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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 2

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

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- 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 would locate proposed changes to the revised (re-structured) manuscript.

(B1) *Unfortunately, the manuscript does not provide a very clear account of the model, specifically failing to provide justification for important model equations and parameters, and provides only limited evidence relevant for evaluating the model performance. The model run in an equilibrium mode and calibrated to reproduce the present-day carbon cycle, with critical parameters that determine the spatial patterns of phosphorous vs. nitrogen limitation chosen rather arbitrarily. It is questionable to my mind whether*

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this is a useful approach, given that the objective of this model is to integrate P dynamics into global prognostic modelling, and it does not become clear how this would be achievable, as the model relies on the monthly NPP by grid-point to be known.

The present model as described here is not prognostic because NPP is prescribed. When the present model is coupled to a global land surface model that calculates NPP and accounts for nutrient limitation on NPP, then the combined model can be used to predict what NPP should be in future studies. The objective of this study is to derive the nutrient limiting factors using our model with a number of nutrient constraints for a given NPP under steady state conditions for 1990's. This point will be clarified in the revised manuscript (**Section 1 and section 2.4** , also see our reply to reviewer 1). Calibration of key parameters for the nutrient cycles, and the uncertainty of the estimated nutrient limiting factors will also be included to address the reviewer's concern (**Sections 5.1 and 5.3, Figure 10**). We will discuss in the revised manuscript how this work will be implemented into a global land surface model that calculates potential NPP and use the nutrient limitation factor from CASACNP to calculate actual NPP for prognostic analysis or predictions in the future (**Section 6**).

(B2) The authors need to improve their manuscript with respect to the presentation of the model and its limitations and (given the high degree of calibration/arbitrary parameter choices) uncertainties. Since this is one of the first paper aiming at modelling spatially explicitly the global P cycle coupled to a CN model, I found the introduction surprisingly short with respect to the discussion of the problems which have hampered the development of such a model so far, and in particular the key novel ideas by the authors to overcome these problems.

Agreed. We will include new paragraphs in the Introduction of the revised manuscript to discuss why we need to include P limitation and the general approach we used in this study (**Section 1**). Limitations of the present model will be discussed in the revised manuscript (**Section 6**).

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(B3) *Major comments: 1) The model description is rather poorly organised in both main text (see minor comments) and Appendix (where series of equations are given with very little explanation or justification). What is the justification for choosing the nutrient limitation function on productivity as two Michealis-Menten type relationship linking leaf concentrations and nutrient uptake in a multiplicative manner (eq 1)? Nutrient uptake/availability is implicitly taken account in assessing the nutrient limitation of productivity via varying foliar nutrient concentrations, and it is not clear, why there is a need for the second term downscaling productivity with nutrient uptake, nor what the physiological basis of such a limitation would be. This function is furthermore surprising as this paper only evaluates equilibrium patterns, in which the uptake limitation does not exist (p9893 l11ff), implying that the second factor is not used in this study, however, it will significantly influence any future application of the model.*

This point was also raised by the other reviewers. We will revise the description of the model in the main text (**Section 2.1**) and appendices (**Appendices B, C and D**). Justification for using Michaelis-Menten function for nutrient limitation is based on previous studies (McMurtrie 1991), and will be made clearer in the revised manuscript with new references (**Section 2.1**). We will add more explanations in the appendices explaining the equations taken from previous work (Wang et al. 2007, Randerson et al. 1996) and the new equations developed for this study (**Appendices C and D**). The second limiting factor (uptake limitation) will be removed from the text and the model (**Section 2.1**). We only consider pools and fluxes at steady state in this study, our results are not affected by removing uptake limitation, as correctly pointed out by the reviewer.

(B4) *The model relies heavily on calibrated/arbitrarily assigned values. This is not necessarily a problem, but the way these parameters have been assigned, what the uncertainty of these parameter values is and how this uncertainty propagates into uncertainty in key model outcomes is not made clear. This is particular true for the following: Two sets of parameters control the relative importance of N vs P limitation: the half saturation constants of foliar concentrations (eq. 2.3), which have been arbitrarily set*

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to achieve a shift from N to P limitation at a N:P of 16 (why? see Townsend et al. 2007 for a critical review on the usefulness of N:P ratios in determining nutrient limitation), and P liberating weathering rates which are assigned to (based on which criteria?) to soil types. It so happens that weathering rates in the P poor soils in the tropics are an order of magnitude lower than in colder ecosystems (why? No discussion of observed rates of P liberation from weathering is given; in particular no justification of a 10 fold difference according to soil order). Together these parameters drive the key model outcome, i.e. the decline of the N:P ratio polewards. One may argue that none of these parameters are truly observable, and hence need to be assigned at “reasonable” values, however, I would have expected an uncertainty analysis of these critical parameters to assess the robustness of the key model outcomes as a minimum requirement. This is particularly important as the authors claim that their strategy applying a globally calibrated model (without the parameters obeying to physiological reasoning or observations) would be preferable to the approach taken by previous model studies that inferred parts of the model’s credibility by comparing/calibrating it to field studies, in which key characteristics had been observed.

Transition from N limitation to P limitation at a leaf N:P ratio of 16 is based on the study by Koerselman and Meeuwsen (1996), and this is meant to represent the broad variations of N and P limitations across the globe in our study. We agree that many other factors can also affect which nutrient (N or P) is more limiting for any particular forests, as shown by Townsend et al. (2007) for tropical forests. Leaf N:P in our model is simulated (not prescribed) as a function of the amount of nutrient available, and demand by plant growth, therefore some of the significant factors affecting nutrient limitation, such as soil order, is considered in our model. Other factors, such as species diversity are not considered in our model, as we do not represent species diversity in our simple model. The half-saturation parameter for N-limitation is based on the work of Linder and Rook (1984) (see Section 2.1). P weathering rate is based on Guidry and Mackenzie (2003) and Chadwick et al. (1999) (**Table 2**). Chadwick et al. (1999) showed that P weathering rate can vary by several order of magnitudes with soil age.

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These references will be added to the text and Table 2 in the revised manuscript.

We will delete the sentence "Calibrating the model using data from the selected sites may lead to improved model parameter values or model structure for that location, but the values of model parameters may not be representative of a given biome globally." to address the reviewer's concern (**Section 3.2**).

(B5) A significant proportion of the manuscript deals with assessing the simulated C cycle (e.g. P 9904 I23- P9907 I 5 discuss in detail C stocks in equilibrium), which is in so far irrelevant for this paper because NPP has been tuned to plausible values, and C allocation coefficients from Fung et al. 2005 have been used. That is to say that these results are not affected by the new model components and, in particular, do not constrain the behaviour of the coupled CNP coupled model. The same is true for the comparison to atm. CO2 measurement (Section 5.2): In equilibrium, regardless of N or P cycling, mean annual heterotrophic respiration will be equal to mean annual NPP. The authors find it important to adjust the temperature sensitivity to match the seasonal cycle to a Q10 of 1.7, however, this does not appear to be a consequence of nutrient dynamics altering the seasonal cycle. Randerson et al. 1997 from which the NPP used in this study already used a quite similar Q10 value, so I wonder what the information gained from this comparison is in terms of the evaluation of the coupled CNP model behaviour? As a minor point, I do not believe that it is instructive to look at the peak-to-peak amplitude without at the same time controlling for the phasing and shape of the seasonal cycle, which the authors fail to do.

We will delete the comparison with atmospheric [CO2] measurements, and also clarify the differences in C cycle between CASACNP and CASA' in our revised manuscript (Section 5.2). Some of the comparison for C cycle, such as litter fall flux and CWD pools have not been compared with simulations by CASA or CASA' before, so they are new results to further confirm the C cycle component.

(B6) The authors claim that biomineralisation of P was important to get the fractional

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distribution of P in soil correctly represented. This is true in their model, but what's the observational basis for this, and why do the authors include this process for P, but ignore it for N, which is known to be very important in boreal ecosystems?

The observational basis for the importance of biochemical P mineralization has been summarized by McGill and Cole (1981). This reference will be cited again where appropriate in the revised manuscript (**Section 2.3**). For the second part of the question, we think that the reviewer refers to uptake of N-containing monomers. That is different from biochemical mineralization of P. As an approximation, that uptake of organic N by plants can be included as part of mineralization, that is to say that those N-containing monomers will be further mineralized into mineral N if not being taken up by plants or microbes, because they are preferred to more complex organic N in the substrate by soil microbes. Biochemically mineralized P will be quickly absorbed in soil if they are not taken up, that is why the fraction of organic P is only about 5 to 35%, whereas over 90% of soil N is organic.

(B7) What is the justification for assuming a different value of $\lambda_{p,up}$ for each biome (but not for $\lambda_{p,tase}$), and how were these values obtained? Note that these values have been omitted from Table 1 where all biome-specific parameter are listed (why?). It is also questionable whether the comparison of Figure 6 is useful. These data have been used to calibrate the model (P9902-I18ff), so they do not provide an independent test. Note that this comparison is provided without alerting to the fact that the model has been calibrated with these data. The authors also fail to discuss other potential causes for the high organic P fraction than biomineralisation, namely the simple soil organic matter model and the assumption of fixed soil C:P ratios in soil organic matter.

Different values for $\lambda_{p,up}$ for different biomes and $\lambda_{p,tase}$ for different soil orders are based on the study by Houlton et al. (2008). We used the data of Cross and Schlesinger (1995) for calibrating the model only. Figure 5 (previously Fig 6) shows how well we can match the estimates of Cross and Schlesinger (1995) by tuning two model

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parameters, and is therefore included in the revised manuscript (**Section 5.1**). The C:P ratio of soil organic matter is only fixed for the newly formed SOM, but varies as it ages, as stated in the manuscript (**Section 2.3**). Because of biochemical P mineralization and soil P dynamics being quite different from that of C or N, a large fraction of newly formed P in SOM was transformed into inorganic P in soil, which is why the fraction of organic P in SOM is less than that of inorganic P. Therefore a simple soil organic matter model with fixed C:P ratio will not simulate different P fractions in soil correctly. This will also be made clearer in the revised manuscript (**Section 2.3**).

Minor comments: (B8) The model description (p9896) is poorly organised: eq 1 introduces $x_{n,up}$ and $y_{n,up}$, but then the text jumps to introducing $x_{p,leaf}$ (without it becoming clear what these are used for, see also major comment 1), before going back to $x_{n,up}$, and then finally introducing $x_{np,max}$, which is a pure calibration parameter, which overrides the effect of equation 1-5 in calculating NPP.

We will delete the text and equation for soil nutrient uptake limitation and state clearly that we used the observed NPP and other constraints to derive the nutrient limiting factors (**Section 1, Section 2.1**). Also see our response to B1 above.

(B9) The model description does not make clear that nutrient concentrations are flexible in this model, nor does it explain how these concentrations vary. The authors should try to explain better how changes in nutrient concentrations are controlled, and in particular, what the factors are that cause shifts in the N:P ratios.

This will be stated clearly in the revised manuscript (**Section 2.4**).

(B10) In general I find that the equations in the Appendix are not well documented, see for instance D1-12, which are left nearly without explanation, making it tedious to try and understand what the model is actually doing, and why. Given that this will be the key reference for future studies of this model, this section should be very clear in the interested of scientists trying to understand the dynamics of this model.

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We have added explanation of equations D1-D12 in Appendix D.

(B11) Not all model equations seem to deliver a mass conserving system. For example, the soil N dynamics (C6) are written such that $N_{s,min}$ can easily become negative as eq C7 (plant uptake) is not reduced to zero when $N_{s,min}$ is approaching zero (see last, constant term). Similar for P dynamics see eq. D8,D12 The text of the Appendix should make clear which of these equations have been taken from previous work; i.e. much of the P cycle appears to be from Wang et al. 2007, which does not become clear from only reading the Appendix.

This results from our careless translation of our Fortran codes into mathematical equations. Decomposition will be reduced when net N mineralization is negative and cannot be met by the available soil mineral N (plant N uptake takes place before net immobilization). This will be added in our revised manuscript (see Eqn C12 and associated text). All pools (C, N and P) in our model do not become negative in our simulations (our program will print out warning if this occurs). Mass conservation is achieved for all our simulations (**Section 4**).

We have now stated clearly most equations in Appendices C and D are taken from Wang et al. (2007) (see revised Appendices C and D).

(B12) The soil N estimate (P9911 l22ff) is consistent with recent studies N cycle modelling studies by Gerber et al. (in press) and Zaehle et al. (in press), who provide an estimate of Soil N in the order of 100 PgN, consistent with observation based estimates.

We will add these two references in the revised manuscript (**Section 5.2**).

(B13) The authors claim on P9914 l17 that previous estimates of soil P are substantially overestimated. However, this has been noted before (Smil, 2000; MacKenzie 2002 (see P9912 l7ff), based more reliable data than the estimate by Jahnke 1992. This is a good point for discussion, but I don't really see why this needs to be emphasised in

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the conclusions.

Agreed, conclusion will be revised (**Section 7**).

(B14) Table 1 contains far more parameters that could have been obtained from McGroody and Cleveland. What are the sources? Why are all values assumed to vary only below 20% of their maximum? The range of N:P and C:N for individual plant types is much larger than this (e.g. White et al. 2001; Townsend et al. 2007).

Yes, we will clarify the sources of leaf C:N ratios for different biomes by including additional references and explanation (**Table 1 caption**). The 20% variation is arbitrary, but more realistic than other models that use fixed C:N ratios. Leaf N:P ratio is estimated using nonlinear optimization, which will be discussed in the revised manuscript (**Section 5.1**).

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