

Interactive
Comment

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

Y. P. Wang et al.

yingping.wang@csiro.au

Received and published: 5 February 2010

Reply to review 3

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

C4316

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



- 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 any changes would be located in the revised (restructured) manuscript.

The authors claim that P limits NPP by about 20and broadleaf moist forests, but elsewhere NPP is primarily limited by nitrogen by up to 40%. This paper does not, however, provide a convincing case for these claims for the following reasons: (C1) While the carbon and nitrogen sub-models are reasonably well ‘constrained’ by observations (e.g. where supplementary information is used to justify model parameters) the phosphorus sub-model is virtually un-constrained by a lack of data on the key processes

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

and pool sizes. The output of the phosphorus sub-model is therefore largely driven by the dynamics of the carbon cycle and the C:P ratios as governed by the P mineralisation rate. The rate parameters were determined by estimation against Cross and Schlesinger (1995) which is a small study of 88 published papers from sites in Canada, the USA, Spain and South America. This is not a comprehensive body of observations against which definitive estimates of P pools and fluxes can be made. Similarly the evaluation datasets are comprehensive for carbon (latitudinal summations of a range of global studies), limited for nitrogen (latitudinal averages for two studies by Post et al) and virtually non-existent for phosphorus (one study by Cross and Schlesinger 1995). As a result, the claims regarding the global phosphorus budget (the most important new piece of work in the paper) provide virtually no new information than what has been generated using simpler methods 20 years ago.

We will clarify what is new in this study in the introduction, i.e. including N and P cycle into CASA' model and deriving the globally spatially explicit nutrient limiting factors and their uncertainties in the revised manuscript (**Section 1**). We agree that measurements of P pools are fewer than C and N pools. The novel aspect of this study is to derive those pools using various constraints (Post et al. 1985; Cross and Schlesinger 1995; Reich and Olksyn 2004) and our understanding of C, N P cycles as represented in our model based on previous work on nutrient limitation (Randerson et al. 1996, Wang et al. 2007, Houlton et al. 2008) (**Section 1**). Previous studies as noted by the reviewer on soil P only give estimates of total soil P globally without any spatially explicit information, and is therefore not very useful for many global studies, whereas our estimates of pools and fluxes are spatially explicit, as required for coupling to a global climate model. The output of P cycle also depends on the biochemical P mineralization, this point will be stated more clearly in the revised manuscript (**Section 5.1**).

(C2) *The steady state condition of the model is not met in 1990. With global CO₂ concentrations increasing by about 25% over pre-industrial values in the late 20th Century, the global biosphere should be responding to increased carbon inputs by entraining*

BGD

6, C4316–C4326, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



more nitrogen and phosphorus. The idea that increasing atmospheric CO₂ concentration leads to increasing root exudation, mycorrhizal infection and nitrogen fixation thereby increasing nutrient availability in ecosystems was first promoted in the late 1970's and early 1980's (see Luxmore 1981, *BioScience*, 31, 626) and there have been numerous later publications examining the processes and potential magnitude of this effect (e.g. see Lloyd et al 2001 in 'Global Biogeochemical Cycles in the Climate System' Academic Press). The correct approach would be to model the steady state in pre-industrial atmospheric CO₂ concentrations and then examine the dynamics of the pools and fluxes in the disequilibrium state in the 1990's to ascertain the magnitude of the entrainment of nitrogen and phosphorus into the global terrestrial biosphere.

The validity of our approach and its limitations will be discussed in the revised manuscript (**Section 6**). The objectives of the present study are to derive the nutrient pools, fluxes and limitations using available constraints (NPP, leaf N:P estimates, soil C, N ratio and soil P fractions) and our model. This study addresses the question: what should the nutrient limitation be for the present NPP under steady state condition? These points will be made clearer in the revised manuscript (**Section 1**). The approach suggested by the reviewer will be our next step, and will be discussed in the revised manuscript (**Section 6**). Effects of increasing CO₂ concentration on mycorrhizal infection and nitrogen fixation are not the topic of this study, and will therefore not be discussed.

(C3). *The phosphorus model is not robust. Figure 1 depicts the transfer of inorganic P to the 'sorbed P' pool but there is no equivalent for sorption of organic P. It shows a flux out of the 'strongly sorbed P' pool as a loss from the system (why is occluded P lost from the system?).*

In our Figure 1, we do not have an inorganic P pool, but a labile P pool. We do not distinguish inorganic P from organic P in labile, sorbed and strongly sorbed P pools. Occluded P is not lost to the system, but unavailable for plant uptake at time scale of decade to century, as stated in our manuscript (our assumption).

BGD

6, C4316–C4326, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



The plant uptake of P appears to occur from every pool. No, Plant takes up P only from the labile pool (eqn D12).

The appendix shows that phosphorus mineralisation is represented by a single asymptotic function modified by the sorption/desorption of P by soil minerals but there is no consideration at all in this paper of the role of mycorrhizae, root architecture and organic acid root exudates; all of which are important determinants of plant P acquisition.

We consider both biological and biochemical P mineralization (**Section 2.3**). Soil or litter P can be mineralised in two pathways: biological mineralization and biochemical mineralization through phosphatase production. The roles of mycorrhizae, root architecture are not explicitly modelled in our model.

The main text states that " the rate of sorbed P to strongly sorbed P is assumed to be proportional to the amount of sorbed P in the soil". There is no basis for this assumption as this is a function of mineralogy and soil age. The parameters K_{plab} and S_{pmax} are both determined from the Cross and Schlesinger (1995) study and then the results of the Cross and Schlesinger paper are used to evaluate the equilibrium pool sizes and fluxes in section 5.1.

Dynamics between sorbed P and strongly sorbed P is described by our equations D9 and D10, based on first-order kinetics as a simplification. This will be stated more clearly in our revised manuscript (**Appendix D**). Other more complex kinetics for sorbed P may be more realistic, but global data are not available, and we cannot justify the use of a more complex kinetics. We agree that parameters K_{plab} and S_{pmax} vary with soil order (see our Table 2).

This is in spite of the fact that the model considers the whole root zone (to an unspecified depth) whereas Cross and Schlesinger considered phosphorus fractions only in the top 15 cm of soil. The assumption that 15% of soil P is occluded was used to estimate the total amount of occluded P (4.6Gt). This assumption was that purported to come from Johnson et al (2003). Johnson et al do not consider occluded P at all

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

in their paper, rather they examine the size of the labile inorganic and organic P pools in tropical and temperate forest soils. I can only assume that this 15% assumption is based on their Figure 6 which is a conceptual diagram of the proportions of P along an axis of increasingly weathered soils. This is not a basis for assuming global occluded P is 15%.

We assumed that the fractions of different P pools (not the amount) in the rooting zone are the same as the top 15 cm, as stated in our manuscript (**Section 5.1**). The use of 15% of soil P as occluded P is a global mean, based on the study by Cross and Schlesinger, not Johnson et al. (2003), as correctly pointed out by the reviewer. We will correct this citation error in the revised manuscript (**Section 5.1**). The fraction of occluded P in soil should vary spatially, but is not explicitly modelled in our present model.

Detailed comments: C4) Abstract P9892 L13-14: That biochemical P mineralisation has not been included in other global models previously is not true. Check the literature.

This sentence will be deleted.

(C5) P9893 L24: Phosphatase production by soil microbes is one means by which organic P can be mineralise. The literature contains a large number of papers on the role of organic acids in solubilising inorganic unavailable phosphorus, mycorrhizae, plant root phosphatase production, and the impact of elevated CO₂ concentrations on these processes. This paper ignores all of this literature.

We stated that soil P can be mineralised biologically and biochemically, both pathways are represented in our model The biochemical pathway represents P mineralization through phosphatase production by plant roots and soil microbes (**Section 2.3**). However we do not distinguish the phosphatase production by roots and soil microbes. We have not considered the impact of elevated CO₂ on phosphatase production, as this is not focus of the present study. Wang et al. (2007) showed that soil phosphates production will respond to elevated CO₂.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

(C6) P9894 L8-10: *The wide variation (of total soil P) here is due in part to differences in which pools are included in the global totals.*

Actually the main difference is the assumed mean P content of the soil. We will add this information to the text (**Section 1**).

(C7) P9895 L18: *Why is CASA followed by an apostrophe?*

CASA' is a global version of CASA for coupling to global climate model. CASA is an offline global model. The differences between two version are discussed in Fung et al. (2005) and Randerson et al. (2009) (information added in Section 1).

(C8) P9896 L21: *What is the physiological basis of the 16 gN/gP threshold?*

The value is not based on our understanding of physiological processes only. It is based on an ecological study by Koerselman and Mueleman (1996) (**Section 2.1**).

(C9) P9900 L20-25: *The assertion that occluded P is not available is not true for disturbed ecosystems such as cropland where P is released from the occluded pool and is available for mineralisation. Also on long timescales (decades - centuries) P in the occluded pool is potentially available for plant uptake.*

We do not consider disturbance here. Occluded P can be potentially available for plant uptake for the disturbed sites, but the contribution is small for most undisturbed systems (**Section 6**).

(C10) P9901 L9: *CO2 concentrations should have square brackets*

Agree.

(C11) P9903 L22 - P9904 L3: *The authors state that "the above information is not sufficient to constrain most of the model parameters" and then justify not calibrating the model. This justification is not sufficient for this model to yield accurate estimates of pools and fluxes as stated above. As a result the model yields results that are not different to earlier studies based on simpler methods.*

We will delete this sentence, and include a new section on model calibration in the revised manuscript to discuss the various nutrient constraints on model parameters (**Section 3.2**). We are not clear what the simpler methods are that the reviewer refers to, therefore it is difficult for us to compare our model with those simple methods. However the strength of our approach is to provide coherent, spatially explicit estimates of pools and fluxes of all three cycles. These estimates are required for global studies, such as the interaction between climate and carbon, nutrient cycles (**Section 6**).

(C12) *P9905 L2-4: The sentence on median values is not required.*

Agree and this sentence will be deleted (**Section 5.2**).

(C13) *P9906 L21-22: "quite sensitive": How sensitive? IN what way? What about disturbance by fire?*

This sentence will be deleted.

(C14) *P9907 L1-4: to what depth are these soil C density estimates made?*

These are estimates of CWD pool sizes, not soil C density as reviewer stated.

(C15) *P9908 L12-20: This comparison between Figure 6 and Cross and Schlesinger (1995) does not match at all. Why not put the soil orders in increasing stage of weathering as was done by Cross and Schlesinger? The P fractions for different soils do not match the results presented by Cross and Schlesinger.*

We have more soil orders than Cross and Schlesinger. We have now plotted the data against soil order names in the revised figure (**now Figure 5**) .

(C16) *P9908 L25-30: The authors state that the "fraction of P in soil organic matter is usually less than one third of total P for most soils" when in fact the results of Cross and Schlesinger show that the fraction of organic P varies systematically from 5% to 35% depending on soil age.*

We will move the citation of Cross and Schlesinger (1995) to make this agreement

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



more explicit (**Section 5.1**).

(C17) *P9911 L13-14: What soil depth?*

We stated that our estimate is for the rooting zone, and depth of rooting zone varies with biome types. We used the root vertical distribution function and parameters from Jackson et al. (1996) (**Section 5.1**).

(C18) *P9911 L24-25: "Their soil N estimate is much lower: : : " How much?*

Estimate of soil N by Xu-ri and Prentice (2008) is now added (**Section 5.2**).

(C19) *P 9913 L9: weathering, fertiliser, and dust account for 56%, 42% and 2% of total P input: Where do these figures come from?*

This will be clarified (**Section 3.1**).

(C20) *P9920 L 15-23: This leaf phenology sub-model is based on deciduous forests. How does it apply to evergreen vegetation or rain fed seasonally green savannas?*

The dataset as provided by Zhang et al. (2006) includes spatially explicit information of leaf phenology for all biomes. Evergreen biome types only have phase 2 (steady state growth phase). This will be added to the revised manuscript (**Appendix B**).

(C21) *P9921 L1-6: What determines these parameters?*

Values of carbon allocation parameters are based on CASA' (Fung et al. 2005) (**Appendix B**).

(C22) *Table 1: "80% of their maximal values" on what basis?*

See our response to Reviewer 2 (B14).

(C23) *Figure 3 "medium" you mean median.*

Agreed. We will fix this (**now Figure 6**).

(C24) *Figure 6: "with phosphatase production" means the same thing as "biochemical*

BGD

6, C4316–C4326, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



P mineralisation". Use one term.

Agreed and will be made consistent in the revised manuscript.

References

Cross, A.F. and Schlesinger, W.H.: A literature review and evaluation of the Hedley fractionation: applications to the biogeochemical cycle of soil phosphorus in natural ecosystems, *Geoderma*, 64, 197-214, 1995.

Fung, I.Y., Doney, S.C., Lindsay, K. and John, J.: Evolution of carbon sinks in a changing climate, *Proc Natl Acad Sci USA*, 102, 11201–11206, doi:10.1073/pnas.0504949102, 2005.

Houlton, B.Z., Wang, Y.P. Vitousek, P.M. and Field, C.B.: A unifying framework for dinitrogen fixation in the terrestrial biosphere, *Nature*, 454, doi:10.1038/nature07028, 2008.

Jackson, R.B., Canadell, J., Ehleringer, J.R., Mooney, H.A., Sala, O.E. and Schulze, E.D.: A global analysis of root distributions for terrestrial biomes, *Oecologia*, 108, 389-411, 1996.

Johnson, A.H., Frizano, J. and Vann, D.R.: Biogeochemical implications of labile phosphorus in forest soils determined by the Hedley fractionation procedure, *Oecologia*, 135, 487-499, doi:10.1007/s00442-002-1164-5, 2003.

Koerselman, W. and Mueleman, A.F.M.: The vegetation N:P ratio: a new tool to detect the nature of nutrient limitation, *Journal of Applied Ecology*, 33, 1441-1450, 1996.

Post, W.M., Pastor, J., Zinke, P.J. and Stangenberger, A.G.: Global patterns of soil nitrogen storage, *Nature*, 317, 613-616, 1985.

Randerson, J.T., Thompson, M.V., Malmstrom, C.M., Field, C.B. and Fung, I.Y.: Substrate limitations for heterotrophs: implications for models that estimate the seasonal cycle of atmospheric CO₂, *Global Biogeochemical Cycles*, 10, 585-602, 1996.

Reich, P. B. Oleksyn, J.: Global patterns of plant leaf N and P in relation to temperature and latitude, *Proc. Natl Acad. Sci.* 101, 11001–11006, doi:10.1073/pnas.0403588101, 2004.

BGD

6, C4316–C4326, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Wang, Y.P., Houlton, B.Z. and Field, C.B.: A model of biogeochemical cycles of carbon, nitrogen and phosphorus including symbiotic nitrogen fixation and phosphatase production, *Global Biogeochemical Cycles*, 21, GB1018, doi:10.1029/2006GB002797, 2007.

Xu-Ri and Prentice, I.C.: Terrestrial nitrogen cycle simulation with a dynamic global vegetation model, *Global Change Biology*, 14, 1745-1764, 2008.

Zhang, X., Friedl, M.A. and Schaaf, C.B.: Global vegetation phenology from Moderate Resolution Imaging Spectroradiometer (MODIS): evaluation of global patterns and comparison with in situ measurements, *Journal of Geophysical Research*, 111, G04017, doi:10.1029/2006JG000217, 2006.

Interactive comment on *Biogeosciences Discuss.*, 6, 9891, 2009.

BGD

6, C4316–C4326, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

