to be the basis for the sensitivity analysis, they should be clearly

explained in the text.

Line 259-261: This is not shown in any of the figures. This statement could be supported by showing a map of biomass from the different simulations and the difference from the C-only simulation.

Line 274: Difference in tropical vegetation biomass is also not shown in any figure. This could be shown as a map or an average biomass value by latitude for different simulations.

Line 294: I would reorder the figures so they appear in the order described – 6 is described before 4 and 5.

Line 304-306: This statement should be supported by a figure showing vegetation carbon as a map or latitudinal gradient.

Line 314: Figure 7 does not show the difference in N compared to Wania et al 2012, so this statement cannot be evaluated.

Figure 4: This figure was difficult to interpret because only the differences in PFT fractions were shown, and not the actual fractions. There also is not much explanation of how relative PFT distributions relate to N and P cycling in the model so it's not clear how relevant this is to the main model developments being described.

Figure 5: There is no explanation of how these correlations are calculated. Is this based on relative amount of all PFTs in each grid cell? This does not seem to be the most useful test of the model since many grid cells are dominated by one or two PFTs. Wouldn't variation in PFTs across grid cells be a more useful metric to test?

Lines 326-328: There is an order of magnitude range in the different estimates, so they don't seem like a very strong constraint on the model. Is there any expectation of which set of estimates might be more accurate?

Line 329: Is CN ratio referring to soil, vegetation, or whole ecosystem? Figure 7 is also unclear about this.

Line 336: Equation 16 included some nitrogen cost of phosphatase parameters. Does this not connect the N and P cycles in a way that could allow co-limitation? It's hard to tell without more explanation of that equation.

Line 341: The model does not include anthropogenic N inputs, so is it reasonable to compare it with estimates that do include anthropogenic inputs? Couldn't this indicate that the model overestimates natural sources since anthropogenic N inputs in reality are very high?

Line 372-374: Global terrestrial P should be included as a line in Table 6. Table 6 does not indicate estimates from terrestrial P models (or at least does not indicate which estimates are from models versus measurement syntheses). What is the evidence that other models are underestimating P in subsoils and not that this model overestimates P in subsoils?

Figure 10: This figure doesn't make much sense to me. Why would the model N:P leaf ratio be perfectly linear with respect to latitude? Is the Reich and Oleksyn relationship a simple linear function with respect to latitude? If so, this seems like a very simplistic test of a complex model. Also, it is difficult to interpret this figure because there was no explanation provided for what controls leaf N:P ratio in the model.

Table A1: There is no reason this short table should be in a separate appendix. It's an important part of the model and should be in the main text.