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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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Wang and Houghton present a global equilibrium model of the coupled terrestrial carbon, nitrogen and phosphorous cycles. Most recent terrestrial biosphere models that attempt to account for nutrient dynamics and their effect on carbon cycling have limited themselves to nitrogen, such that tackling the phosphorous issue is a timely thing to do, and certainly within the scope of Biogeosciences.

Unfortunately, the manuscript does not provide a very clear account on 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

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 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.

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.

2) 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

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.

3) A significant proportion of the manuscript deals with assessing the simulated C cycle (e.g. P 9904 l23- P9907 l 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.

4) The authors claim that biomineralisation of P was important to get the fractional 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? 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-l18ff), 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.

Minor comments:

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.

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.

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.

Not all model equations seem to deliver a mass conserving system. For example, the soil N dynamics (C6) are written such that Ns.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.

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.

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 the conclusions.

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).

References: Gerber, S., L. O. Hedin, M. Oppenheimer, S. W. Pacala, and E. Shevliakova (2009), Nitrogen cycling and feedbacks in a global dynamic land model, Global Biogeochem. Cycles, doi:10.1029/2008GB003336, in press.

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Zaehle, S., A. D. Friend, P. Friedlingstein, F. Dentener, P. Peylin, and M. Schulz (2009), Carbon and nitrogen cycle dynamics in the O-CN land surface model, II: The role of the nitrogen cycle in the historical terrestrial C balance, Global Biogeochem. Cycles, doi:10.1029/2009GB003522, in press.

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