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Biogeosciences Discussions

Interactive comment on "A global model of carbon, nitrogen and phosphorus cycles for the terrestrial biosphere" *by* Y. P. Wang et al.

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Reply to reviewer 1

We appreciate the constructive comments on our manuscript by all three reviewers, and will make some significant changes to the structure of this manuscript. The major proposed changes are:

- We will state clearly the objectives of this study (introduction) and the approach we used. We will clarify the use of the NPP dataset from Randerson et al. (1997) in this study.
- We will remove the text and equations in the manuscript associated with nutrient

uptake limitation (see Model Description).

- We will revise section 3.3 (datasets) and clarify which datasets were used as model inputs, model calibration and model evaluation.
- We will combine "Results" and "Discussions" into one section (Section 5). We will divide section 5 into three subsections: model calibration (5.1), steady-state pool sizes and fluxes for 1990's (5.2) and nutrient limitation (5.3). We will add a new figure showing the uncertainty of nutrient limitation in Section 5.3.
- A new section (section 6) will be added to state the significance and major limitations of the present study.
- We will add a number of new references on which our estimates of some model parameters are based and explanation about our N and P submodels in Appendices C and D.
- We will remove the Figure comparing the modelled and measured surface [CO2] and the associated text in the manuscript.

We believe these changes will address the major concerns by three reviewers.

For clarity we have reproduced (in italics) and number each issue raised by the reviewer. In bold we note where we anticipate making any changes to the revised manuscript.

(A1) *Overall this is a well-written article and a potentially important contribution to the coupled C-N-P modeling literature. Having said that, I have to admit that I am not entirely comfortable with the seemingly free use of arbitrary relationships to represent the coupling between C, N and P cycles. Sure, currently there are many knowledge gaps in how the three fundamental cycles are coupled together, but the authors could*

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have helped their case by presenting their rationales for the quantitative relationships used in their model.

We will clarify this in the revised manuscript. The C model is based on CASA' (Randerson et al. 1996, Fung et al. 2005) and the N and P cycle model on Wang et al. (2007) and Houlton et al. (2008). Equations that are not from those two models will be explained in the text with source references cited where appropriate (**Section 1, Appendices B, C and D**).

(A2) *For example, what are the bases for the formulations of the N-limiting and P-limiting factors to NPP?*

The N-limiting and P-limiting factors are based on Wang et al. (2007), and a similar formulation was also used in other models for N, such as McMurtrie (1991) (**Section 2.1**).

(A3) *Are these factors needed because NPP is not simulated? What exactly is the role of the NPP of Randerson et al. (1997) in this study?*

Our model can be run either coupled to a land-surface model which would simulate the NPP, or with prescribed NPP as has been done here. In this study, we use the spatially explicit estimates of the monthly CASA NPP to run the model to steady state, and compute the biome-specific nutrition limitation factor (xnp), and then vary the biome-specific parameter, xnpmax, to match the CASA NPP for each biome. We then used the estimated xnpmax to calculate NPP using eqn (1) for each grid cell, and run the model to steady state to compute pool sizes, fluxes, nutrition limitation and its uncertainty at steady state in the 1990's In this study, we address the question of what the nutrient limiting factors and C, N, and P pool sizes and fluxes should be if we assume the NPP is known, along with some nutrient constraints and the relationships among the three cycles as described in our model. We will state more clearly in the revised introduction about the approach and general philosophy of this study (**Section 1**) and how the NPP is used (**Section 2.4**). Future work will use the coupled model with sim-

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ulated NPP (**Section 6**).

(A4) *Why do you need both the leaf nutrient concentration limitation and the soil uptake limitation? What are the bases for the formulations of the plant N and P uptake?*

We will remove the soil uptake limitation in the text. At steady state, the soil uptake limitation is always 1 (**Section 2.1**). Therefore excluding soil uptake limitation will not affect our results at all, this is also confirmed by our model simulations.

(A5) *Here the formulations that need to be explained (defended) are listed: (1)-(5), C7-C9, C12, D6-D9, D11, D12.*

Agree. They will be explained in the revised manuscript (**Appendices C and D**).

(A6) *Because the N and P limitation factors are defined arbitrarily, one wonders how reliable the predicted global distribution of N and P limitation on terrestrial productivity is. If these limitation factors are formulated differently, one might come to different global distributions.*

To address the first part of the comment, we will include the uncertainty of the nutrient limiting factors and regions of N-limitation, P-limitation and NP co-limitation in the revised manuscript (**new Figure 10 and Section 5.3**). To address the second part of the comment, we would have to repeat all the calibration and simulations for any alternative functions. We agree that this is an interesting research question for future studies. The Michaelis-Menten function we used in this study has been used in many previous studies, for example, McMurtrie (1991), Melillo et al. (1993). The estimated nutrient limiting factors agree quite well with results by LeBauer and Treseder (2009), which gives us some confidence in our results and the function we chose.

(A7) *Sources or rationales of model parameters used in this paper also need to be given (e.g. Table 2).*

Additional references for the parameter values in Table 2 will be added in the revised manuscript (**Tables 1 and 2 captions, and Sections 3.2 and 5.1**).

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(A8) *The presentation of the equations could be improved. The constraints for the various transfer coefficients should be given together with the first-order differential equations (e.g. the fractions of allocation should add to 1). In some equations, the condition* $k \neq kk$ *may be misplaced. So check carefully. By the way, in most places* $k \neq kk$ *should have been written as* $kk \neq k$ *since k can be any pool while kk cannot be the pool k.*

Agreed. Constraints on the transfer coefficients will be included and the inequality will be revised as suggested by the reviewer (**B3 and associated text**).

(A9) *All the simulated budget numbers are for steady state and for the 1990 NPP. Therefore they should not be presented as if they are estimated for the present terrestrial biosphere.*

Agree, and we will clarify this point in the revised manuscript (**Section 5.2**).

(A10) *Have the solutions of the coupled C-N-P systems been tested for mass conservation? This is a good way to checking the mutual consistencies of the equations.*

Yes, we checked the mass balance right from the beginning of model development and found that mass balance was always achieved (**Section 4**).

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