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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.

Anonymous Referee #1

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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 have helped their case by presenting their rationales for the quantitative relationships used in their model. For example, what are the bases for the formulations of the N-limiting and P-limiting factors to NPP? 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? 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?

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

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.

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

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 Ć kk' (k not equal to kk) may be misplaced. So check carefully. By the way, in most places 'k Ć kk' should have been written as 'kk Ć k' since k can be any pool while kk cannot be the pool k.

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.

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.

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